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PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

EDITED BY  
SIR JOHN Y. W. MACALISTER  
UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

---

**VOLUME THE SEVENTEENTH**  
SESSION 1923-24

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PART III

SECTIONS:—

OBSTETRICS AND GYNÆCOLOGY - ODONTOLOGY - OPHTHALMOLOGY -  
ORTHOPÆDICS - OTOLOGY - PATHOLOGY - PSYCHIATRY -  
SURGERY, WITH SUB-SECTION: PROCTOLOGY -  
THERAPEUTICS AND PHARMACOLOGY - TROPICAL DISEASES AND PARASITOLOGY -  
UROLOGY - WAR -

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LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1924





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SECTION OF OBSTETRICS AND GYNÆCOLOGY



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LONGMANS, GREEN & CO., PATERNOSTER ROW  
1924



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## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### A Case of Adenomyoma in a Mal-developed Uterus.

By H. E. EVERARD WILLIAMS, M.D.

PATIENT, aged 31, a typist, was admitted to Charing Cross Hospital under Dr. Lockyer's care on January 6, 1922, on account of amenorrhœa and severe abdominal pain.

*History:* Patient has never menstruated, but has attacks of severe abdominal pain each month.

In character the pain is a colic and intermittent; it is very intense, the duration is from three to four days, and patient states that it is getting worse. The pain is not severe enough to make her faint or vomit, but she complains of severe headache at these times.

There is no obstetric history, and in the past general history there is nothing of note except that she was examined under an anæsthetic at the Waterloo Hospital ten years ago and informed that nothing could be done for her condition.

*Examination under Anæsthesia*, January 11, 1922: The vagina practically absent, and represented by a small depression situated a little to the left of the mid-line. At the apex of the depression a small solid ovoid body can be felt. Beneath the urethra is a membrane resembling the hymen, with a small central orifice, through which the probe can be passed for a distance of  $\frac{1}{2}$  in. Posteriorly the membrane is distinctly thinner than elsewhere.

*Operation*, January 14, 1922: The abdomen was opened in the usual manner. Examination of the pelvis showed an ill-developed uterus with two separate horns. The left horn was attached to the sigmoid and the back of the left broad ligament by dense adhesions; the right side was clear. The cervix uteri and both uterine horns and ovaries were removed. The abdomen was closed in the usual manner after a portion of the right ovary had been grafted between the folds of the right broad ligament.

*After-history*, September 19, 1923: Patient feels very well; has had no pain since operation.

#### DESCRIPTION OF THE SPECIMEN.

The specimen shows a uterus bicornis unicollis.

The portion of cervix seen is small because the amputation was performed through the cervix (so that the greater part was retained), and, in addition, a portion was removed from the specimen for microscopic section before mounting.



## 2 Williams: *Adenomyoma*; Gilliatt: *Placenta Prævia*

Extending from the nodule of cervix, the two uterine cornua are seen, each measuring approximately 6 cm. in length. At the extremities of the uterine cornua and extending as far as the free borders of the broad ligaments are seen the Fallopian tubes, apparently normal in character. At the site corresponding to the cornual angles are two spherical tumours. That on the right side is the smaller and it measures 2 cm. in diameter. The surface is smooth and free from adhesions. On section this tumour was found to be solid. The tumour at the left cornual angle measures 4 cm. in diameter. It differs from the tumour on the opposite side in the fact that the surface is shaggy, showing that many adhesions were divided in its removal. On section this tumour also was found to be solid. Many adhesions are seen on the back of the left broad ligament. On the right side behind the broad ligament is the cortex of the ovary, free from cysts and macroscopically normal. On the left side the ovary is seen in the usual situation; there are a few adhesions on its surface at the outer pole, but otherwise it appears normal.

Sections were prepared from both tumours, from the cervix, and from the left ovary. They were stained by hæmatoxylin, eosin and Van Gieson's stains. The sections through the cornual tumours have similar appearances; they show glands morphologically resembling those of the endometrium, surrounded by a highly cellular stroma. Some of the glands are dilated, some contain goblet-cells, and many have blood-cells in their lumina. This endometrial tissue is extensively infiltrating the surrounding muscle, and this infiltration appears between individual muscle fibres and is not confined to the cellular planes between the muscle bundles.

The sections through the other tissues show nothing of pathological note. There is an absence of any inflammatory reaction throughout the sections.

*Addendum.*—My thanks are due to Dr. Lockyer (President) for permission to publish the case.—H. E. E. W.

## Placenta Prævia in Four Successive Pregnancies.

By W. GILLIATT, M.S.

PATIENT, aged 32, first seen at the request of her doctor, on April 6, 1919, at 4 a.m., on account of severe antepartum hæmorrhage. She had been pregnant for seven months, and this was her second pregnancy. She had been bleeding for twenty-four hours, and during the night (April 5-6) had continued to lose a rather alarming amount. I was told by telephone that she was having weak pains at fifteen-minute intervals, that the cervix did not admit one finger, and that the patient's pulse-rate was 104. I advised the doctor to plug the vagina. On arrival an hour later I found the patient having strong pains every five minutes, and her pulse-rate was 92. On examination of the abdomen the uterus was found to correspond in size with that of a twenty-eight weeks' pregnancy. The child was presenting by the breech, and the foetal heart could be heard distinctly. The patient was anæsthetized, the plugging removed, and a central placenta prævia discovered. The cervix by this time was nearly half dilated. A hole was made in the placenta and a leg of a child was brought down. The patient was allowed to recover from the anæsthetic, and delivered herself in about twenty minutes of a male child weighing 2 lb. 14 oz. The placenta was expelled five minutes later. The child did not survive. The patient made an uninterrupted recovery.

The patient continued well and became pregnant again, her last period being July 22-26, 1920. From August 28 to September 14 she had a slight blood-stained discharge each day, which ceased after rest in bed and the administration of small doses of ergot and potassium bromide. On February 27, 1921, when about thirty-two weeks pregnant, she began to bleed. She was kept in bed and given morphia. On examination the uterus corresponded in size with a thirty weeks' pregnancy. The child was lying transversely with its head in the left iliac fossa. The foetal heart was 144. On vaginal examination the cervix was undilated, and nothing except the bleeding and the abnormal presentation suggested definitely the presence of placenta prævia. The patient continued to bleed, but the amount was never very great, and on March 13 she gave birth to a male child, weighing 5½ lb. The child presented as a first vertex and was delivered spontaneously. The placenta was adherent, and on account of post-partum hæmorrhage had to be removed manually. It was rather unusual from the fact of its consisting of one main piece, attached to the periphery of which were several small circular lobes, varying in size from 1 in. to 3 in. in diameter. There were eight lobes in all; one of the larger of these showed evidence of premature separation, and a certain amount of tearing, and it had undoubtedly been attached to the lower uterine segment, over the internal os. The diagnosis of placenta prævia could not be confirmed by feeling the placenta through the cervical canal, as I did not arrive in time to examine the patient before the birth of the child, but in its manual removal after labour there was no doubt that the placenta was attached to the lower uterine segment. The mother and child both did well.

Four months after this pregnancy I examined the patient and could not discover any abnormality of the uterus.

The patient again became pregnant in 1922, her last period dating April 17-22. She had slight hæmorrhage on the following dates: August 6 and 7, October 12-20, November 6-10, and on November 13 there was rather more hæmorrhage than at the previous dates. I examined her on November 14; the uterus corresponded in size with a thirty weeks' pregnancy, the child presented by the breech, and the foetal heart was 160. Vaginal examination was considered inadvisable. She was kept in bed and ordered a mixture containing bromide and small doses of morphia. There was a certain amount of bleeding every day, and on December 13 labour began. When seen two hours after the beginning of labour, during which interval of time there had been considerable bleeding, the patient's pulse was 108. The uterus was the size of a thirty-two weeks' pregnancy, the child presented by the breech, the foetal heart was 140. On vaginal examination the cervix was more than half dilated and placenta could be felt covering two-thirds of the dilated canal. I ruptured the membranes and brought down a leg. Delivery of a living male child weighing 4 lb. 6 oz. was completed in ten minutes. The placenta had to be removed manually on account of hæmorrhage, and showed no unusual features. The patient's temperature was raised owing to mild uterine infection for ten days, but subsequently she did very well. I have seen both these children recently and each of them is up to normal in physical and mental development.

The patient's husband is a medical man, and I am informed that her first pregnancy in October, 1914, was complicated by a lateral placenta prævia. There was considerable hæmorrhage for a week before labour, which occurred at seven and a half months and resulted in a stillborn child.

In reviewing the literature a case is recorded in the *Lancet*, 1889, by E. H.

Fitzpatrick [1], in which placenta prævia is said to have occurred in five successive pregnancies. The patient had four normal pregnancies, the fifth was a central and the sixth a marginal placenta prævia, neither pregnancy being under the observation of the author of the paper. During her seventh, eighth and ninth pregnancies she was under his care. In her seventh pregnancy she had a marginal placenta prævia, and in her ninth a central placenta prævia. The eighth pregnancy terminated at the fourth month and placenta prævia was diagnosed, as in manually removing the placenta he found it "engrafted low down on the anterior uterine wall."

The only other recorded case of placenta prævia occurring more than once in the same patient that I can discover is one described by Hedback [2], in which the condition was met with twice.

Mr. L. C. RIVETT said that he was surprised Mr. Gilliatt had found so few cases recorded in the literature. He (Mr. Rivett) would not have thought the condition was so rare. He was very much interested in the paper, as he had recently performed Cæsarean section for central placenta prævia on a 7-gravida who had had a central placenta prævia with her last pregnancy.

He recalled the case of a patient whom he saw in consultation, with her fifth pregnancy. Her first pregnancy was stated to have gone to about term. There was central placenta prævia, treated by boring a hole through the placenta and pulling down a leg. The second pregnancy terminated at about three and a half months, the placenta being expelled first. The third pregnancy terminated at about five months, the placenta again being first. In the fourth pregnancy there was a central placenta prævia, treated by plugging, and the placenta was born covering the child's head. When he saw her she was very anxious to have a living child. He again diagnosed central placenta prævia, as a mass could be felt between the examining finger and the head. There were two or three attacks of bleeding, which subsided on the patient resting, and the pregnancy was carried on the thirty-eighth week, when he delivered a living child by Cæsarean section, and removed a central placenta prævia. It was not in his experience that patients in cases of placenta prævia suffered from attacks of bleeding in the early months of pregnancy. In fact his experience was rather the opposite, and he recalled a case which bled intermittently practically every day for six months, and then went to term and had a normal labour. He himself had examined the placenta and membranes, and had found a rent almost exactly opposite the placenta. It was important to take very great care in verifying the symptoms in these cases. He had that morning seen a case in Queen Charlotte's Hospital which was brought in with ante-partum hæmorrhage due to a blow from the handle of a mangle. On careful questioning he elicited the fact that the bleeding had begun the day before, and that the patient was trying to get her washing done before labour really came on.

#### REFERENCES.

- [1] FITZPATRICK, E. H., *Lancet*, 1889, i, 678. [2] HEDBACK, A. E., *St. Paul Med. Journ.*, Minnesota, 1903, v. 48.

### A Case of Inversion of the Uterus with Squamous-celled Carcinoma of the Fundus.

By HERBERT WILLIAMSON, M.D., and G. F. ABERCROMBIE, B.Ch.

C. B., AGED 54, married 23 years, was admitted to St. Bartholomew's Hospital on May 14, 1923, complaining of a profuse vaginal discharge, offensive and blood-stained.

Menstruation had commenced at the age of 14, the cycle was twenty-eight days, the duration seven days, and the flow profuse. The menopause occurred in 1921. The patient had never been pregnant.

She was in her usual health until October, 1922, when she first noticed a vaginal discharge, sometimes blood-stained. This gradually increased in amount. In March, 1923, the discharge became yellow and offensive and hæmorrhage occurred two or three times weekly. About this time she commenced to suffer severe pain, felt chiefly in the sacral region, she became anæmic and her health deteriorated rapidly; she also developed incontinence of urine on coughing and on exertion and when lying in bed. These symptoms persisted to the time of her admission.

When first seen the patient looked ill, she had a yellow, sallow complexion, her tongue was furred and she was anæmic. The temperature was  $100.4^{\circ}$  F., and the pulse-rate varied between 80 and 100.

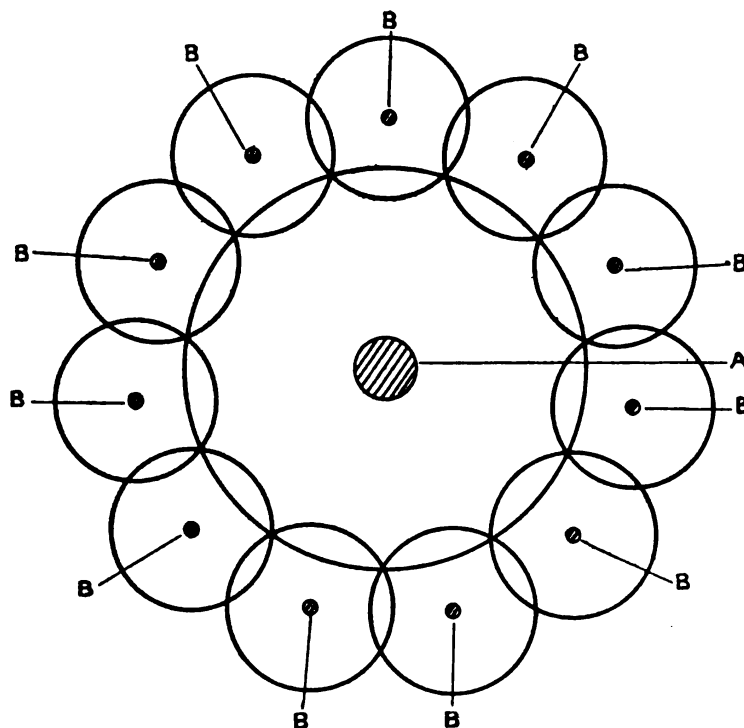


FIG 1.—Diagram to show arrangement of radium tubes and needles. The shaded dots represent the radium. The circles indicate the area of growth irradiated. A, tubes containing hydrated radium bromide equivalent to 99.4 mgr. of radium metal; B, needles containing collectively hydrated radium bromide equivalent to 62 mgr. radium metal. (Diagrammatic.)

Abdominal examination did not reveal anything abnormal. The vagina was occupied by a sloughing irregular mass as large as a man's fist; the tissues composing it were friable and large fragments came away on touch. The relation of the mass to the cervix uteri was not definitely determined but the anterior lip could be felt high up behind the symphysis pubis and its texture was apparently normal. The examination caused such profuse hæmorrhage that it was abandoned, the vagina hastily plugged and an anæsthetist sent for. On his arrival the plugging was cautiously withdrawn and the remainder of the mass removed by the finger. It was then discovered that the uterus was completely inverted and that the necrotic mass was

## 6 Williamson and Abercrombie: *Squamous-celled Carcinoma*

attached by a broad pedicle to the fundus. The growth was easily detached and left an irregular area of ulceration, measuring approximately 3 cm. in diameter, on the fundus; several small hard nodules were felt about its periphery. Microscopical sections of the growth proved it to be a carcinoma.

The vagina was plugged and the patient sent back to bed. Vaginal douches of lysol were given twice daily and at the end of a week the discharge had almost ceased and the surface of the inverted uterus appeared clean.

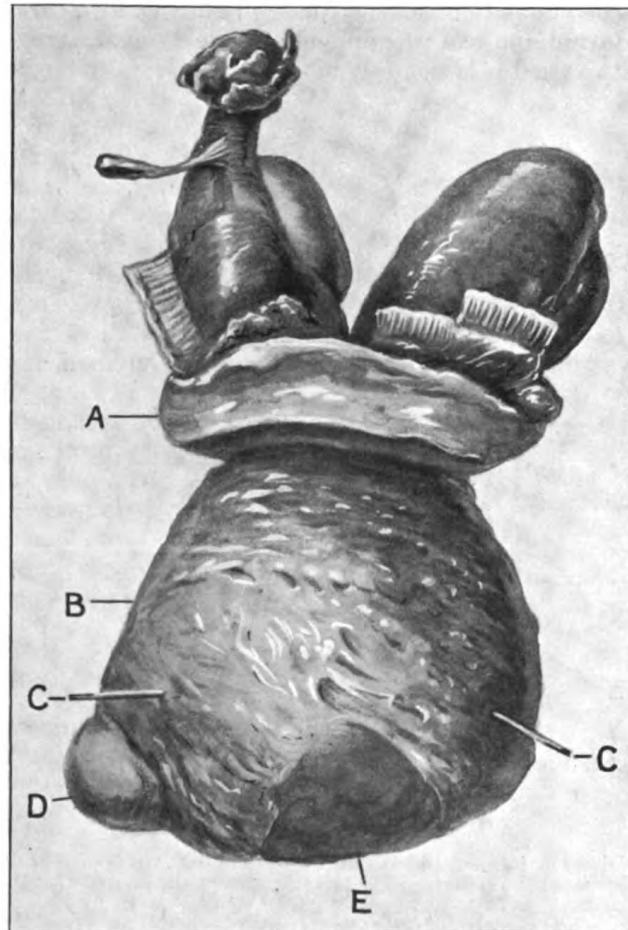


FIG. 2.—Specimen of inversion of the uterus caused by squamous-celled carcinoma of the fundus. A, cervix uteri; B, inverted body; C, uterine ostia of Fallopian tubes; D, nodular growth; E, area from which vaginal mass was detached.

For the last two years we have made a practice of irradiating malignant growths of the uterus before removal, but in this case we hesitated to apply radium until the septic condition of the vagina and growth had abated. On May 23 radium was applied: Thirteen needles, containing in all hydrated radium bromide equivalent to 62 mgr. of radium metal screened by 0.4 mm. of platinum, were inserted into the tissues around the periphery, and two tubes containing hydrated radium bromide equivalent to 99.4 mgr. of radium metal

screened with 1 mm. of platinum were placed in contact with the growth and kept in position by means of gauze plugging (fig. 1). Tubes and needles were left *in situ* for forty-eight hours. The total dose was therefore approximately 7,650 millicure hours. Examination on June 4 showed that the ulceration was healing. On June 11 panhysterectomy with removal of both ovaries and tubes was performed by the abdominal route. Convalescence was uninterrupted and the patient left hospital on July 2.

*Description of the Specimen.*—The specimen consists of uterus, ovaries and Fallopian tubes. The uterus measures 6·5 cm. in length. The body is completely inverted and is seen as an ovoid mass protruding through the dilated cervix, which surrounds it like a collar. The greatest circumference is at the lower pole and measures 14·5 cm. The mucosa covering the inverted body is rugose and ulcerated in places. A rounded nodule of growth measuring 2·8 cm. in diameter and raised 1 cm. above the level of the surface is situated to the right of the fundus near the uterine ostium of the Fallopian tube. From this nodule a section has been prepared. Below this, on the fundus, is a smooth oval area measuring 2·5 by 2 cm., marking the site from which the vaginal mass was detached and to which the radium was applied. The uterine ostia of both Fallopian tubes are seen and a coloured glass rod has been inserted into each. The ovaries and tubes are seen at the top of the specimen and are drawn into the cup-like depression of the peritoneal surface produced by the inversion. The abdominal ostium of the left tube is sealed and the infundibulum dilated into a club-shaped swelling. The ostium of the right tube is patent. Both are infiltrated with blood. A small accessory tube with closed ostium is attached to the ampulla on the right side (fig. 2).

We are indebted to Sir Bernard Spilsbury for the following description of the microscopical appearances.

“ *Sections prepared from the fundus uteri* show a malignant growth which consists of dense fibrous tissue traversed by small masses and narrow columns of tumour-cells. The cells, many of which are compressed by the fibrous tissue, are of different sizes and shapes. The arrangement of the cells suggests either a squamous-celled cancer or an endothelial sarcoma, but the presence of prickly-cells and of areas of keratinization in sections of the growth removed on May 15 indicate that the tumour is an atypical squamous-celled cancer. No mitotic figures have been observed. *Sections of the area exposed to radiation* show a broad strip of recently formed and vascular fibrous tissue adjacent to unstriped muscle. Between the two tissues is a zone in which hyaline necrotic material and blood-cells can be seen. In this zone are numerous multinucleated giant-cells of the order of foreign-body giant-cells. No malignant growth is visible in the section and there is no indication that the necrotic tissue was originally neoplastic. *Sections of the inverted uterine wall* show unstriped muscle with a little fibrous tissue on its surface and several glands which penetrate for a short distance into the muscle. The glands are lined by columnar epithelium arranged regularly, but the lumina are somewhat compressed. There is no inflammatory reaction around the glands and no indication that they are neoplastic. The condition is probably an irritation over-growth.”

It is not easy to understand how a squamous-celled carcinoma can arise in the fundus uteri. The first explanation that suggests itself is that the inversion preceded the growth, and by a process of metaplasia the epithelium of the inverted fundus was converted into the squamous type. The patient, however, had never been pregnant, and the specimen shows no fibroid in the wall, so that it is difficult to see how the inversion could be produced; and, in addition, where epithelium is preserved on the inverted wall, it is columnar, not squamous-

## 8 Williamson and Abercrombie: *Squamous-celled Carcinoma*

celled. Further, the sudden development of frequent and profuse hæmorrhages, pain and incontinence of urine in March, 1923, suggest that the inversion took place at that time.

Chronic inversion of the uterus due to a malignant growth attached to the fundus is not common, and in nearly all recorded cases the growth has been a sarcoma. In 1911 Thorn [1] collected 437 cases of inversion of the uterus, eighty-three of which were caused by a neoplasm, and of these five only were due to malignant growths. Of these growths four were sarcomata, and the nature of the fifth is not clear: it is one of the cases recorded by Stark [3], to which reference will be made. In 1919 Dr. Herbert Spencer [2] recorded nine cases of chronic inversion, four of which were caused by neoplasms, but only one of these, a spindle-celled sarcoma, was malignant. In 1913 Stark [3] recorded two interesting cases, but the descriptions of the tumours make it improbable that the growths were carcinomata. The first patient was a woman aged 57. A sloughing mass was found in the vagina attached to the fundus of the inverted uterus; the patient was desperately ill and no operation was performed. The tumour was reported as "undoubtedly malignant and composed of large epithelioid cells. . . . There is no trace of fibromatous, muscular or glandular structures in any part. It is carcinomatous, of an endothelial type." This description suggests that the tumour was an endothelioma, and therefore a sarcoma or mesothelioma rather than a carcinoma. Stark's second patient was a woman aged 56. The vagina was blocked by a gangrenous friable tumour, which at first appeared to be a polypus, but on further investigation was found to be the inverted uterus with a malignant neoplasm attached to its fundus. The case is described as one of inversion of the uterus caused by a cancerous tumour of the fundus, but Teacher's report upon the growth stated that it was "a perfect example of an endothelioma." It is doubtful, therefore, whether the growth in the first case was a carcinoma, and certain that the growth in the second was not.

As far as we have been able to ascertain, there is no record in the literature of an authentic case of chronic inversion of the uterus caused by a carcinoma of the body, and we have brought the specimen before the Section to-night because of its rarity.

### REFERENCES.

[1] THORN, W., "Zur Inversio Uteri," "Samml. Klin. Vortr.," 1911, N.F., No. 625-27 (Gynäk., No. 229-31), pp. 101-209. [2] SPENCER, H. R., *Proc. Roy. Soc. Med.* 1919, xiii, (Sect. Obst. and Gyn.), p. 20. [3] STARK, J. N., *Journ. of Obst. and Gyn. Brit. Emp.*, 1913, xxiv, p. 68.

### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) briefly referred to the rarity of the specimen.

Sir GEORGE BLACKER agreed that the best results were obtained when the radium was imbedded in the growth or round the periphery, but he said that this was often not easy to accomplish, especially in cases of carcinoma of the cervix uteri. It was very important that radium should not be used in cases in which there was evidence of any acute local septic infection; in such cases general septic infection was very likely to follow, and it had supervened in one case of this description treated at University College Hospital. Radium had practically no sterilizing effect upon the tissues.

## **The Treatment of Puerperal Sepsis by Quinine Injections. (A New Method Illustrated by Charts.)**

By S. GORDON LUKER, M.D.

PUERPERAL septicæmia, the most acute and fatal form of puerperal sepsis, continues to occur with considerable frequency in spite of modern preventive methods before and during labour.

In the year 1920, in England and Wales, there were nearly 1,800 deaths from this disease, which represented a mortality rate of 1·87 per 1,000 births. This figure is no lower than that of preceding years, though ante-natal clinics and increased care in the management of labour should have exerted a beneficial effect. It seems, therefore, that while continuing the preventive campaign with unabated vigour, we should strive to find some method or methods of treatment which will abort the disease in its early stages or cure it when once it is established.

Up to the present no line of treatment has been found to be very successful in septicæmia. In general practice local treatment is nearly always tried by giving intra-uterine douches or even by curetting, and drugs are administered by the mouth in the form of iron, ergot, aspirin or quinine. A small dose of anti-streptococcal serum is sometimes given subcutaneously, and in a few picked cases a vaccine is made from organisms found in (or isolated from) the blood or uterus and given in small doses. As to the above drugs given by the mouth, I have been disappointed with them, and have felt that in many bad cases they are not absorbed by the stomach, and therefore have very little, if any, effect. As regards local treatment, an intra-uterine douche is not without risks if given without rigid aseptic precautions, which can only be carried out under an anæsthetic; and curettage is dangerous.

Owing to the doubt which has up to the present existed as to any form of treatment which is really efficient in the early stages, and the lack of any routine method of treatment, a patient suffering from puerperal infection of only moderate severity becomes worse, till a condition of septicæmia supervenes; or, in some cases, harmful interference such as curetting or intra-uterine douching without an anæsthetic is carried out, and the disease is aggravated rather than cured. I have, therefore, endeavoured to find a method of treatment which could be used in a routine way in all cases of acute puerperal infection. The method which I have evolved and used consistently now for more than a year is based upon intravenous and intramuscular injections of bi-hydrochloride of quinine. The method has been employed and tested in cases in which the clinical picture of puerperal septicæmia is present; the success obtained in these cases makes me feel that if the treatment be started early in all cases of puerperal infection it will, so to speak, abort this and prevent the development of the septicæmic state.

During the past eleven years—excluding 1914 to 1919—I have tried various methods of treatment—some old, some new—with results not sufficiently satisfactory to warrant continuation of their use. The tests provided were very rigid, as many of the cases treated were very acute, some almost beyond hope of recovery. The majority of the cases were in the Isolation (Puerperal Septicæmia) Ward at the London Hospital, and were, therefore, under similar conditions of observation and nursing. Among the methods of treatment which were tried were certain intravenous injections and treatment with vaccines.



Vaccines were worked out thoroughly some years ago, as a result of which it was concluded that an autogenous vaccine was useful in some cases. Drugs used intravenously were collargol, neosalvarsan, and later—as a result of war experience—eusol and Dakin's solution. The administration of these two last mentioned drugs seemed to be followed by a favourable reaction, but, under critical observation, one death checked the enthusiasm inspired by several successes. In May, 1922, I was considering the value of anti-streptococcal serum and the possibility of producing something of the nature of protein shock by injecting anti-streptococcal serum intravenously. As I considered this to be not devoid of risk I decided to revert to the subcutaneous injection of large doses and to follow this with intramuscular and intravenous injections of quinine bi-hydrochloride in water. This treatment was so successful from the beginning that for the past year it has been adopted as a routine and used in a series of twenty-five cases with only two deaths.

As far as a routine can be adopted, the following method is carried out. As soon as the puerperal woman shows any signs of acute sepsis or as soon as she comes under observation, careful examination of her general and local condition is carried out in order to decide whether exploration of the uterus is advantageous or necessary. This is, as a general rule, carried out if there is any history suggesting retention of placenta or membranes, if there is any abnormal bleeding or if there is any foul discharge with subinvolution of the uterus, but only in a minority of cases. If exploration of the uterus is decided upon it is carried out with great care and asepsis, and always under an anæsthetic. Exploration of the uterus is not performed if the history of a normal third stage is obtained, if the uterus is of normal size, if the lochia are unaffected, or if there is any abdominal distension or pelvic peritonitis. If exploration is carried out it should be done with very great care with the gloved finger, so as to avoid causing bleeding; the uterine cavity should be swabbed out with iodine and a hot intra-uterine douche given. I do not consider continuous irrigation of the uterine cavity by the Carrel-Dakin method suitable or useful except in a special case. During the last twelve months I have explored the uterus less often than formerly. Other surgical treatment occasionally is required, e.g., for pyæmic abscesses or peritonitis, but is not common in the acute stage.

The special treatment is as follows: (1) *Injections of anti-streptococcal serum.* These are given for the first three days in doses of 30 c.c. on each of the first two days, and 20 c.c. the third day. (2) *Injections of quinine.* (a) Intramuscular injections of quinine bi-hydrochloride, 5 gr. in 1 c.c. of sterile water are made in the buttock or thigh on alternate sides every day for six to ten days. It is important to massage away the solution from the site of injection. This is usually followed by some definite improvement, which is shown on the charts by a diminution in the number of rigors or a drop in the temperature or pulse-rate. An interval of a few days is allowed and the injections can be repeated. Intramuscular injections have been tried in almost every variety of case with extremely satisfactory results. There appears to be no risk attached to the injections, and in only one case was there any ill effect—slight giddiness and headache. These intramuscular injections are useful not only in the early and in the acute stages of the disease, but in the more chronic stages, to hasten convalescence. The success attending intramuscular injections of quinine justified an investigation of the effects of (b) Intravenous injections, in order to get a more rapid or increased effect in the worst cases. For this purpose injections containing quinine bi-hydrochloride, 3 gr. in 10 c.c. of sterile water have been employed; as a general rule, two or three injections

have been given at intervals of forty-eight hours, but further injections may be given if the improvement is not maintained or if a relapse occurs. The intravenous injections should be followed by a course of intramuscular injections. Intravenous injections have been reserved for the worst cases so that the test should be as severe as possible. The results have been very striking, including the cure of two patients with panophthalmitis complicating puerperal septicæmia. Previously, I had thought this complication the most certainly fatal of all. More recently the dose of quinine given intravenously has been increased to five grains.

*Reaction to the Treatment.*

In most cases no symptoms result, at once, or later, from the injections, whether intravenous or intramuscular. But within a few hours of an intravenous injection the pulse and temperature will begin to fall and the patient will feel better.

After intramuscular injections the improvement, as would be expected, is more gradual but none the less certain. The intramuscular injections may cause a little discomfort for a few hours but there is no risk of infection occurring later, nor even of a fixation abscess.

No symptoms of cinchonism have occurred even in the severest cases except in one patient who seemed to have an idiosyncrasy; however, she made a good recovery.

The effect of the treatment can be best illustrated by charts of the cases treated (see figs. 1-4, pp. 12, 13). If any value can be attached to figures showing the mortality of the disease in cases admitted to the Septicæmia Ward, the following are interesting. For the past twelve months, during which time the treatment had been carried out in a routine way, the number of cases admitted to the Septicæmia Ward was thirty-six, with only three deaths; of these fatal cases one was hopeless on admission and died the day following, before quinine injections had been begun. Thus, only two deaths occurred in the twelve months among the patients treated with quinine, a mortality of 5·8 per cent. Comparing this with previous years, the number of cases admitted from 1919 to 1921 was one hundred and forty-six, the number of deaths, fifty, giving a mortality of 34·2 per cent.

*The Action of Quinine.*

The manner of action of quinine is one of conjecture only. From a study of its action in malaria it is known that it is very active, even in very dilute solutions, on the lower forms of living protoplasm such as the amœba and the parasite of malaria. According to Cushny there is at first a diminution in the number of white corpuscles in the blood after quinine administration and a lowering of their vitality; this is followed by an increase in the number of white corpuscles.

According to Roth, "a single dose generally causes leucocytosis at first, probably arising from contraction of the spleen. This is followed by a fall in the number of white corpuscles, especially of the lymphocytes, though the polynuclear cells are also reduced. The polynuclears then increase in number until a distinct leucocytosis is present but the lymphocytes remain reduced in number while in the preliminary leucocytosis they predominate."

In some of my series of cases blood-counts have been done before and after quinine injections, but no very definite information can be obtained therefrom. In one case the following figures were obtained:—

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(1) Number of white corpuscles, before treatment, 23,200. Percentage of polynuclear neutrophils, 72 per cent. Percentage of small lymphocytes, 16 per cent.

(2) After some treatment, one week: Number of leucocytes, 10,880. Percentage of polynuclear neutrophils, 72.5 per cent. Percentage of small lymphocytes, 13 per cent.

(3) One week later: Number of leucocytes, 9,200. Percentage of polynuclear neutrophils, 66 per cent. Percentage of small lymphocytes, 25.5 per cent.

In one patient who died under treatment the leucocytes rose from 5,200 to 11,300 in the course of six days. The percentage of polynuclear neutrophils changed from 68 per cent. to 81 per cent. The percentage of small lymphocytes fell from 14 per cent. to 8 per cent. These investigations are probably influenced by the presence of severe anæmia.

My own feeling about the action of quinine is that it may stimulate the living tissue cells to increased resistance and may in some way prevent the harmful effect of the poisons or toxins circulating in the blood.

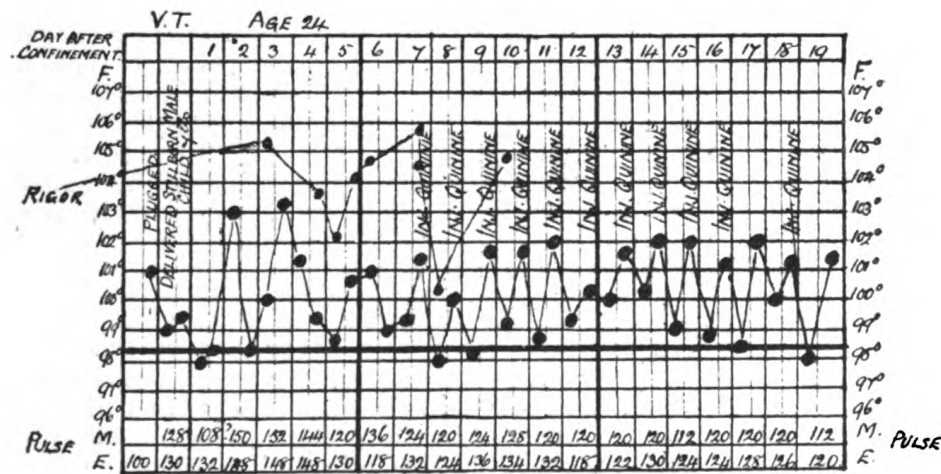
My conclusions may be summarized as follows:—

(1) The treatment of puerperal septicæmia by intramuscular and intravenous injections of quinine is of great value.

(2) The treatment is also useful in all forms of puerperal sepsis both in the early and also in the more chronic stages.

(3) Its action is preventive, or abortive, as well as curative; thus if given in all cases of acute sepsis it will prevent septicæmia.

FIG. 1.



PUERPERAL SEPTICÆMIA. PLACENTA PRÆVIA MUCH HAEMORRHAGE PLUGGED. ONSET SECOND DAY RIGORS STOPPED BY INTRAMUSCULAR INJECTIONS OF QUININE. UTERINE CULTURES STREPTOCOCCUS, STAPHYLOCOCCUS, AND COLIFORM. DISCHARGED FORTIETH DAY.

FIG. 2.

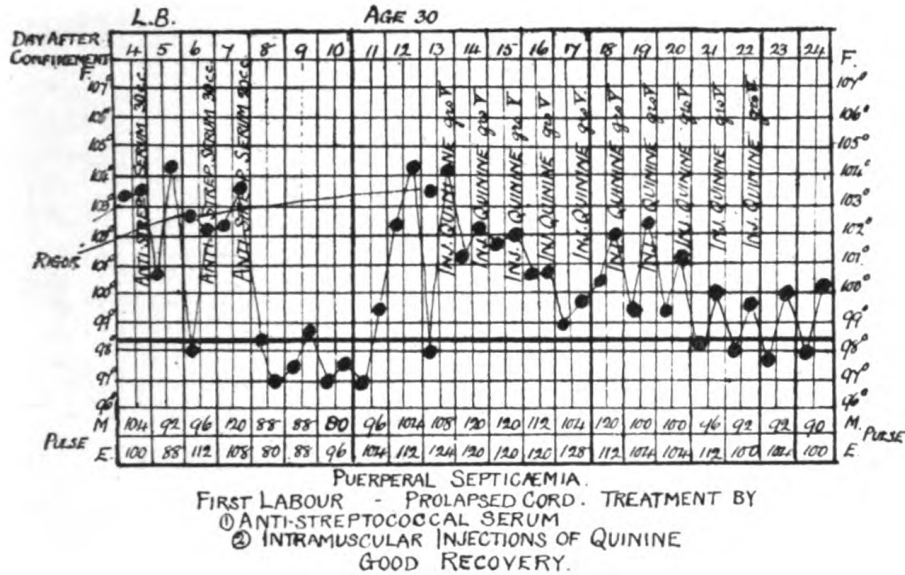
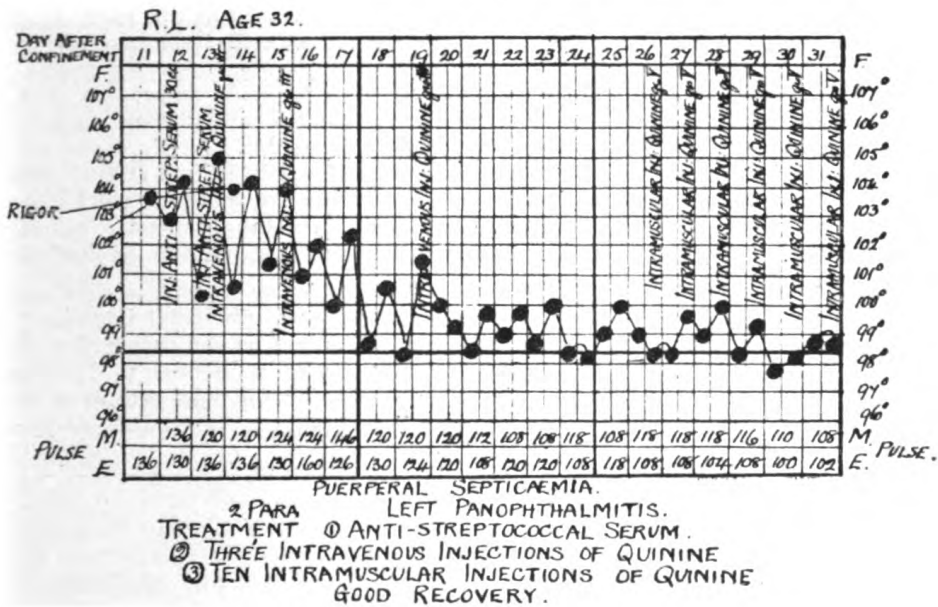
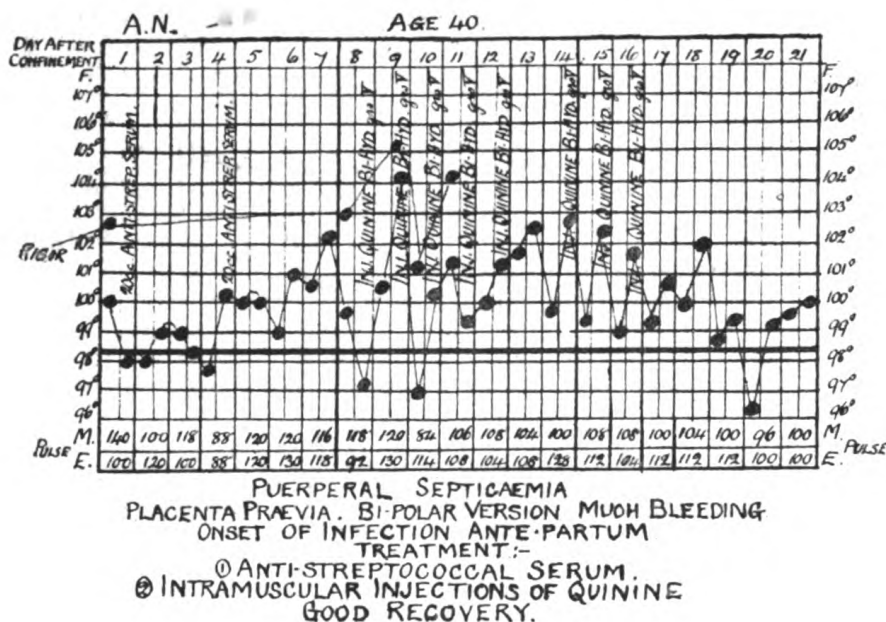


FIG. 3.



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FIG. 4.



## DISCUSSION.

Mr. J. D. BARRIS said that the treatment adopted by Mr. Luker was specially interesting because it produced a diminution in the number of leucocytes. This was contrary to what resulted from applying the usual methods, which aimed at protecting the patient by increasing her white cells. The views expressed in the paper required careful consideration, and he pointed out that there were difficulties in assessing the results to any particular treatment in such cases. Could Mr. Luker say what action quinine had upon cultures of streptococci, and whether he could at present give any evidence by animal experiment that quinine protected the animal from a fatal dose of streptococci. He (Mr. Barris) could not accept all Mr. Luker's cases as true septicaemia, for in some of them there was no record of positive blood culture and the pulse. A recorded comparison of cases was also requisite: of some treated by serum, of some by vaccine, and of some by Mr. Luker's method. He was in entire agreement with Mr. Luker, however, in his conservatism in avoiding intra-uterine manipulations.

Mr. LUKER (in reply) said that he realized the difficulty of diagnosing a case as "puerperal septicaemia" as opposed to less severe forms of puerperal infection; and it was in order to emphasize the severity of the cases that he had shown the charts. The following points made up the clinical picture of septicaemia: (1) Rigors; (2) temperature over 102° F., if repeated or persistent; (3) pulse-rate over 120, if repeated or persistent; (4) sleeplessness; (5) constipation or diarrhoea; (6) pyæmic abscesses or emboli. Several or all of these were constantly present. A positive blood culture could not be taken as the only reliable guide as it was absent in many cases, some of which ended fatally. He wished to emphasize that his new method of treatment had been used particularly in the most severe cases and had been withheld if the patient showed any distinct improvement during the first two or three days of observation in the hospital ward. This was one of the reasons why anti-streptococcal serum was given for three days before the other treatment was begun. The special isolation ward at the London Hospital was for cases of puerperal septicaemia only, and no case of less severe infection, such as sapraemia or parametritis, was allowed to be admitted.

## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### A Case in which over 3 ft. of Small Intestine were dragged out through a Perforation in the Uterus.

By A. C. PALMER, F.R.C.S.

AT 1 p.m., on August 26, I received a telephone message from a practitioner asking for an immediate consultation. When I reached the house, the doctor gave me the following history:—

The patient, a single woman of 28, had been bleeding for three weeks after six weeks' amenorrhœa. The patient had previously been seen five days earlier by another doctor, who diagnosed an "incomplete miscarriage," and advised exploration of the uterus.

On August 25 an examination had been made with a view to exploration of the uterus by the doctor who asked me to see the case. The doctor stated that he found some soft tissue just inside the cervix, that he pulled this tissue down and failing to recognize it sent the patient home to bed.

The tissue in question had since prolapsed to a considerably greater extent, and its nature was still in doubt. The doctor further stated that he had made inquiry with regard to any interference before coming to him for advice; and that the patient had replied that nothing whatever had been done.

I proceeded to examine the patient, finding her apparently well and complaining of nothing. Her complexion was good, the tongue clean and moist, the abdomen was soft, and no resistance or tenderness was discovered anywhere. The pulse was 90, and the temperature 98'6° F. The patient stated that the bowels had been open on the morning of the previous day.

Protruding from the vulva was a mass of *black* tissue, to which blood clot was adherent. On examining the tissue it was found to be in the form of a loop, about 2 ft. 6 in. in circumference. At the point most distal from the vulva, the tissue was considerably lacerated, the loop being almost completely divided. It was then discovered that the loop possessed a lumen, and on turning a portion inside out, intestinal mucosa was recognized. The radicles of the loop disappeared into the vagina, and on vaginal examination they could be followed up to the external os, where they passed into the uterus. Bimanual examination was a little difficult; the uterus appeared to be slightly enlarged, though certainly not larger than an eight weeks' pregnancy.

The patient was immediately transferred to hospital, and was seen by my house surgeon one and a quarter hours after the completion of my

examination. At this time the abdomen was slightly distended, no tenderness could be felt; the temperature was 99° F. and the pulse 96. At the time of operation, one and a quarter hours later, distension had considerably increased, with the result that the abdomen resembled in appearance a six to seven months' pregnancy. The general condition of the patient remained unaltered.

*The Operation.*—The abdomen was opened by a mid-line sub-umbilical incision. A little blood—2 oz. at most—was found in the abdominal cavity.

The first structure encountered was a soft *black* mass, infiltrated with hæmorrhage. This was a portion of the mesentery, minus small intestine. At either end of the black mass, intestine was found attached to the mesentery, the peritoneal coat being rolled back on the attached intestine after the manner of a rolled-up sleeve. From each point of attachment to the mesentery each strand of small intestine was followed down to the fundus of the uterus, where it disappeared into the cavity of the uterus, passing through and being gripped by the walls of a somewhat ragged transverse perforation (1 in. in length) in the top of the fundus of the uterus.

The two strands of intestine were cut across at their point of entry into the fundus. The lacerated thrombosed mesentery was removed, the ends of the small intestine were trimmed, and an end-to-end anastomosis performed. Mr. T. Twistington Higgins kindly withdrew the excised intestine from the vagina and uterus. Owing to the fact that the cervix was gripping the gut some difficulty was encountered in the withdrawal.

The perforation in the fundus was trimmed and sutured, a large rubber tube was passed down into the pouch of Douglas and the abdominal wound was then sutured.

*Post-operative Condition.*—The tube was removed on the second day, its removal being followed by a slight persistent purulent discharge and a good deal of infiltration round the track of the tube. The wound continued to discharge slightly for twenty-four days, but after that it healed rapidly.

For eleven days after the operation the temperature varied up to 102° F.; until the twenty-first day it varied up to 101·8° F., and then rapidly settled. On the fifth day the pulse reached 108, until this time having varied between 84 and 104. After the seventh day the pulse remained below 100, varying between 72 and 96.

Vaginal discharge was purulent and offensive for five days after operation, and on the fifth day the uterus was gently explored, nothing being discovered except a few fragments of necrotic débris. Considerable infiltration was found in the pouch of Douglas. The infiltration cleared up with douches, and at the time of discharge from hospital, fifty-six days after operation, the patient was well. No abnormality was felt in the pelvis; the temperature and pulse had been normal for twenty-five days.

#### DESCRIPTION OF SPECIMEN.

The specimen is in two parts and consists of (1) a portion of small intestine, and (2) a portion of mesentery.

(1) The intestine measures 3 ft. 3 in. (97·5 cm.) in length; it is black or pinkish-brown in colour. In rather more than half its extent, where the peritoneum has been stripped, the outer surface of the gut is rough; in the remainder, where the peritoneum remains attached, the outer surface is smooth and glistening. Over an area of 10 cm. in the middle of the specimen,

where the intestine has been lacerated, the gut is represented by a thin bridge of tissue, varying from 0.5 cm. to 1.5 cm. in width, and consisting of a thin strip of peritoneum, muscle and ragged mucosa. Twenty cm. from one end of the specimen the lumen of the gut has again been opened in two places. The holes in the wall measure 2.5 cm. and 1.5 cm. in diameter respectively, and are separated by a narrow bridge of tissue 1 cm. in breadth. Tags of peritoneum remain attached to the wall of the gut, the longest at one end measuring 10 cm. in length and up to 1.3 cm. in breadth. Towards the middle of the specimen a strip of peritoneum, up to 2 cm. in width, has been separated from the underlying muscle; it remains attached at either end and forms a loop 18 cm. in length.

(2) The mesentery measures 20 by 3 by 2.5 cm. The free edge is swollen, bluish-black in colour and the vessels are thrombosed. Attached to the free edge are two fringes of peritoneum, measuring up to 1.5 cm. in width. Between the peritoneal fringes, running along the whole length of the free edge of the mesentery, is an irregular gutter, varying up to 1.5 cm. in depth and partially lined by blood clot. At one end of the mesentery the peritoneal coat is complete for a distance of 2 cm. and forms a cuff 2 cm. in diameter.

#### DISCUSSION.

Dr. ANDREWS said that Mr. Palmer had been brilliantly successful in his operation. Forcible dragging down of intestine was probably more common after rupture of the vagina in labour at term than in attempts at emptying the uterus in the early months of pregnancy. A few years ago Professor Meiklejohn showed a somewhat similar specimen in Edinburgh. Almost the whole of the small intestine had been pulled out through a rent in the posterior fornix after a brutal delivery, and yet, when the abdomen was opened at the post-mortem examination, the intestines seemed all to be *in situ*, the peritoneal coat having remained almost intact while the whole of the mucous membrane and part of the muscle had been separated from it.

Mr. EARDLEY HOLLAND said that Mr. Palmer had exhibited skilful surgery in his operation. He (Mr. Holland) thought the title of the specimen could be improved, for the case was one of evisceration rather than of herniation of intestine through the perforated uterus. He thought these cases occurred less rarely than was supposed, and he himself had had experience of two cases of evisceration of small intestine through a perforation of the uterus caused by the inept use of ovum forceps for evacuating the uterus in early pregnancy.

In the first case, as a passive spectator, he had seen the whole tragedy enacted from beginning to end, and it was a striking demonstration of how the accident happened, and, above all, of the deceptive appearance of the intestine when it was dragged out. The case was one of evacuation of the uterus, for a legitimate purpose, in the fourth month of pregnancy. The ovum forceps grasped something which offered considerable resistance to withdrawal, and brought away a small lump of tissue which later revealed itself as a piece of uterine wall, with peritoneum on one side and decidua on the other. The forceps was again introduced and, without much resistance, was withdrawn, holding the end of a loop of membranous-looking tissue. There was speculation as to what this was. Was it umbilical cord? Some more was withdrawn for better inspection. It did not look like cord. Was it foetal intestine? This seemed to be the solution, so a good deal more was withdrawn, and only then was it realized by the operator that he had eviscerated a long loop of the unfortunate woman's small intestine. The appearance was extraordinarily deceptive; the intestine was stripped off its mesentery as cleanly as if had been cut off with a knife (the doubled loop with mesentery being drawn through an opening too small to allow the easy passage of both, the mesentery got left behind); the limp, stretched and bloodless intestine was like a pale, narrow ribbon.

A few years ago he had been called to the second case by the unfortunate doctor



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who, in the course of clearing out, for no legitimate purpose, a two and a half months' pregnancy, had withdrawn a loop of gut through the cervix before he realized he was eviscerating his patient. He (Mr. Holland) found a loop of characteristically attenuated, anæmic, lifeless gut hanging from the vulva of a slightly collapsed and anæmic patient. He opened the abdomen and excised 45 in. of small intestine, together with a big wedge of mesentery which was infiltrated with blood almost up to its root; the divided intestine was united by end-to-end anastomosis and the patient made a smooth recovery. The excised 45 in. probably represented less than half that length of intestine in the normal state, so much had it been stretched during the process of pulling it through the perforation, which appeared a mere jagged puncture in the posterior wall of the retracted uterus.

A few months ago he was called to a third case of evisceration of intestine, but in this case the intestine had been dragged out through a perforation in the posterior vaginal fornix by a doctor who was attempting to remove a placenta retained after a full-time labour: he thought he had a grasp of membrane and umbilical cord, but blindly dragged out a handful of small intestine. In this case the intestine was not so attenuated but was stripped off its mesentery. Mr. Holland excised 16 in. of intestine, partly large and partly small gut, with the cæcum forming the mid-point. The patient was so collapsed that no more could be done than to excise the gut and attach the divided ends to the abdominal wound. The patient lived for a week.

When an expert gynæcological surgeon had to evacuate the early pregnant uterus (a rare operation, for cases of therapeutic induction of abortion were uncommon) he approached the operation with considerable respect, for he realized its dangers and difficulties; but it was surprising how lightheartedly some doctors would undertake this operation. Most of these cases of evisceration of intestine were due to the use of the ovum forceps, with which it was very easy to grasp and tear away a doubled-up piece of the thin, relaxed uterine wall.

### Venous Infarction in a Foetal Liver.

By A. C. PALMER, F.R.C.S.

THE specimen is a liver removed from a stillborn foetus aged 282 days. Foetal death was due to asphyxia from pressure on the umbilical cord during a long second stage.

The mother was well during pregnancy. The Wassermann reaction was negative and the pelvic measurements were normal.

#### DESCRIPTION OF SPECIMEN.

The liver is 12 cm. wide. The greater part of the tissue of the right lobe is replaced by a mass of blood clot (7 by 5 by 4 cm.). The cut surface of the clot shows an outer dark red zone, an intermediate opaque yellow zone, and a central white zone.

The hepatic veins in the clot and the primary branches of the right hepatic vein are filled with clot up to the point of entry into the inferior vena cava.

Two portions were taken for microscopic sections:—

(1) From the *opaque yellow area*, including a portion of the opaque white area and the capsule;

(2) From the *deep red tissue* and adjacent unaltered liver.

In (2) the hepatic columns are very widely separated and broken by blood. In this blood there are relatively few leucocytes. The picture is that of an extreme congestion leading to disintegration of the hepatic tissue.

In (1) the hepatic veins are very widely dilated and are filled with red

thrombus; the thrombus contains relatively few leucocytes and is nowhere organized.

In the *opaque yellow area*, the hepatic columns are widely separated and the cells are necrosed, only a few pale structureless nuclei being present. The channels between them contain a net of filaments, presumably fibrinous, in which are numerous neutrophil leucocytes and relatively few red corpuscles. The necrosed hepatic columns are usually bounded by a very narrow line of collagenous fibrils.

*The white area.*—Here there is complete necrosis. It is very difficult to differentiate the site of the hepatic columns and capillaries, but in places they are recognizable. The blood spaces are not so wide, they contain ghosts of cells and necrosed clot. The hepatic columns are replaced by wide trabeculæ of a very faintly stained homogeneous substance in which collagenous fibrils are just recognizable; at the borders of these trabeculæ the collagenous fibrils are stouter and more deeply stained.

*Organization.*—At the border of the white area a definite fibrous granulation tissue is formed. This tissue is formed from the walls of the large, thrombosed hepatic veins and from the capsule. The tissue which forms the trabeculæ in the site of the hepatic columns is converted into a definite fibrous tissue in which are many fibroblasts. The organization is most complete and fibrous next the walls of the veins and especially beneath the capsule. In the other direction it becomes more cellular, ending in streaming fibroblasts.

*Capsule.*—Upon the outer surface, over the necrotic area, there is a narrow layer of fibrin which is being organized. Beneath this is a deep layer of fibrous tissue. This can be distinguished from the true capsule beneath, because it contains no elastic fibres and its collagenous fibres are not so thick and not so densely packed. It contains numerous very wide capillaries. There is, therefore, upon the surface of the capsule a thick layer of fibrous tissue which has been formed by organization of a fibrinous deposit.

#### CONCLUSION.

The greater part of the changes in the abnormal area of liver appear to be due to venous infarction. The necrotic areas could also be ascribed to such venous infarction. But, in that case, organization of the thrombi in hepatic veins should be present and should be as advanced as the organization upon and beneath the capsule, and around the veins in the necrosed area. The microscopic examination has, so far, revealed no organized thrombosis of the hepatic veins. According to the present stage of the investigation, therefore, the venous thrombosis and consequent venous infarction would appear to be secondary to the changes in the yellow and white necrotic area.

The thick layer of organized fibrin upon the surface of the necrotic area might result from infarction of the subjacent tissue. But it would, perhaps, be better explained by an infective inflammation of the subjacent tissue. The capillaries in the necrotic areas contain numerous leucocytes whilst the capillaries in the surrounding wide area of obvious venous infarction contain ordinary blood. This, perhaps, also, favours infective inflammation of the necrosed area. But the actual histological picture in the necrosed area is a strange one for a hepatitis; the capillaries are distended by white clot and the hepatic columns are necrosed without inflammatory infiltration.

I am indebted to Professor Turnbull, of the Pathological Department of the London Hospital, for the histological investigation and provisional conclusions in this case.

### Three Cases of Pregnancy, with Extravasation of Blood Associated with Albuminuria.

By A. B. DANBY, F.R.C.S.Ed.

I HAVE ventured to record the following cases with a view to ascertaining the opinion of this Section as to whether the extravasations of blood in the pelvic cellular tissues can be regarded as one of the manifestations of a toxæmia of pregnancy. Large and small hæmorrhages are common pathological findings in eclampsia, and it appears reasonable to suppose that these hæmorrhages are not entirely due to the high blood-pressure so often found, but that other factors may in addition be present. The following is the record of the case-histories:—

*Case I.*—L. E. E., primipara, aged 30. Was attended during the confinement by Dr. Horton, of Chasetown, to whom I am indebted for kindly supplying notes on the case.

The patient's history was unimportant. No sign of varicose veins nor evidence of arteriosclerosis or renal disease. The last period ended on November 29, 1922, and the pregnancy was perfectly normal until a month before the confinement, when slight œdema of the ankles was noticed. No albumin in urine at four and a half months, but a trace was found fourteen days before labour.

She was first seen at 1 a.m. on August 29, 1923, about two hours after labour commenced.

A small swelling, the size of a walnut, was noticed in the right labium. The os was fully dilated, vertex presenting in L.O.A. position and only partially engaged. The swelling became progressively larger with each pain, until it bulged internally and prevented a vaginal examination being made. Assistance was then sent for, and by this time the swelling had increased to the size of a small fetal head and had extended to the perineum. Spontaneous rupture took place on the vaginal surface of the labium: a large amount of blood was expressed; this made it possible to extract a living 7½ lb. child with forceps.

The wound was tightly packed with iodoform gauze because of hæmorrhage. The duration of labour was four and a half hours. No pituitrin was given. I was asked to see the patient on August 30; she was then rather pale, but appeared comfortable. Pulse 120, temperature 97·6° F., blood-pressure 155 systolic; thick cloud of albumin in urine.

Nothing abnormal could be detected by abdominal palpation. The packing was removed and this removal revealed a large irregular cavity in the right labium. This was not explored as no anæsthetic was given, but it appeared to extend upwards towards the base of the broad ligament and backwards into the perineum. There was a certain amount of general oozing but no bleeding points were seen. The surrounding skin was discoloured and the vaginal wall in the region of the swelling extremely friable, the edges of the ruptured surface being very ragged. The cavity was again packed with iodoform gauze for twenty-four hours, and subsequently irrigated and lightly packed with gauze soaked in eusol. The patient was treated as for albuminuria of pregnancy.

The convalescence was rather prolonged, there being a slight rise of temperature, which did not subside until the end of the sixth week, by which time the wound had completely healed.

October 25: Small trace of albumin in urine.

*Case II.*—E. C., aged 32, primipara. Was admitted on March 13, 1923, to the General Hospital, Birmingham, under Mr. Beckwith Whitehouse, suffering from a para-vaginal hæmatoma following parturition. Dr. Pracy, of Atherstone, who attended the labour, kindly gave the following particulars:—

"At 5 a.m., on March 12, 1923, Mrs. C. was spontaneously delivered of a fully-formed

5 lb. child: the baby was alive but premature, probably of eight months' development. The placenta was expelled soon after, there being much less bleeding than usual. Pituitrin 1 c.c. was then given, and at the end of two hours the patient was very well, with a pulse of 72.

At 8.30 p.m. she began to have what appeared to be a return of labour pains; at 10.40 p.m. the pulse had risen to 144, and she was very pale and collapsed. Palpation showed the body of the uterus to be well contracted, but there was a tense swelling, occupying the cavity of the pelvis, the perineum and left labium."

The history of the pregnancy was the following:—

The patient could not remember the date of her last period; she felt in excellent health until January, 1923, when attacks of nausea and vomiting commenced accompanied by a burning sensation in the pit of the stomach. Lately she had noticed a yellow tinge of her skin, and had complained of much itching. The urine was last tested in the middle of January, and it then contained no albumin. On admission to hospital she was pale and slightly jaundiced, the temperature being 97° F., pulse 136, blood-pressure 132-72. There was a large hæmatoma the size of a foetal skull, involving the left labium, the perineum and buttock. The skin over the swelling was tense and of a dark purple colour. Abdominal palpation was difficult, owing to marked tenderness and rigidity; the uterus was displaced to the right and reached to just below the umbilicus. There was marked bulging of the left wall of the vagina, making catheterization extremely difficult. No tear of the perineum was detected. The urine contained a thick cloud of albumin. Blood-count 3,000,000 red cells. Coagulation time normal.

On March 16, under a general anæsthetic, a 2 in. incision was made on the anterior surface of the left labium, a large mass of blood-clot being evacuated. This exposed a ragged cavity extending into the buttock posteriorly and reaching above to the base of the broad ligament. Three drainage tubes were inserted and irrigation carried out with eusol.

Five days afterwards a rapidly spreading coliform bacillus infection set in. This gradually subsided, and after a prolonged convalescence she was finally discharged from hospital on April 11. I saw her again in the out-patient department on October 24, when she looked extremely well, and confessed that she was pregnant again.

Her last period ended on May 20, the uterus reached to just below the umbilicus. The operation scar was almost invisible, and nothing abnormal could be detected either abdominally or per vaginam. Heart and lungs normal, blood-pressure 130-80, pulse 72. Pathological report on catheter specimen of urine:—

"No casts seen, only a few squamous epithelial cells; a trace of albumin." Urea 4 per cent., NH<sub>3</sub>, co-efficient 1/2, diastase No. 5.

Case III.—B. T., aged 26, 1-para; normal pelvis; last period ended at the end of June, 1922.

*Obstetric History:* A five months' miscarriage in November, 1919. Urine last examined on March 1; no albumin; neglected reporting at the ante-natal clinic subsequently.

Labour commenced a.m. April 28, 1923: position of child L.O.A. Foetal heart heard; slight ante-partum hæmorrhage; os two fingers; no placenta felt; vagina and fornices packed with gauze and tight binder applied.

Admitted to hospital 4.45 p.m. the same day. Temperature 99.6° F.; blood-pressure 148-98, pulse 88, regular, of good volume, and not in keeping with patient's general condition—she was extremely pale, restless, dyspnoic and anxious-looking.

*Palpation:* The uterus reached to within two finger-breadths of xiphisternum. It was hard, globular, did not alternate between contraction and relaxation, and was extremely tender on deep pressure; no foetal heart or limbs detected; foetal movements ceased some hours beforehand.

*Vaginal Examination:* The os admitted two fingers, placenta not felt, vertex presenting and membranes ruptured; moderate amount of bloodstained discharge; numerous brownish-red patches which had the appearance of petechial hæmor-

rhages were noted in the vaginal walls. There was a thick cloud of albumin in the urine.

A diagnosis of premature separation of placenta with concealed and external hæmorrhage was made.

Cæsarean section under spinal anæsthesia was immediately performed. When the abdomen was opened the uterus presented a remarkable sight, it was of a deep purple colour and had the appearance of an ovarian cyst with a twisted pedicle. There were hæmorrhages in both broad ligaments, also beneath the pelvic peritoneum, including the peritoneum covering the bladder, which also looked œdematous. There was no free fluid or blood in the peritoneal cavity. Upon opening the uterus a large quantity of blood and amniotic fluid gushed out and a freshly dead full-time fœtus, weighing 6 lb., was extracted by the feet, followed by a completely detached placenta. A large amount of fluid and coagulated blood then escaped, estimated to be about a pint and a half.

A subtotal hysterectomy was performed with conservation of the right tube and ovary. An intravenous injection of gum saline was given on the table. At the completion of the operation the pulse-rate was 96: temperature 98, blood-pressure 180-60.

*Progress.*—Apart from developing lobar pneumonia on the third day, which subsided on the ninth, the convalescence was uneventful and the patient was discharged on the twenty-fifth day. She remained rather weak and pale but it was not found necessary to do a blood transfusion. The discoloured patches on the vagina had disappeared on the twenty-third day.

#### *Laboratory Examinations.*

May 2, 1923.—Catheter specimen of urine: Albumin, 2·5 per cent.; no globulin detected; N as  $\text{NH}_3$ , = 7 per cent.; urea, 2·5 per cent.; no sugar. The sediment consisted of leucocytes, a few red corpuscles, squamous and transitional epithelial cells; also streptococci, diphtheroid bacilli and coliform bacilli.

May 21.—N as  $\text{NH}_3$  = 6·6 per cent.; urea, 2·1 per cent. Hæmoclastic crisis: (1) 5,800 white blood cells; (2) 9,000 white blood cells. Blood urea, 0·19 mgr. per c.c. Diastase, No. 10; Wassermann reaction negative.

October 20.—No albumin; no casts; urea, 1 per cent. Only squamous and epithelial cells seen in sediment.

*Pathological examination.*—The placenta was normal in size and appearance; weighed 1 lb. 2 oz.; length of cord, 21½ in. The uterus was purplish in colour, showing numerous hæmorrhagic patches beneath the peritoneum, most marked on the posterior wall near the fundus on the left side. The left tube, ovary and broad ligament were removed with the uterus. The ovary appeared free: the tube was infiltrated with blood, so was the broad ligament. The insertion of the placenta was on the posterior wall and fundus of the uterus, well above the lower uterine segment.

*Report by Professor Haswell Wilson.*—"I have examined sections from the patient, B. T., and have to report as follows:—

"Diffuse hæmorrhage has occurred over a considerable area of the uterine wall. Sections stained in the ordinary way with hæmatoxylin and eosin show no gross changes other than the hæmorrhage referred to. Frozen sections, however, stained by Sudan III reveal the presence of fatty degeneration in the walls of many capillaries, especially those in the area where hæmorrhages have occurred. In some of these the outlines of the vessels are almost obscured by the accumulation of fat globules. No change other than the appearance of slight hyaline degeneration is found in the larger vessels. Some œdema is present between the muscle bundles outside the hæmorrhagic area.

"The appearances suggest that the hæmorrhage has resulted from multiple ruptures in capillary vessels and the fatty changes above described would appear to be an adequate explanation of this occurrence."

#### COMMENTS.

The association of albuminuria with accidental hæmorrhage is stated to occur in 40 to 50 per cent. of cases (Gordon Ley [1]), but I cannot find any reference by British or American writers to this association with hæmatoma

of the vulva. The literature on the subject is scanty and mostly in German, and in a brief search through the German articles two writers only were found to mention albuminuria associated with this condition.

Hirsch [2] records two cases, one of eclampsia and one of nephritis of pregnancy; Liepmann [3] also notes two, both eclampsia. Ch. Frank-Kamenetzky [4] collected 124 published cases of hæmatoma of the vulva, 82 puerperal and 42 traumatic. Of the puerperal cases noted over 50 per cent. were in primiparæ. The hæmatoma appeared in four cases between the birth of twins, in twenty cases during labour but before delivery, and in fifty-three cases after the birth of the child. Spontaneous rupture took place in forty-three cases. The mortality was 9·5 per cent., including one death from hemiplegia. No reference was made to albuminuria.

It is interesting to note that in both my own cases (Cases I and II) the patients had well-marked albuminuria, both were primiparæ and in neither instance was there any evidence of varicose veins or trauma. There was no history of pre-existing renal disease or arterio-sclerosis but in each case there was a slight trace of albumin subsequently.

The blood-pressure was distinctly high in one case, but in the other, although within normal limits for a healthy person, it might be considered to be relatively increased in anyone under the condition of profound shock.

Mrs. E. C. gave a history of symptoms suggestive of some form of toxæmia, whilst in the case of Mrs. L. E. E. there was œdema of the feet and a slight trace of albumin, fourteen days before delivery.

The time of onset of the bleeding is noteworthy, for in one case it came on fifteen hours after parturition, while in the other, it actually obstructed labour, and in both trauma could be excluded. In this connexion, it might be of interest from the medico-legal point of view. It is quite conceivable that an action for malpraxis might be brought against the doctor for using undue force; and it would be a strong point in evidence in a court of law if the toxæmic nature of these cases could be established.

The remaining case of "concealed and evident accidental hæmorrhage" exhibited a great similarity to the condition described by Couvelaire [5] as "utero-placental apoplexy" and subsequently reported very fully by Whitridge Williams [6] and others. I could not, however, find any reference by them to the brownish-red patches which were noted in this case or to the fatty changes in the small capillaries seen in the frozen sections stained with Sudan III.

Mallory [7] states that injury to blood-vessels may be caused by toxins and that the presence of fat in the different cells forming the walls of the blood-vessels is the best guide we have to functional derangement of those cells. Also Kaufmann [8] makes a strong point of fatty degeneration being the most important degenerative change affecting capillaries. It is especially associated with toxæmic states and may result in hæmorrhages of considerable degree owing to rupture of the vessel walls. He specifically mentions the uterus as an organ in which this is likely to occur.

Adami [9] refers to fatty degeneration of the capillaries in connexion with circulatory toxins and states that this condition may result in an actual rupture of the blood-vessels, with hæmorrhage into such organs as the brain or lungs.

I am, so far, unable to find any reference to the occurrence of fatty changes in the capillaries of the uterus during involution. It is, of course, well known that the large vessels may show hyaline changes and fat globules may appear in the muscle fibres during this process.

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There seems to be an analogy between the hæmorrhages occurring in the uterine wall and sub-peritoneal tissues, and the hæmatomata of the vulva. The former are now generally accepted as being toxæmic in origin: may not a similar factor be responsible for some of the cases of vulval hæmatoma, as in two of the cases here recorded?

In conclusion, I should like to thank Mr. Beckwith Whitehouse for his kind help and permission to use one of his cases, also Professor Haswell Wilson for his valuable suggestions and for his report on the sections.

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## The Relation between certain Abnormal States of the Female Generative Organs and Symptoms Ordinarily Considered the Province of the Physician.

By H. MORELAND MCCREA, M.D., and VICTOR BONNEY, M.S.

### (ABSTRACT.)<sup>1</sup>

THE authors pointed out that in the past gynæcologists were physicians and laid much stress on certain general symptoms which they regarded as reflex, or sympathetic to disease of the genital tract.

Gynæcology was now a branch of surgery, and it had become the fashion to discredit these collateral affections. The older gynæcologists, however, were not wrong, for abnormalities of the genital organs did, in certain instances, affect the general health by causing toxic or bacterial absorption, and the symptoms so produced did not at first sight in many cases suggest a gynæcological origin.

The authors described a class of case in which intestinal stasis was primarily dependent on displacement of the pelvic floor and uterus. The mechanism of defæcation in women was closely considered. Three factors were concerned: (1) The contraction of the bowel wall; (2) the intra-abdominal pressure which acted on the rectum by the medium of Douglas's pouch which formed a pneumatic wedge compressing the rectum against the sacrum; and (3) the muscular pelvic floor which made counter-pressure against the diaphragm and abdominal muscles during the act of straining and the posterior fibres of which pulled the anus over the advancing fæcal mass. When any of these factors became inoperative the onset of constipation and stasis was favoured.

The condition was a chronic one and very commonly symptoms local to the pelvis, if they existed, were overlaid and obscured by the symptoms due to the inert distended bowel and the subsequent toxæmia. This class of patient consulted a physician and not a gynæcologist, so that unless the physician was able and willing to make an intelligent vaginal examination the primary cause of the symptoms was very apt to be overlooked. Stasis was far commoner in

<sup>1</sup> This paper is published in full in the *Lancet*, November 10, 1923, p. 1025.

women than in men, a fact at once suggesting that the peculiarities of the female pelvis were on occasions causative.

The authors then dealt with cases in which absorption directly resulted from genital disease. Chronic streptococcal infection of the cervix might produce arthritis, fibrositis, myositis and similar generalized conditions which were in danger of being treated as primary entities unless a thorough vaginal examination was made. Genital infection was often associated with infection of the urine, the latter being a secondary result, and unless the genital canal was treated it was impossible permanently to purge the urine of organisms. Affections of the eye like choroiditis and iridocyclitis might even have a genital origin, and they instanced cases in which cure or arrest of the disease had followed treatment of the genital infection.

#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) said that Dr. McCrea and Mr. Victor Bonney had introduced a subject which would be sure to lead to a useful discussion. In stating that "the study of medicine in compartments is cramping to the mental outlook," the authors sounded a note which was familiar to the Fellows of the Royal Society of Medicine. The recent establishment of a new Section of Comparative Medicine was an acknowledgment of the fact that the human subject formed too narrow a sphere to include the whole realm of medical science, and the idea of team-work in human medicine doubtless arose with a view to obviate the inherent dangers of specialism. From the start, then, no doubt the sympathy of the meeting would be with the writers in their endeavour to press home, in this Section, the need of falling into line with the spirit of the age.

When the writers stated that there seemed to be a gulf at present between the general physician and the gynæcologist, and that this was due on the one hand to the fact that the physician did not make a vaginal examination, and on the other to the supposition that the knowledge of the gynæcologist ended at the brim of the pelvis, they were surely carrying their point to the limits of exaggeration, at any rate he himself (the President) whilst admitting his limitations, would be sorry to find himself included in the latter category.

In regarding the paper with a view to discussion, the points raised fell under three headings: (1) The clinical states which in the writers' view, were due to a relaxed and injured pelvic floor; (2) the effects on the body generally of infection of the genito-urinary tract; (3) the general results of pressure within the pelvis.

To prevent the discussion becoming discursive the President suggested that it should follow these subdivisions, which would mean considering first the mechanical results of injury to the pelvic floor; these, as stated by the authors, were intestinal stasis, dilatation and prolapse of the rectum and pelvic colon, general enteroptosis, and so on. In this connexion Mr. Bonney's work on the action of the pelvic muscles and the part played by the normal pouch of Douglas in the act of defæcation were particularly helpful, but in thanking him for his anatomical data and mechanical theories one must reserve the right to criticize his conclusions. It was difficult to see how the supposed action of the pneumatic wedge, if lost by the pressure of a retroverted uterus or by pelvic tumour, would normally act when the pouch of Douglas was filled by coils of small intestine. That mutilation of the pelvic floor produced by an extended operation for cervical cancer led to difficulties in defæcation he (the President) was ready to admit. The treatment of enteroptosis by raising and suspension of the uterus seemed very unconvincing, so also was the employment of the same operation to relieve chronic diarrhœa.

Dr. ANDREWS said that the authors' communication was both interesting and suggestive. Papers linking up gynæcology with general medicine were to be welcomed, as there was always a danger of specialists working too much in water-tight compartments. Just as every gynæcologist ought to serve a term as house physician and house surgeon, so ought every general physician, and still more every general surgeon,



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to have acted as resident officer in obstetrics and gynecology. He fully agreed with a good deal that the authors said on the subject of the bad effect that might be produced on the general health by extensive damage to the pelvic floor and its resulting displacements, and of the benefit experienced after repair of the pelvic floor, but he could not follow them so whole-heartedly in their remarks about uncomplicated retroversion of the uterus as a cause of intestinal stasis. The uterus of normal size was not large enough to do away with the pneumatic wedge, if the pelvic floor was sound, and he thought that chronic constipation in a nulliparous woman with retroversion of the uterus could usually be cured without abdominal section. The effect of drinking a pint and a half or two pints of fluid beyond the amount habitually drunk, together with colonic massage done by the patient herself and bedroom exercises, would often bring about a cure. To his mind there were too many operations already performed for retroversion, and he would be sorry to see any addition to the so-called indications for operative treatment of this common and often unimportant deviation from the normal. Scybala were often mentioned in the paper. It was surely simpler to attempt to soften them than to perform operations to facilitate their passage. He found it difficult to believe that infection due to residual urine in cases of cystocele was common. Had the authors proof, by passage of a catheter after the patient had done her best to empty her bladder, that residual urine was of frequent occurrence in cases of cystocele ? If so, this evidence was important,

Sir GEORGE BLACKER said that he could not agree with most of the views put forward in this interesting but unconvincing paper. Surely the commonest factor in producing constipation in the female was the failure to drink a sufficient amount of fluid rather than any action on the part of the retroflexed uterus. He could not believe that this had any effect whatever. It certainly could not press upon the rectum and when unenlarged its weight was negligible. He did not think that any accurate or useful deductions could be drawn as to the action of the so-called pneumatic wedge by the condition present when the abdomen had been opened and the patient was under an anæsthetic, nor did Mr. Bonney's description of its supposed action carry any conviction to his mind. He had little belief in septic absorption occurring through the vagina, as it was only modified skin, and he knew of no proof that such septic absorption did occur from discharges derived from the cervix. The finding of organisms in the cervical mucosa and the proof that they played any part in the production of disease due to septic absorption were two different things ; many of these organisms were non-pathogenic and harmless. It would appear that the authors of the paper wished to revive some of the pathological teaching of fifty or more years ago and for his part he could not accept it.

Mr. ERNEST CLARKE said that a patient of his had been referred to in the paper. It was the worst case of so-called "hæmorrhagic retinitis" that he had seen for some time ; the whole fundus was covered by superficial hæmorrhages, and the papilla was swollen, and the area round it cedematous. Every possible cause was eliminated and he finally asked Dr. McCrea to see the patient and ferret out the cause. Dr. McCrea suspected some form of poisoning, and Dr. Carnegie Dickson was asked to isolate some toxin, if present. He was able to isolate streptococci as the cause, and, after vaccinations spread over a period of four or five months, the patient completely recovered, and at the present time there was not even a scar of the old trouble. He (Mr. Ernest Clarke) had never seen such a complete recovery from so serious a condition. There was no doubt it was not only the diagnosis, but the *early* diagnosis that had proved so valuable to the patient. He thought it emphasized the remarks made by the President about the value of team work.

Dr. F. W. COLLINGWOOD said that, in his experience in Poor Law institutions and elsewhere, patients with chronic pelvic conditions did not often suffer from arthritic trouble. On the other hand, pyorrhœa was commonly associated with arthritis. It was suggested that after extraction of the teeth the arthritis might persist from intestinal infection owing to the micro-organisms which had caused the pyorrhœa having been swallowed. In constipation, so frequently seen in chronic pelvic conditions, an ideal condition existed for the multiplication of intestinal flora, including the recently introduced species.

Dr. CARNEGIE DICKSON referred to a case in point—a married lady who complained of ear trouble, found by the aural surgeon to be "toxic," and traced by Dr. Moreland McCrea to a suppurating dermoid. He (Dr. Carnegie Dickson) then emphasized the importance of the bacteriological examination of cervical discharge, and also of very carefully taken catheter specimens of urine for the presence of streptococci (as well as for that of *Bacillus coli* and other organisms); for example, in rheumatic and rheumatoid cases. He also pointed out the ultimate success of vaccine treatment in these cases.

Dr. MCCREA (in reply) stated that, judging from the criticisms made by speakers, it was evident that he had conveyed an entirely wrong impression. It was not his intention to suggest that a weakened pelvic floor was the only cause of stasis or that an enlarged retroflexed uterus was the only cause of constipation in young girls; or that the urine of all patients suffering from cystocele was infected, but rather he wished to draw the attention of the Section to the fact that these different pelvic conditions did on occasion cause medical disorders elsewhere.

In reply to Dr. Andrews, who asked for proof that a cystocele frequently contained residual urine he had himself tested this and found as much as 2 oz. of urine obtained by catheter after the voluntary act of micturition had been accomplished. It should be noted that it was common to find at the end of twenty-four hours that the plates showed no growth and it was well in these cases to carry on incubation up to ten days, as frequently streptococci did not become visible till the fourth or fifth day and pyocyanus might not appear for ten days. He did not agree with the speaker whose contention it was that before organisms could do any material harm pus must be present; that was not his experience at all.

Mr. VICTOR BONNEY (in reply) pointed out that he and Dr. McCrea did not allege that *all* cases of intestinal stasis in women were dependent on genital displacement, or that the patients in *all* cases of displacement suffered from stasis. They merely desired to call attention to a class of case in which such displacement appeared to be the only cause of the condition and in which operative cure of the displacement had cured the stasis. It was fruitless to enter into a discussion on the results of retroversion. His opinions were diametrically opposed to those of certain of the speakers and there was no hope of an agreement being reached. His (Mr. Bonney's) description of the mechanics of defæcation had been criticized, on the ground that observations on the opened abdomen did not apply to the closed abdomen. But radiology showed that during the act of straining, the intestines, and especially the colon, were driven downwards by the descent of the diaphragm whilst the protrusion of bowel into a hernial sac was a matter of everyday observation. The utero-rectal pouch in women (and the vesico-rectal pouch in men) acted during defæcation exactly like a hernial sac.



## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### Pedunculated Endothelioma of the Vulva.

By T. G. STEVENS, F.R.C.S.

THIS small tumour was removed from a female patient aged 46, who said that it had been slowly growing for about twenty years. It was attached by a broad, thin pedicle to the right labium majus about its centre. It had the appearance of a simple fibroma, but it was rather harder than usual and had a lobulated feeling, as if it was a tumour covered by skin and fat. Its macroscopic appearance was curious when cut into, as it consisted of a number of hard oval or rounded masses enclosed in a capsule of fat and connective tissue. This was quite unlike any fibroma or lipoma. Microscopically one of these hard areas shows a fibrillated stroma enclosing numbers of strings and masses of cell-elements of the endothelial type, in places surrounding small lumina. The latter, more often empty, sometimes contain a yellowish structureless material. From the narrow, elongated character of many of these cell masses I am inclined to think that they are really capillary vessels surrounded by proliferating endothelial cells derived from the perivascular lymphatics. The growth in that case ought to be called an endothelioma of the perithelial type. More often endotheliomata of mucous glands, of which that in the parotid is the commonest, are derived from the peri-acinous lymphatics. It is, however, well known that when endotheliomata have existed a long time, it is not always possible to say with certainty in which of these ways they have originated.

I am not able to prove that this growth originated in Bartholin's gland, but I have good reason to believe that this happened, because of its situation, its close resemblance to the common parotid endothelioma, the absence of any other structure in that situation from which it would be likely to arise, and because it is quite unlike the endotheliomata of the skin.

#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) said he placed a section of endothelioma of Bartholin's gland under a microscope for comparison with Mr. T. G. Stevens' specimen. The two growths were very dissimilar in their histological features. In his (Dr. Lockyer's) section the endothelial cells were arranged in a reticular network within alveoli. In Mr. Stevens' specimen there was no reticular massing of cells but a clumped arrangement of solid masses of cells such as is seen in a rodent neoplasm. In each specimen, however, the cells had their origin in the endothelium of the capillaries.

Dr. HERBERT SPENCER said he hoped Mr. Stevens' case would not be recorded as a case of growth in Bartholin's gland, of which there was no evidence whatever of its being, except that it occurred in approximately the same part of the vulva.

### **Small Ovarian Cyst showing Papilliferous Growths in an Early Stage.**

By T. G. STEVENS, F.R.C.S.

MRS. S., aged 29. October 22, 1923. Complained of leucorrhœa of two years' duration and severe dyspareunia extending over the same time. Had been married seven years, but had never been pregnant. The menstrual periods were regular, but very scanty, lasting only one day for the last three years. No pain at these times.

On examination the uterus was found to be small and anteflexed, and the right ovary was felt to be enlarged, prolapsed and very tender. The left ovary was enlarged but not to the same extent as the right.

Exploratory laparotomy was advised and performed. The right ovary was adherent in Douglas's pouch and is the one which is the subject of this communication. The left ovary was also adherent and cystic. The right ovary and tube were removed, the cystic portion of the left was resected and the remains of the ovary sutured. On section the right ovary proved to be cystic, containing two or more cavities, which apparently had broken down into one another, leaving remnants of septa here and there, some of which took the form of delicate strings very closely resembling chordæ tendinæ in a foetal heart. The fluid was opalescent, yellow in colour and contained cholesterin, but no obvious fat or pseudomucin. There are two solid yellow masses near the hilum of the ovary, one of which is close to the abdominal ostium of the tube and measures three-quarters of an inch in diameter. The other, rather smaller, is at the opposite pole but both appear to encroach on the mesosalpinx, which appears to be shortened as if it had been opened up by these growths projecting into its folds. These two growths are exactly alike in structure and are really two cystic cavities completely filled by branching papillary growths, covered by short columnar epithelium. The cavities are somewhat irregular in outline and in places show diverticula pushing into the adjacent ovarian stroma.

These growths, therefore, have the structure of a very early stage of papilliferous ovarian cysts, and although there is no actual evidence of malignant infiltration, from the actively proliferating intra-cystic growths it would appear that they must at least be regarded as potentially malignant.

The other cysts also contain here and there small papillary ingrowths, which are rather suggestive of implantations from one or other of the two main growths. As far as my investigations go at present there is nothing to be found to suggest the origin of these growths, but bearing in mind Goodall's work on the origin of ovarian cysts in general, it would seem that they may be derived from a persistence of that part of the foetal ovarian tubular system which is known as the rete ovarii. They are developed in this case strictly in relation to the hilum of the ovary, a point which has always been insisted upon for the true papilliferous ovarian cyst.

The main cysts are lined by short columnar epithelium, quite unlike that which lines a pseudomucinous cyst, and also unlike the flattened lining of simple enlarged Graafian follicles. The origin of these cysts is quite obscure, but they also may be papilliferous cysts, or on the other hand cysts derived from some other persistent part of the ovarian tubular system which has become secondarily infected with papillary growths.

Dr. HERBERT SPENCER said that this case was interesting, especially in regard to the future of the resected ovary, the other ovary being malignant. He (Dr. Spencer) had removed one ovary for papilloma, leaving the other. Five years later he had to remove a large cancerous tumour of the other ovary, together with 22 in. of small intestine, and afterwards the patient's breast was removed for cancer, although the patient remained well three years later. He thought it was generally wise to remove the other ovary when one was affected with papilloma and still more so when it was carcinomatous, though he and others had published cases in which the patients remained well for many years after unilateral ovariectomy for cancer.

### Carcinoma of the Fallopian Tube.

By J. BRIGHT BANISTER, M.D.

A. W., AGED 47, married nullipara, was admitted to hospital on September 13, 1922, complaining of a swelling in the lower abdomen, which she had noticed for about two years. Recently this had become painful, particularly during the periods, which occurred regularly at twenty-one days' interval with great loss.

There had been no loss of weight. Constipation was marked and there had been definite frequency of micturition for some months. *Examination of the abdomen* revealed a very hard, rounded tumour reaching 2 in. above the symphysis, very slightly movable and not tender.

*On vaginal examination* the cervix appeared normal, the body of the uterus was continuous with the abdominal tumour, while on the right a second tumour was felt about the size of an apple and also very hard.

*Operation, September 14, 1922.*—The bladder was displaced upwards, the uterus was about the size of a 4½ months pregnancy and the right Fallopian tube was much enlarged. Adhesions were widespread. Subtotal hysterectomy with removal of both tubes and ovaries was done.

Subsequent progress was uneventful. The patient was seen in November, 1923, and pelvic examination failed to reveal any abnormality.

*Pathological Report on a specimen removed September 14, 1922.*—Uterus 11 by 8 by 11½ cm., removed by subtotal hysterectomy, together with the appendages of both sides. The peritoneal surface is smooth. On section the wall of the uterus contains multiple interstitial and submucous fibromyomata, the largest 7.7 cm. in diameter. The cervical epithelium shows a polypoid proliferation. The left Fallopian tube and ovary are normal. The right Fallopian tube is coiled and greatly distended, up to 4 cm. in diameter, and is closely adherent to the ovary, which is enlarged. On section the tube is found to contain blood-stained fluid and large cauliflower-like masses of white, granular growth. The growth has protruded from the surface in the region of the abdominal ostium and invaded the ovary. Sections show the growth to be a columnar-celled, papillary carcinoma of the Fallopian tube with invasion of the ovary. Professor Shattock has confirmed the diagnosis.

Dr. CUTHBERT LOCKYER (President) remarked that the literature of carcinoma tubæ had already attained considerable dimensions since the publication in 1888 of the two first cases—one by Orthmann and the other by Alban Doran, a past President of this Section. In 1910 Doran published 100 cases in the *Journal of Obstetrics of the British Empire*. In 1914 Cecil Vest, of Baltimore, brought the number up to 132. By 1916 the number had reached 138 and included those last shown at this Section by Dr. Herbert Spencer (fourth case), Dr. Russell Andrews (second case) and Lady Barrett's case. Mr.

## 32 Banister: *Carcinoma of Fallopian Tube; Cæsarean Scar*

Banister's case presented the features of a diffuse adeno-carcinoma growing from the mucosa of the tube. It was in no sense a papilloma which had become malignant. Of the two well recognized types, malignant papilloma (papillary carcinoma) and adeno-carcinoma, the latter was by far the rarer. Histologically this rare type of tubal cancer varied within wide limits from a tubular structure to a diffuse spheroidal-celled growth, so much so that much confusion had arisen in classification and many cases were described as "mixed tumours," the authors being at their wits' end how to classify them. Hence the admission that sarcoma and carcinoma appeared to be concomitant. In fact malignant tubal growths transgressed by metaplasia our arbitrary classification based on derivation from mesoblast and epiblast. For the purpose of nomenclature it seemed best to be guided by the predominant features only, subsidiary data might then find mention in the text.

Much interest had centred around the connexion between salpingitis and tubal cancer. A close study of the epithelial changes which occurred in salpingitis nodosa had convinced him (the President) that the displaced epithelium could be found to demonstrate clearly precancerous changes, salpingitis and carcinoma of the tube thus being linked up. He had been able to demonstrate this close association at this Section in May, 1916, at the time when Dr. Herbert Spencer, Dr. Russell Andrews and Lady Barrett showed their cases of malignant Fallopian tubes.<sup>1</sup> On that occasion it was noteworthy that one case (Lady Barrett's) showed both epithelioma and caseating tubercle side by side.

### Ruptured Cæsarean Section Scar.

By J. BRIGHT BANISTER, M.D.

F. B., AGED 27, married, pregnant for second time, was admitted to Queen Charlotte's Hospital on January 12, 1923, in labour.

On May 2, 1921, her first pregnancy had been terminated by Cæsarean section, owing to obstructed labour. The puerperium was complicated by severe sepsis, with fever lasting eighteen days. The second pregnancy was uneventful, and on admission she was in labour, the head being fixed in the brim and progress apparently normal. At 5.30 p.m. she suddenly became acutely shocked, and on examination the foetal head was found floating and the whole abdomen very tender. Diagnosis of ruptured Cæsarean section scar was made and laparotomy was decided upon.

The child and placenta were removed. The sides of the rent being found to be lying very everted, it was considered wiser, in view of the patient's dire condition, to remove the uterus.

The patient's subsequent progress was good.

The first Cæsarean scar was sutured with silk in three layers, but no trace of these could be found. I presume that there had been sepsis in the uterine wound following the operation, and that all stitches had been passed *per vaginam*.

*Pathological Report on a Specimen removed January 24, 1923.*—A puerperal uterus 15 by 15 by 8 cm. removed by subtotal hysterectomy, together with the appendages of both sides.

In the anterior wall there is a ragged L-shaped tear involving the whole thickness of the uterine wall. The vertical limb, lying to the left of the middle line, extends to the fundus and is 10 cm. long; the horizontal limb, 4 cm. in length, extends towards the right beyond the middle line.

To the edges of the tear are adherent some recent blood-clot and some old peritoneal tags; the sides of the uterus slope towards the tear as though

there had been a gutter-scar before the rupture took place. The decidua lining appears to extend over nearly the whole face of the breach in the uterine wall, the actual torn surface being very shallow in depth and represented by the peritoneal tags and thin scar tissue. The placental site is on the posterior wall. Sections taken across the face of the tear show the decidua lining the greater part of this area.

Mr. EARDLEY HOLLAND pointed out that in 80 per cent. of reported cases of rupture of the Cæsarean section scar the rupture had occurred either during pregnancy or early in the first stage of labour. It seemed apparent that the scar was more liable to be ruptured by the bursting-stress of the general intra-uterine pressure than by the direct uterine pressure. It therefore seemed to him that to rupture the membranes artificially directly labour began in patients who had had a former Cæsarean section would be a useful prophylactic measure against rupture of the scar, and he advised that this be done in all cases.

### **A Case of Cyst Development in an Ovarian Graft.**

By VICTOR J. LACK, F.R.C.S.Ed.

CYSTIC changes in ovarian grafts appear to be rare, for, although several authors mention the risk of cystic degeneration as a possible objection to ovarian grafts, in the literature on the subject I can only find two cases specifically mentioned, one recorded by Graves of Boston [2] in which three subsequent operations had to be performed for the removal of cysts, and one by Blair Bell [1]. The latter author has had one other additional case, which, as far as I can discover, is at present unpublished. I shall refer to this later.

*Notes on the case, which occurred recently at the London Hospital:—*

W. E., a single girl of 20, was admitted to the medical wards of the London Hospital on October 14, 1922, suffering from ascites, and a tentative diagnosis of tuberculous peritonitis was made.

The abdomen was tapped on two occasions (October 17 and November 7), when ten and nine pints respectively of clear viscid yellow fluid were removed.

After the paracentesis, an abdominal tumour was discovered, and she was transferred to the Gynæcological Department under Dr. Russell Andrews on November 26, 1922.

At this time a cyst, about the size of an adult head, was palpable in the abdomen. There was still much ascites present. The girth was 36 in. at the umbilicus. It was decided to perform an exploratory laparotomy.

*November 28, 1922. Operation. Laparotomy* (Dr. Russell Andrews).—Mid-line incision. A large amount of free fluid escaped, estimated at 10 to 15 pints.

A right-sided ovarian tumour, about the size of an adult head, was revealed. It was partly cystic and partly solid. On the left side the ovary was enlarged to about the size of a tangerine orange and was cystic.

On account of the obviously malignant appearance of the right-sided tumour, bilateral ovariectomy was performed.

No secondary nodules were seen on the peritoneum or in the omentum. A portion of apparently healthy ovarian tissue, about the size of a bean, was discovered in the left-sided cyst. The greater part of this was fixed by one stitch to the fascia behind the right rectus muscle during the process of closing the abdominal wound. A much smaller piece was dropped in the wound behind the left rectus muscle. The wound healed by first intention. Convalescence was uneventful.

*Description of Specimen.*—(Pathological Institute, S.D. 2205, 1922.)—(1) The specimen is a cyst, measuring 18 by 16 by 12 cm. Attached to the cyst is the terminal



### 34 Lack: *Case of Cyst Development in an Ovarian Graft*

6 cm. of the right Fallopian tube. The surface of the cyst is covered with dried blood. In addition to this, a small quantity of fibrin is adherent to the surface over an area measuring about 11 by 10 cm. In the centre of this area the cyst is perforated. The inner wall of the cyst is lined by a mass of growth arranged in ridges, plaques and fringes; the cut surface of this growth is moist and shows white, granular tissue arranged in papillæ upon cores of grey tissue. (2) A portion of an ovary, which measures 4.5 by 3.5 by 1.5 cm., and is attached to the distal 4 cm. of the left Fallopian tube. In the ovary are several unilocular cysts, the largest of which measures 1.5 cm. in diameter.

*Microscopic Diagnosis.*—(1) Intra-tubular and intra-cystic papillary, columnar-celled, mucous carcinoma of right ovary. (2) Follicular cysts in left ovary.

The patient was next seen:—

May 17, 1923, when she stated that she had remained well but had not menstruated since the operation.

June 15, 1923.—A similar note.

In July, 1923, she had a slight bleeding for two days, and twenty-eight days later, in August, had a normal menstrual period lasting four to five days. Fourteen days later she began to bleed again and continued to do so for seven days. The amount lost was slightly excessive.

Between this time and her readmission to hospital, she had two further periods of bleeding with only a short interval of freedom. The amount lost each time was considerable and the feeling of lassitude caused thereby was probably the reason for her seeking medical advice.

October 21, 1923.—She was readmitted to hospital complaining of five weeks' menorrhagia (as above) and slight abdominal discomfort; she stated that her own doctor "had noticed a small swelling in her abdomen."

*Examination.*—General condition good. There was a small, rather firm, cystic swelling in the abdominal wall, about 4 cm. long by 2.5 cm. wide, situated 3 cm. from the operation scar in the mid-line and about equidistant from the symphysis pubis and the umbilicus. It was fixed to the muscle and was not tender. With this exception the abdomen presented no abnormal features.

Vaginal examination showed the uterus to be normal in size and position, and failed to reveal any evidence of secondary deposits in the pelvis.

On account of the menorrhagia and the possibility of malignant changes in the graft, it was decided to explore.

October 23, 1923.—Exploratory operation (Dr. Russell Andrews): A small incision was made over the swelling which was found to consist of two small cysts, one about twice the size of the other. The larger was filled with brownish, clear serous fluid, the smaller with pale yellow, clear fluid. The mass was excised. The wound healed by first intention.

*Description of Specimen.*—(Pathological Institute, S. D. 2163, 1923.) The specimen is a portion of tissue which measures 2 by 1.5 by 0.9 cm., and is partly fixed by formaldehyde solution. One surface is in part smooth and grey, in part wrinkled, yellowish-grey and glistening. The other surface is occupied by ragged, pinkish-grey and brown tissue, which suggests muscle, and by a cyst which is 0.8 cm. in diameter and has been opened. On section, two cysts, both of which have been opened, are seen to lie upon a layer of tissue which measures 2 by 0.8 cm. and resemble muscle and fibrous tissue. The cysts measure about 0.8 and 0.5 cm. in diameter respectively. One has a yellow lining.

*Microscopic Examination.*—In a dense fibrous tissue, which passes into tendon and into fibrotic, voluntary muscle, lie two large, collapsed, cystic cavities, surrounded by a zone of ovarian stroma, and, at a considerable distance from these, lies a small isolated area of ovarian stroma. In one section the ovarian stroma surrounding the cysts contains two ova, each of which is surrounded by a single layer of cubical cells. In another section a third ovum occupies a position close to these two. One of the two cysts is lined by cells of the stratum granulosum. These cells in one small area are spherical, 6 to 8  $\mu$  in diameter, and arranged in several layers. Over part of the surface they are cubical, slightly larger and arranged in a single layer. In other areas they are

round, swollen to a diameter of 10 to 12  $\mu$  and arranged in one to six layers. The cytoplasm of these swollen cells is abundant, faintly stained and finely granular; it resembles that of lutein cells, but is, usually, stained a little more deeply. In the theca interna interstitial cells are very abundant. They form a zone which is very seldom interrupted and is from four to eight cells deep. This zone is sometimes separated from the stratum granulosum by a line of collagenous fibre and spindle fibroblasts, but it usually appears to lie in contact with the cells of the stratum granulosum. The cells are from 8 to 12  $\mu$  in diameter. Their cytoplasm is slightly less deeply stained than that of the swollen cells of the stratum granulosum, and, thus, resembles more closely the cytoplasm of lutein cells. The other cyst seldom shows any lining. Where the lining is present, it consists either of a single row of cubical cells or of clumps of swollen, pale spherical cells. The theca interna is fibrotic and deepened. The interstitial cells are small and scanty and lie in small groups in the outer part of the theca interna.

Both these cysts are evidently follicular cysts. In the cyst which showed a yellow lining to the naked eye, the interstitial cells of the theca interna are abnormally numerous and large. Further, many of the cells of the stratum granulosum are swollen and apparently fatty. These changes resemble closely changes which occur in the formation of corpora lutea spuria, except that the organization of a central mass of fibrin is absent. In these respects this cyst contrasts with the follicular cysts present in the portion of the ovary from which the graft was taken and with a series of follicular cysts examined as controls.

The graft contains, therefore, two ova in primitive follicles and two follicular cysts. In one of the follicular cysts there are changes which suggest a modified lutein reaction.

In considering this case I should like to draw attention to the following points: (1) *Menorrhagia* appears to accompany this condition with some regularity. In a letter to me, Professor Blair Bell states:

"In some 150 cases of ovarian grafts which I have performed personally, I have only twice come across cystic developments subsequently. In both cases cysts were removed because they were associated with menorrhagia which ceased after removal of the cysts from the grafts and the patients continued to menstruate normally. Without looking up the sections, and this I am too busy to do at the moment, I am under the impression that the cysts were follicular and lined with membrana granulosa cells and not with lutein cells. However, there is no reason why lutein cells should not be present, for one finds lutein cysts in the ovary in connexion with follicles which have been unable to rupture owing to adhesions. In these circumstances, the cysts generally contain blood-stained fluid. They are not very common, but I have seen and described them."

In the case quoted by Graves hysterectomy was performed.

Blair Bell records two other cases in which swelling of the grafts occurred and in each case this was accompanied by menorrhagia.

(2) The regularity of this symptom offers fresh scope for speculation as to the relation of ovulation to menstruation. For one might suggest as a hypothesis that in a normal Graafian follicle the interstitial cells secrete into the follicle the hormone determining menstruation and that the rate of absorption is roughly equal to the tension within the cyst, the stimulation to menstruation increasing either up to or just beyond the time of rupture. If the follicular cyst develops in an abnormal position, as in an ovarian graft, rupture is unable to occur and so absorption is likely to go on for a prolonged length of time, thus giving rise to menorrhagia.

(3) I can find no case recorded in the literature in which malignant changes have occurred in an ovarian graft. The possibility of this occurrence, however, remains and one can see no sound reason against it. In the case quoted, one could not be sure that there were no carcinoma cells secondarily implanted in

that part of the ovary used as a graft and this risk was one of the reasons for exploration.

(4) This case confirms the general findings on the results of ovarian grafts by Blair Bell, Tuffier and others as collected and analysed by Martin [3], namely, (a) that small grafts are more likely to be successful than larger grafts, and, (b) that ovarian grafts are capable in some cases, if sufficient endometrium be present and the patient of suitable age, of stimulating the uterus to menstruation for a certain length of time, and that this function is usually re-established about seven months after the implantation of the graft.

I am greatly indebted to Dr. H. Russell Andrews, under whose care the patient was, for permission to publish this case and for his help and advice; to Professor Blair Bell for allowing me to mention his cases and for his assistance in giving me several references to the literature, and to Professor Turnbull, for permission to publish his reports upon the tissue removed by operation.

#### REFERENCES.

- [1] BLAIR BELL, W., *Lancet*, 1920, ii, pp. 879-84. [2] GRAVES, W. P., *Surg., Gyn., and Obstet.*, 1915, xxv, p. 321. [3] MARTIN, F. H., *Surg., Gyn., and Obstet.*, 1922, xxv.

Dr. CUTHBERT LOCKYER (President) stated that in July, 1919, he removed the entire uterus together with two very adherent tubo-ovarian masses in the case of a multipara aged 35 years. A wedge-shaped piece of the left ovary was implanted between the rectal sheath and muscle. In January, 1920, a tense cystic swelling had formed under the skin  $1\frac{1}{2}$  in. to the left of the wound and 2 in. below the umbilical level. This proved to be a blood-cyst which had formed in the ovarian graft. It was opened and scraped. In June, 1920, another small cyst formed in the same situation, but nothing further was done as it caused no inconvenience. Since the above experience he (Dr. Lockyer) had been careful to remove the cortex from ovarian tissue before engrafting was carried out.

### Radiography of the Fœtus in Utero.

By T. I. CANDY, M.B. (Newport, Mon.)

THE reading of a paper on this subject is the outcome of a suggestion made by Dr. Robert Knox to whom I showed some of the radiographs which I had obtained of the fœtus *in utero*. As Dr. Knox also suggested the possibility of some of the members of the Electro-Therapeutic Section being present, in the preparation of this paper I have thought that it would be better to discuss the radiological technique employed as well as to outline the possible obstetric applications of this branch of radiological work.

It was in March, 1922, that I first tried to obtain a skiagram of the fœtus *in utero*. One of my colleagues at the Royal Gwent Hospital, Newport, Mon., asked me if I could confirm, by means of a skiagram, his suspicion of pregnancy of about the seventh or eighth month in a young unmarried woman who strongly denied the possibility of such a condition. I told him that I was doubtful, as hitherto X-rays had not been very satisfactory as an aid in the diagnosis of pregnancy, but that I would take a few skiagrams in any case. When I saw the patient I was of opinion, clinically, that it was a case of pregnancy but the first skiagram I took, though an excellent one of the spine and pelvis of the suspected mother-to-be, did not show any evidence at all of fœtal parts. In a second skiagram I thought I could see the outline of a fœtal skull but my colleague good-humouredly told me that I was gifted with a lively imagination.

It was this failure (for a healthy baby was born a few months afterwards) to be able to give a conclusive finding that stimulated my interest in this work, for I felt that X-rays ought to be of definite help in this difficulty in which a doctor is often placed. If I had the same case to-day I could give a positive answer.

I first consulted the English literature to find what had already been achieved but I got little help. There was more in the Continental literature. As early as 1897, Warnekros, Berlin, says that Levy-Dorn recognized the skull in a skiagram in an eight months' pregnancy and that he concluded that X-rays might be of value in the diagnosis of twin pregnancy, but that many other competent workers had reported failures to demonstrate any foetal parts in a skiagram in cases of undoubted pregnancy in spite of exposures in some cases of one and a half hours. He says that Grysowski, in 1904, pronounced that X-rays as an aid to diagnosis even in the later months of pregnancy were extremely unreliable. At about this time Albers-Schönberg introduced his method of tubular compression and he succeeded in showing parts of the foetal skeleton in many cases in the later months. But this was not of much service from the obstetric point of view because the field was too small and the method was unable to give any help as to the question of position or lie of the foetus in relation to the maternal parts. There is a communication upon the radiography of the foetus *in utero* published in the *Bulletins et Mémoires de la Société de Radiologie Médicale* of April, 1913, by Drs. Potocki, Delherm and Laquerrière. They obtained a number of skiagrams in the latter months; some illustrations were published. It was in 1921 that Warnekros published his atlas of skiagrams of the foetus *in utero* and these appear to have been the first really successful skiagrams published showing the whole foetal skeleton in its relation to the maternal skeleton. The apparatus he used was a powerful transformer apparently specially made for the purpose and capable of an output of 150 to 200 milliampères in the secondary circuit for a period of one or two seconds. The gas tubes which he employed could be used only once and had to be reconditioned if they survived. This appeared to be too extravagant a research for me. My apparatus consisted of a modern 10 K.V.A. transformer and Coolidge tube. The makers said it was capable of a maximum output of 80 to 100 milliampères in the secondary circuit, but I never tested it because the risk to the tubes was great and Coolidge tubes are too costly to experiment with. My hope was to find a method which would be no more difficult to carry out than an examination of the kidneys or the lumbar spine. It was about this time that I became the possessor of a Potter-Bucky diaphragm and this piece of apparatus eventually proved to be the key to the problem. In November, 1922, I obtained the first result which pleased me. It was a case of pregnancy of about the eighth month and the skiagram showed the foetus clearly and well differentiated from the maternal bony parts and the method was as simple to carry out as I hoped for.

The technical difficulties to be overcome in obtaining a satisfactory skiagram of the foetus *in utero* are: (1) The thickness of the maternal soft tissue to be penetrated, which causes scattering of the beam of X-rays and general fogging of the film. (2) The cartilaginous nature of the foetal skeleton, giving poor contrast in such a thick mass of soft tissue. (3) The presence of the amniotic fluid, which increases the scattering of the beam of X-rays. (4) Foetal movements, as instantaneous exposure is practically impossible.

The first three difficulties are the most important and these can be overcome almost completely by the use of the Potter-Bucky diaphragm. I do not

intend here to go into the technique of the use of this apparatus except to say for the benefit of those who may not be familiar with it, that it is an instrument recently introduced to eliminate the radiologist's old bugbear, namely, scattered radiation. And there is no doubt in my opinion that it does achieve all that the inventor claimed for it. For detail and contrast in thick parts of the body I believe that it is a *sine qua non* and that it is the most useful piece of accessory X-ray apparatus introduced since the advent of the Coolidge tube. The fourth difficulty, namely, foetal movements during exposure, can be minimized and to a great extent removed completely by correct positioning of the patient and by the use of a broad calico compression-band not unlike an ordinary obstetric binder. Sufficient fixation of the foetus can thus be obtained and I have given exposure up to ten seconds without movement showing.

#### POSITION OF THE PATIENT.

The patient should lie face downwards upon the Potter-Bucky diaphragm with the trochanters about an inch above the lower edge of the film in the cassette. The chest should be well raised on pillows and the position will not then be found to be uncomfortable. Sand bags should be placed upon the backs of the thighs.

#### EXPOSURE TECHNIQUE.

The anti-cathode of the overhead tube is centred over the third lumbar vertebra at a distance of 30 in. from a 15 by 12 duplitized film in a cassette with double intensifying screens. The patient is asked to hold her breath and at the same time to press the abdomen firmly into the Potter-Bucky diaphragm. She will find this quite easy to do if the chest be well raised as stated. The assistant immediately applies firm, but not vigorous, compression with the compression-band and the exposure is made usually for about five seconds, using 25 to 30 milliamperes with a spark gap of  $5\frac{1}{2}$  in. The compression-band is made of strong calico, 42 in. long by 12 in. broad, double thickness. It is attached at one end to the side of the Potter-Bucky diaphragm. It has a hem of about 1 in. at the other end through which passes a round stick about 14 in. long and about  $\frac{1}{2}$  in. in diameter. The ends of a piece of stout window blind cord, about 20 in. long, are attached to each end of this stick. A stronger stick about 36 in. long is used as a lever to pull upon this cord, the lower bar of the couch being used as a fulcrum. The advantage of this method is that compression can be applied in an instant and removed in an instant with the result that the patient has only to endure the compression for the duration of the exposure. It effectively fixes the foetus and at the same time reduces the antero-posterior thickness to be penetrated. The exposure time of five seconds could be reduced still further by increasing the milliamperage, but I found that the sudden blast of a high milliamperage caused the patient to give a momentary tremor which of course detracted from the definition of the resulting skiagram. Lately, however, I have reduced the exposure time to three and sometimes two seconds by using the new super-speed film which Kodak, Ltd., have just introduced.

#### DEVELOPMENT.

Too much stress cannot be laid upon the importance of this operation, for a good result will depend as much upon the right technique here as elsewhere. One of the commonest faults in general radiographic technique, in my opinion, is under-development. Density in the negative is obtained in most cases by over-exposure at the expense of the contrasts. As brilliancy and contrast in

the negative are essential in radiography of the foetus *in utero*, over-exposure is to be avoided and development must be full. We should not depend upon our judgment to tell us when development is completed as too often mistakes will arise even with experienced workers. A most trustworthy method, and in my opinion the only reliable method when one has to depend upon assistants, is that recommended by Kodak, Ltd., namely, development by time and standard temperature and with a standard developer formula. It will surprise many workers, if they carry out this method exactly, to find what a great reduction can be effected in exposure time and what a great improvement in the brilliancy and contrast of the resulting negative is obtained.

IS THERE ANY DANGER TO THE FŒTUS OR TO THE MOTHER FROM  
THE EXPOSURE TO THE X-RAYS?

This is always the first question I have been asked by the patient's doctor. In my experience there is no danger at all, as the exposure time is so short. Of course, as in any other branch of radiography, an indefinite number of exposures should not be made. I once made six exposures in succession, and a further four exposures after a month's interval in one case with no ill effects whatever, subsequent labour being normal and the baby particularly healthy and vigorous.

SOME OF THE USES OF A SKIAGRAM OF THE FŒTUS IN UTERO FROM THE  
OBSTETRIC STANDPOINT.

(1) The demonstration of the foetal skeleton upon a skiagram is an absolute, positive sign of pregnancy, in fact the most positive sign obtainable. I have had several cases in which pregnancy was denied by the patient, though suspected by the clinician, and in which the skiagram demonstrated the foetus beyond all doubt. An interesting case was that of a young unmarried woman who gave a history of two and a half months' amenorrhœa. The skiagram I took at this stage failed to show the foetus. A second skiagram was taken one month afterwards, and again no foetus was demonstrated. After another month a third skiagram showed the foetal skeleton beyond all question, that is, four and a half months after the onset of amenorrhœa.

(2) Failure to demonstrate the foetal skeleton in a skiagram by the technique described, during or after the sixth month of pregnancy is, in my opinion, an absolute, negative sign of normal pregnancy. I believe it is a very reliable negative sign, even as early as the fourth month if the patient be carefully prepared in the same manner as for an abdominal operation and if this technique be carried out with particular care as to the exposure and development. The earlier the supposed stage of pregnancy the more difficult it is to portray the foetal skeleton, and hence it might be easily missed if exposure and development were not accurate. I have found that in these cases it is best to err on the side of under-exposure and over-development. Another skiagram I show illustrates a case of early pregnancy, and reveals a foetus which was estimated from the history to be not more than three and a half months old.

In speaking of the early diagnosis of pregnancy there is the method of pneumoperitoneum. When I was in Germany last year I was shown at Erlangen some skiagrams of the gravid uterus in the first few months of pregnancy obtained by this method with the patient in the Trendelenburg position. Two American writers, F. Stein and A. Arens, have lately referred to the value of pneumoperitoneum in the differential diagnosis of early pregnancy, but I do not think the method is justified, except in some very exceptional case, because of the risk of provoking abortion.

I have said that I believe that failure to demonstrate the foetal skeleton in a skiagram after the sixth month is an absolutely negative sign of normal pregnancy. The importance of that word "normal" is illustrated by the following case, which occurred recently at Newport. I will read the notes as given to me by the doctor in charge of the case, to whom I am indebted.

Mrs. M., aged 43. Married (to present husband two years). No pregnancies as result of previous marriage.

August 2, 1923: History of eighteen weeks' amenorrhœa. On examination Hegar's sign positive. Breasts showed increased pigmentation, and some fluid could be squeezed from nipples. Fundus uteri half way between symphysis pubis and umbilicus. The patient was told that she was pregnant.

September 9 (nearly six weeks later): As the patient had not felt any movements and she was not getting larger she asked for another examination. The fundus uteri had not advanced towards the umbilicus. On this occasion the patient was told that something had happened to interfere with the growth and development of the fœtus, and that she would probably have a miscarriage at an early date.

October 4: No further development except for a history of a small quantity of brownish discharge on several occasions.

As twenty-seven weeks had now elapsed since the last menstrual period the patient was beginning to get anxious about her condition, and the question of X-ray examination was considered. She thought, however, that she would prefer to consult a gynecologist first. This was done, and the opinion expressed that she was probably suffering from a fibroid tumour. Fortunately, however, the idea of a missed abortion was not absolutely ruled out, and the patient was referred to her medical adviser to be kept under observation for a few weeks longer.

On October 21, that is in the thirtieth week, the patient became alarmed by the onset of a fair amount of hæmorrhage. She passed a carneous mole. She made an uneventful recovery.

Had this case come for X-ray examination the result would have been negative because there was no foetal skeleton to show. Yet it was a case of nearly seven months' pregnancy, but of course not a normal pregnancy. I mention this case because it illustrates the necessity of being on guard against such a possibility.

Another very interesting case was that of Mrs. Q., aged 30. She had had several miscarriages, but no living child. Her last period was the middle of March, 1923. At the end of May a fleshy mass was passed. The pathologist reported that this was a seven weeks' fœtus. As the abdomen was continuing to enlarge she came for X-ray examination in September, 1923, and the skiagram showed a fœtus of about six months' gestation. I leave you to speculate whether this was a case of abortion of one of twins, or a bicornute uterus or an extra-uterine gestation.

With regard to the value of a negative skiagram in the later months of pregnancy I have an interesting case to record. Mrs. X., aged 28 (previous history: one living child, aged 8, one miscarriage four years previously), consulted her doctor for morning sickness, in June, 1922. He diagnosed pregnancy of three months. She was seen frequently afterwards and vaginal examinations were made. The abdominal tumour increased in size as in a normal pregnancy. On December 7, 1922, she was seen by Mr. Crinks (my colleague) in consultation, as she had had two fits. The patient said that she had felt movements for the past four months and the abdominal tumour present corresponded to that of a full-time pregnancy. There was no foetal heart or souffle heard and she was kept under observation for one week.

As doubts of pregnancy were entertained she was sent to me for X-ray examination. The skiagram did not show any evidence of a foetal skeleton. There was a large homogeneous shadow of moderate density occupying the lumbar region and pelvis and a small clump of denser shadows in the right kidney area. I was of opinion it was an ovarian cyst with calcified matter. The abdomen was opened by Mr. Crinks on December 15, 1922, and a large ovarian cyst was removed. On section it was found to consist of one large cyst and several small cysts filled with gelatinous fluid and with calcareous nodules in the right upper portion (see fig. 7). There was also found a nearly three months pregnant uterus. This was subsequently emptied and found to be recent and not to correspond to the time when she was first seen by her doctor. So much for the value of negative findings.

(3) A skiagram will give positive evidence upon the question of single or multiple pregnancy (*see* skiagram of twin pregnancy at 6½ months, fig. 6).

(4) A skiagram will give useful information upon the position and lie of the foetus and will differentiate positively between a head, a breech and a transverse presentation. I had an interesting case in which the skiagram showed the foetus in the transverse position with the head in the right iliac fossa. The patient being greatly constipated several enemata were given. The skiagram taken afterwards showed the head in the pelvis and to all appearances the position and lie were now normal. Unfortunately I am unable to show these skiagrams as they met with an accident, the hot water tap being turned on in error during washing, and of course they were irretrievably damaged.

It is interesting to note the changes in position of the foetus which may take place. For instance in this breech presentation (skiagram) you will see that the occiput is to the right whereas in this second skiagram, taken only five minutes afterwards, the occiput is to the left (see figs. 4, 5).

(5) It may be possible from a study of the ossification of the foetal bones to determine the age of the pregnancy. This, however, is a matter more of academic than of practical interest.

(6) A skiagram will give useful information upon the relationship of the size of the head to the maternal pelvis. It is definitely helpful in the transverse diameter, but in the antero-posterior diameter, owing to the impossibility of taking a skiagram of the pelvis in the lateral plane, the information to be obtained is not so definite. I have not had the opportunity of examining a large number of cases of contracted pelvis and consequently I have not been able to form a real idea of the limitations of the usefulness of a skiagram in such cases. I believe, however, that good stereoscopic skiagrams, by showing the head and the pelvis in relief, would contribute valuable information.

(7) A very interesting point, which is well demonstrated in a skiagram, is the separation which takes place at the sacro-iliac joints. I have heard that there are some who doubt whether a separation really takes place during pregnancy at the sacro-iliac joints but I think the skiagram definitely settles the question. It also appears to take place quite early during pregnancy. I have seen it in a case as early as three months and I believe that it may be a very valuable confirmatory sign when the foetal skeleton fails to show owing to insufficient ossification or faulty technique. The separation at the symphysis pubis does not appear to show so strikingly.

(8) Medico-legal aspect.

A recent judgment of the French Law Courts is of the greatest medico-legal importance, where damages were awarded against a surgeon who had failed to employ X-rays, and who had operated for a fibroid tumour and found



a viable infant instead. (See *Paris Médicale*, September 8, 1923, and the *Lancet*, October 20, 1923.)

The accompanying illustrations (figs. 1-7) are examples of some of the results that have been obtained by the employment of the technique described.<sup>1</sup>

In conclusion, may I put forward a plea for a more extended use of X-rays in pregnancy. I do not think that it can be denied that a good skiagram will

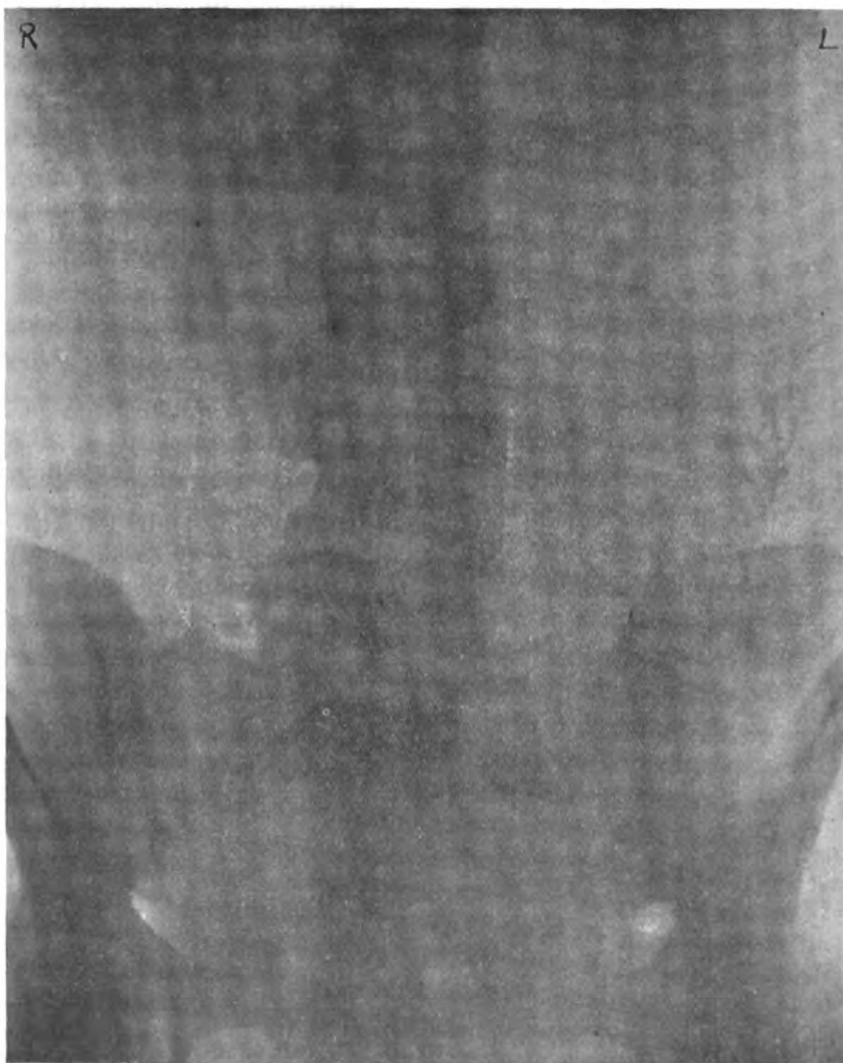


FIG. 1.—Primigravida, 8½ months gestation. Head presenting and fixed in pelvis. Occiput to the left. Subsequent delivery normal. L.O.A. presentation. A healthy full time male child.

give information on many points that cannot be ascertained with the same degree of certainty by the ordinary physical means of examination. The added assurance that it will give to the obstetrician will enable him to approach the case with less anxiety.

<sup>1</sup> The blocks of the X-ray photographs illustrating this paper have been kindly lent by the Editorial Board of the *Archives of Radiology and Electro-Therapy*.

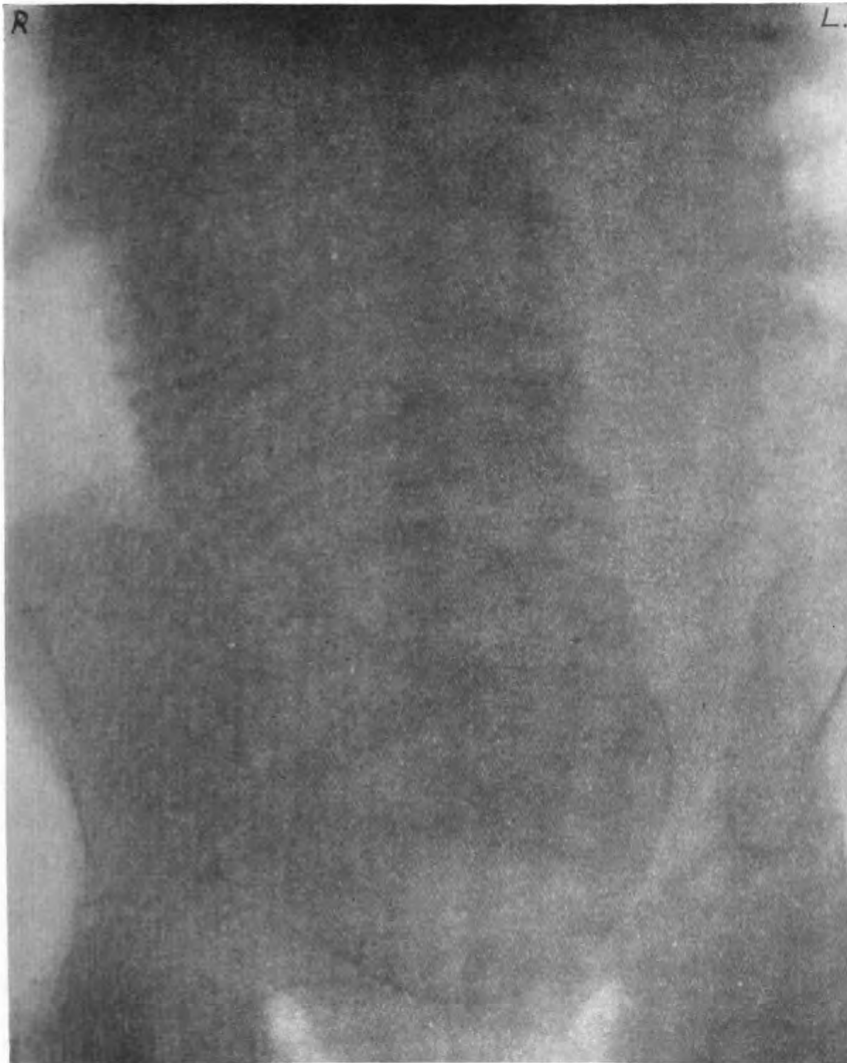


FIG. 2.—Primigravida, 8½ months gestation. Head presenting and fixed in pelvis. Occiput to the right. Subsequent delivery normal. R.O.A. presentation. A healthy full time female child.



FIG. 3.—Multipara, 8 months gestation. Deformed pelvis. Head presenting but not fixed. Occiput to the right. Subsequent delivery normal. R.O.A. presentation. A healthy full time female child.



FIG. 4.—Primigravida, 7½ months gestation. Breech presenting. Subsequent labour difficult. Delivered as breech. Female child, lived 24 hours. Note detail of the foetal hands.



FIG. 5.—The same case as fig. 4, taken five minutes afterwards. Note that the occiput has now rotated to the left.

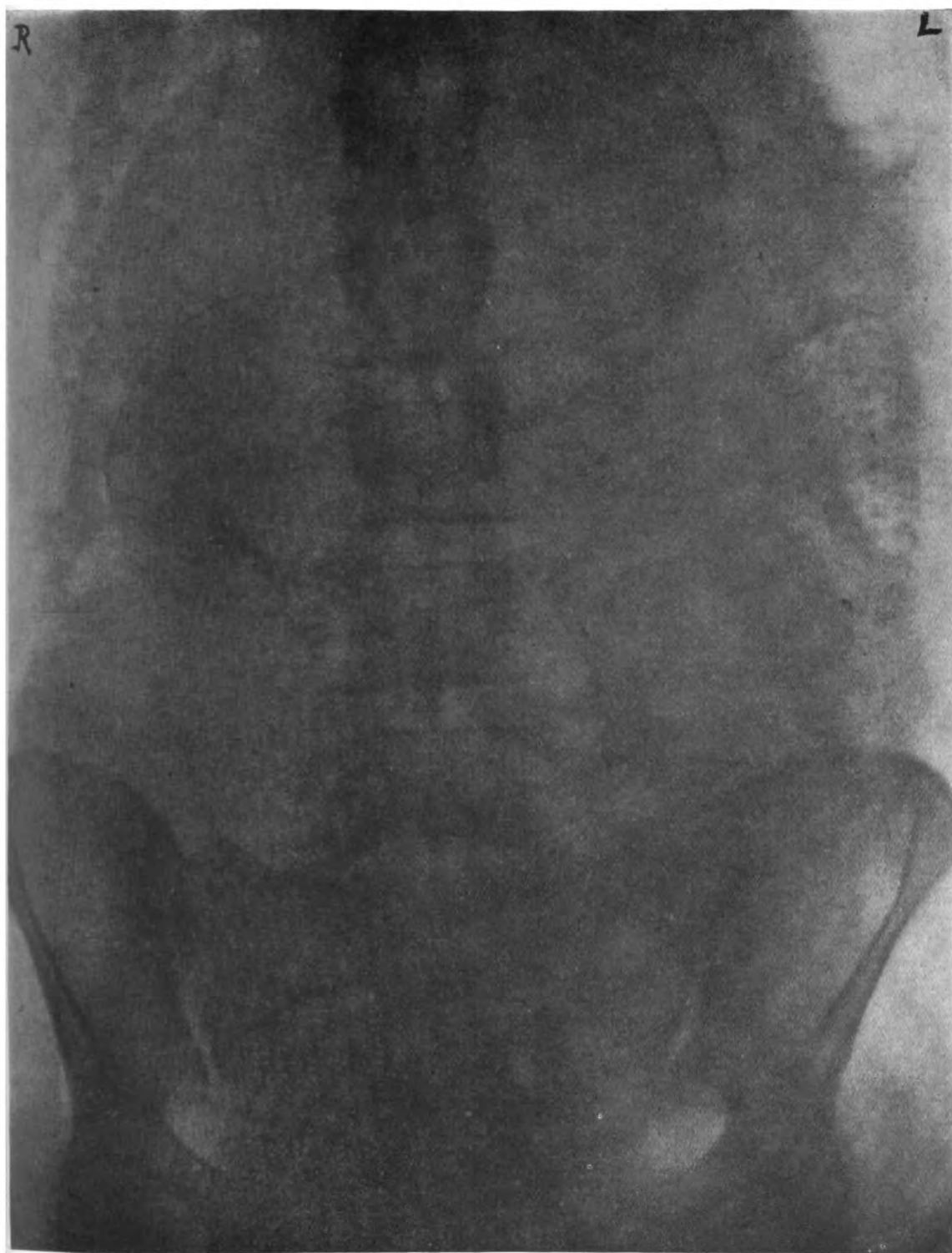


FIG. 6.—Multipara. Twin gestation 6½ months. Double breech face to face. Subsequent labour normal, one breech and one head delivery. Healthy male and female children.

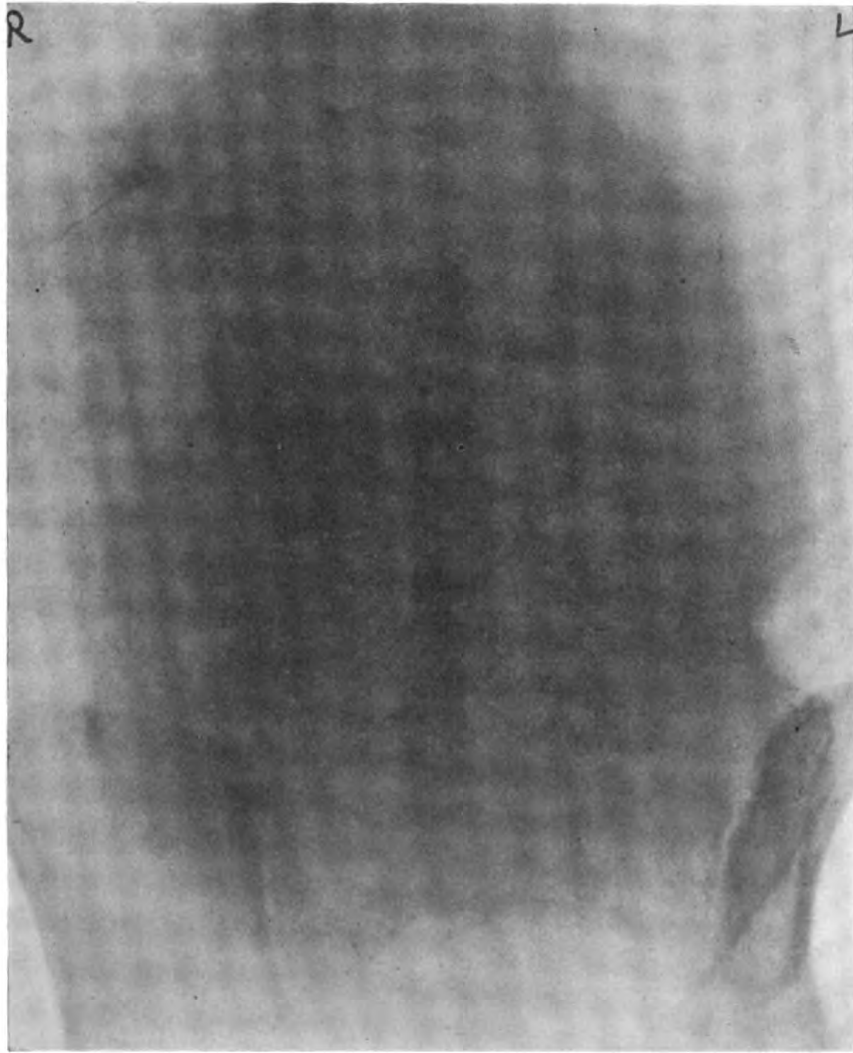


FIG. 7.—A case of a large ovarian cyst which clinically simulated a full time pregnancy. Some calcified nodules are seen in the right hypochondrium. The radiograph definitely excluded the presence of a fœtus. The lack of clearness and definition is due to the thick contents of the cyst, a point which helped in the diagnosis.

## **Radiography of the Fœtus in Utero.**

By GEOFFREY FILDES, M.B.

DURING the past two years it has become increasingly apparent that the scope of radiology as an assistance to the clinician has widened in all branches of medicine.

Lately there have been inventions put on the market by means of which considerably more intimate detail can be obtained in skiagrams, and the handling of such machines brought forward the possibility of the demonstration of the foetal skeleton at quite an early period of gestation.

Foremost among devices for this purpose is the Potter-Bucky diaphragm, an instrument for obviating the fogging effects of the scattered secondary radiation emanating from the body tissues on the passage of X-rays through them.

It was hoped in the earliest cases to which this method was applied, to be able to determine at least the position of the foetal head, and also the presence of more than one foetus. At first there were naturally failures, and as most cases examined were in an advanced state of development, much difficulty in penetrating so large a quantity of fluid was met with. In addition, this fluid is rich in scattered radiations besides being difficult of penetration.

It has been found possible, by taking cases a little earlier, i.e., about the sixth month, to produce skiagrams showing the entire foetal skeleton down to such small bones as the metacarpals.

The number of foetuses present, and their relation to one another, can clearly be shown, together with their relationship to the maternal spine and pelvis, and a very helpful opinion can be formed of the contour of the maternal pelvis by the aid of stereoscopic films.

Some difficulty is met with in obtaining stereoscopic films of the foetus itself, as, owing to the mechanical contrivances which have to be used, there is frequently movement of the whole or part of the foetus during the few seconds elapsing between the exposure of the two films.

As to the earliest period at which pregnancy can be demonstrated, opinions vary, but it has been definitely possible to demonstrate twins as early as the eleventh to twelfth week. This case has been followed up monthly, and the development watched up to the seventh month.

Use has been made on several occasions of skiagrams in cases in which a breech presentation was clinically obvious, but in which an external version failed. In four such cases there was extension of one or both lower limbs.

A further endeavour was made to ascertain the exact site of the placenta. It was expected that that organ would, on account of its particular construction, throw an irregular shadow, and in fact, in almost all the negatives such a shadow was to be seen. Although note was taken of this shadow, it was impossible to differentiate it clearly from a loaded bowel, which is so common among hospital patients, and at the same time it was hardly possible for the obstetrician to determine the site clinically at a parturition which was practically normal. At length a case was sent which was, clinically, a placenta prævia. Skiagrams showed the usual shadow in the left flank, and no marked shadow was seen in the position of the internal os. The obstetrician was shown this but no diagnosis was attempted, and a few days later the case came to labour and proved to be one of nearly central placenta prævia.

This goes to show that in reading a skiagram, the very greatest caution should be exercised and the obstetrician should in all cases be consulted.



Indeed it is hoped that where he stands to gain by X-rays, he and the radiologist will work in close co-operation. It is stated that the photographic film cannot lie, but, though this be the case, the judgment of the most experienced radiologist may sometimes be at fault.

### X-Ray Demonstration of Fœtus in Utero.

By A. LOUISE McILROY, M.D.

A SERIES of skiagrams was shown illustrating the position of the fœtus and its relationship to the maternal pelvis.

*No. 1.—At thirty-eighth week* in a generally contracted pelvis of minor degree. Primipara, 23 years. V. R.O.A. Head high but beginning to engage in brim. Delivery by forceps at term.

*No. 2.—At fortieth week* in a generally contracted pelvis of minor degree. Primipara, 24 years. V. L.O.A. Head floating. Breech diagnosed at thirty-third week. Spontaneous version at thirty-fourth week. Delivery by Cæsarean section.

*No. 3.—At thirty-fifth week* in minor pelvic contraction. Primipara, 25 years. V. L.O.A. Head floating. Did not engage in pelvis. Induction of labour eleven days before term. Progress normal.

*No. 4.—At thirty-sixth week.* Just at onset of labour in a case of twins with hydramnios. Primipara, 39 years. Diagnosis difficult, confirmed by skiagram. One head engaged in pelvis, one entering.

*No. 5.—At thirty-eighth week.* Showing fixation of head in a 2-para, 30 years. V. L.O.A.

*No. 6.—At forty-second week.* Showing post-mature fœtus. Head high, back to right, face looking downwards. 11-para, 39 years. Previous labours normal. Delivery at forty-third week by induction. V. Persistent R.O.P. Child healthy, weight 8 lb. Bones well ossified.

*No. 7.—At thirty-sixth week* in normal pregnancy. Primipara, 20 years. V. L.O.A. Head beginning to engage. Not yet delivered.

*No. 8.—At thirty-sixth week.*—Breech. 2-para, 30 years. Back to left, head flexed. Delivery at term of normal breech.

*No. 9.—At thirty-ninth week.* Breech. Primipara, 33 years. Back to right. Version performed. V. Persistent L.O.P. Forceps applied for delay.

*No. 10.—At thirty-third week.* V. L.O.A. Showing disproportion between head and pelvis. Primipara, 24 years. Was a breech at thirtieth week, transverse at thirty-second week. Head floating at onset of labour. Trial given. Head extremely moulded, delivery by forceps. Healthy infant.

*No. 11.—At thirty-eighth week.* Old tubercular hip-joint disease. Primipara, 34 years. Previous labour very difficult. V. R.O.A. Head floating. Left hip ankylosed, irregular contraction of pelvis. Delivery by Cæsarean section at term. Child healthy.

*No. 12.—At thirty-sixth week.* 11-para, 28 years. Twins. Vertex and breech. Diagnosis made at thirty-fifth week.

The skiagrams were taken in the X-ray Department of the Royal Free Hospital under the directorship of Dr. Ulysses Williams.

### DISCUSSION.

Dr. HERBERT SPENCER said he wished to raise one question which had not been alluded to—the possible danger to the fœtus of X-ray examination during pregnancy.

It was known experimentally that X-rays administered to the embryos of lower animals caused the development of monsters. Mr. Comyns Berkeley's case of alopecia of the foetal scalp, due to the application of radium to the cervix during pregnancy, showed the possible effect of prolonged applications of radium. Could it be asserted with absolute confidence that the application of X-rays for even a few seconds was harmless to the foetus? X-rays had often been employed during pregnancy, though it must be exceedingly rarely required by a competent obstetrician for the purpose of diagnosis during the latter half of pregnancy. Although there was no apparent effect on the infant at the time he (Dr. Spencer) believed cases had been recorded in which the children were stunted in growth when examined four or five years later.

Dr. EDEN said that while he could not fail to admire the beautiful plates which had been shown, he did not think that there was any great need for the use of X-rays for the diagnosis of pregnancy. It might happen, however, that radiography could be employed to demonstrate such points as the descent and rotation of the head during labour. By means of a series of photographs the whole of the intra-pelvic movements might be recorded, and this would be much more exact than any method hitherto employed.

Dr. ROBERT KNOX said, in reference to Dr. Eden's remarks, regarding the use of X-rays in the study of labour, that while in America recently he (Dr. Knox) had had the privilege of seeing a complete set of radiograms showing the stages of labour. In addition another set illustrated the application of the midwifery forceps, and in one negative the moulding of the foetal head could be seen. The demonstration was given by Professor Hickey, of Ann Arbor University. It was becoming a routine method for teaching obstetrical students. There could be no doubt that by means of X-rays a valuable set of negatives could be obtained for teaching purposes. Dr. Knox further said that his experience of the radiography of pregnancy had been limited, but about twelve years ago he had examined a patient who had been admitted to a nursing home and prepared for an operation for the removal of an ovarian tumour, the patient being a girl of about 22. On inspection of the abdomen nothing diagnostic of pregnancy could be seen. A number of radiographs showed clearly the foetal head and spine of a foetus between the fifth and sixth month. Dr. Candy had read a very instructive paper, and Dr. Fildes had shown beautiful skiagrams. A great future could be expected for the radiography of the pelvis in pregnancy, particularly from the point of view of teaching.

Dr. R. A. GIBBONS said he was glad that Dr. Spencer had mentioned the risk of danger to the foetus. Some years ago he (Dr. Gibbons) wanted to use the X-rays with reference to early pregnancy and he consulted the late Dr. Lewis Jones, then in the electrical department of St. Bartholomew's Hospital, who dissuaded him from their use because he said that he was strongly of opinion that they caused injury to the growing cells of the brain, and on that account he was opposed to their use in the treatment of ringworm of the scalp in young children. He (Dr. Gibbons) considered then that Dr. Lewis Jones had good grounds for his opinion; but it was now known from the experience of dermatologists, that when the rays were properly applied that opinion was wrong. In view, however, of the known influence of the rays on uterine fibroids and other tumours, one could not help feeling that their too frequent application to the same woman during pregnancy might be productive of harm to the foetus. Experience of years would alone be able to settle this point.



## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### Normal Pregnancy after Operations for Ectopic Pregnancy on both Fallopian Tubes.

By F. WINSON RAMSAY, M.S., F.R.C.S.Ed.

BEFORE reading the notes of this case I should like first to say a few words as to the reasons for which the operations were undertaken.

The question as to what part, if any, the Fallopian tube takes in the causation of tubal pregnancy is at the present undecided, but if the tube were removed in all cases of ectopic pregnancy it is an obvious fact that no further light could be shown on the matter.

Acting on this assumption I have for some years been in the habit, in suitable cases, of performing a conservative operation and leaving the tube. My hope in doing this was that if the tube were the cause of the accident probably a certain number of recurrences would occur in that tube. As far as I am aware, no recurrences have occurred, but my cases carry over several years, are few in number, and gave me no definite results.

At last an opportunity came of trying the experiment under better conditions and I will now give you the notes.

Mrs. D., aged 27, was married on July 6, 1918, and went to live with her husband, who was a naval officer in Edinburgh.

I have to rely for the account of the first operation on the patient's own recollection, as Dr. Haultain, who operated on her, is now dead.

On September 1, 1918, she was taken with great pain and Dr. Haultain was called in, in consultation. He however did not advise operation. I do not know whether the condition was diagnosed at this time or not. She became considerably worse and was operated upon by him on September 6, for ruptured tubal pregnancy on the left side. The left ovary and tube were removed, and though she was extremely ill at the time, she recovered and eventually regained her usual health. I have not pursued the matter of this first operation further, because the only thing affecting the future operation was the fact that the left tube was absent, and this was confirmed at the later operation.

In January, 1920, she was under the care of Dr. Snell, of Christchurch, complaining of abdominal pain and uterine hæmorrhage. Ascertaining that she had missed a period and taking into consideration her previous history, Dr. Snell suspected another ectopic pregnancy and asked me to see her with him in consultation.

When I examined her I found there was no doubt as to the diagnosis as she had in addition to the symptoms a small tumour in her right tube. Here, then, was a condition which offered a chance of a definite experiment, one tube being already absent it might be possible to obtain some definite information as to what would happen if this tube were left.

I explained the position carefully to her husband and herself, telling them that if the tube were removed, which was the orthodox method of procedure, she would be perfectly sterile, i.e., rendered incapable of bearing a child, but that if I operated in the method I suggested she ran the risk of a recurrence of the same accident with its attendant danger and expense. I told her, however, that although I could give her no definite reason I could not see why she should necessarily have a recurrence of the accident. They therefore consented to the conservative operation, leaving the risk of further pregnancy to be decided at a later date.

She was operated upon on January 16, 1920. The tube was opened, its contents removed and the incision closed with a continuous catgut suture. Convalescence was uneventful and the patient was soon quite well.

Later on the question arose as to what the prospects were of a normal or abnormal pregnancy, if any. It was impossible for me to give any definite advice on this matter. I consulted with several colleagues and the patient also obtained advice, seeing Mr. Comyns Berkeley and Dr. Arthur Giles. Both wrote me on the matter but could not, any more than myself, give any definite opinion. They however, I think, were like myself desirous that things should be allowed to take a normal course. The patient, moreover, was willing to take the risk and resumed normal life with her husband.

I heard no more until the patient came to see me in June, 1922. She then had what was apparently a normal pregnancy.

She returned to Bournemouth in January, 1923, and was delivered by Dr. Snell and myself of a living female child on January 30. There was no particular difficulty about the labour and both mother and child did well.

My object in bringing this case before your notice to-night is, first, to put on record what I think is a unique case and also to suggest that salpingotomy, as against salpingectomy, should be more largely practised in these cases.

It is, of course, impossible to dogmatize from a single case but this one seems to prove that the tube is not at fault, or if that is not admitted, that at all events the fault is not a permanent one, and that the result is sufficiently encouraging to justify a more general adoption of the conservative operation until it is confirmed or negated by further and more general experience.

#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) congratulated Mr. F. Winson Ramsay on being able to record a very interesting and important example of conservative surgery. He (the President) believed that the claim to priority in this case would be substantiated.

In 1914 Professors Thomas Wilson and Beckwith Whitehouse read a paper in the North on the results of an investigation based on the dissection of thirty fresh specimens of tubal mole, tubal abortion and tubal rupture. In January, 1922, Professor Beckwith Whitehouse made another communication to this Section upon the same subject<sup>1</sup>. In this instance the author showed the ease and safety with which a tubal mole could be removed, pointing out that its attachment to the floor of the tube was reduced to a narrow pedicle, so that the structure represented simply a tubal polyp and that the site of removal was easily restored by a single catgut stitch. When dealing with tubal *rupture*, salpingectomy was a better procedure than salpingotomy, but for tubal mole and tubal abortion the latter was worthy of trial.

The President read a letter from Professor Beckwith Whitehouse, dated February 7, 1924, in which the writer stated: "I have now done twenty salpingotomies for tubal mole, including two patients with only one tube. Both the latter are unfortunately near the menopause, but there is still hope. Three of the other cases have had normal pregnancies since, and there has been *no instance of recurrent gestation in the same tube*. A little later it would be interesting to send out a questionnaire generally, as no one individual is likely to get sufficient figures except in a lifetime."

<sup>1</sup>*Proceedings*, 1922, xv (Sect. Obst. and Gyn.), p.17.

The President said that the above experience supported the view of those who, like Couvelaire, regarded embedding of a fertilized ovum in the tube as a pure accident and therefore as likely to occur in a healthy tube as in one which had been altered by salpingitis.

Mr. ALECK BOURNE said that he could not agree that salpingotomy was a safe method of treatment of ectopic gestation on account of the danger of a further gestation in the same tube. Preservation of the tube for further function was extremely attractive conservative surgery, but he thought that the risk of repetition of ectopic gestation required complete removal of the tube. He described a case bearing on this point. A woman had had a tubal pregnancy on the left side six years ago, and at the operation the tube had been conserved. Last autumn she became pregnant again, attaining a development of five months. A few slight attacks of pain finally culminated in a serious attack caused by the rupture of an interstitial tubal pregnancy on the left side. Bleeding was so severe as to cause death at the time of the operation.

### **A Case of Chorion-Epithelioma.**

By J. P. HEDLEY, M.Ch.

(ABSTRACT.)

THE patient, who had lived in India, was 35 years of age, had been married three years and had had no child. Her periods had always been slightly irregular, and about Christmas, 1922, she began to have bleeding every ten days. In April, 1923, the patient had very severe pain in the lower abdomen and almost fainted. The next day she had rather severe vaginal bleeding, and in the course of the next three weeks had attacks of pain in the lower abdomen of increasing intensity, with vaginal bleeding. At the end of this time, on examination, marked tenderness of the lower abdomen was detected and ill-defined masses could be felt p.v. on both sides of the uterus. As the patient had had cystitis some months previously a diagnosis of salpingitis was made, and she was treated by rest in bed. She improved for a few days but at the end of a week there was a tender mass reaching up to the level of the umbilicus on the right side. The temperature was 100° F.

Colonel Franklin was called in and diagnosed appendix abscess, but at operation found the swelling was a large left tubal gestation, which he removed. The uterus was slightly enlarged. The patient was relieved of her pain and made a rapid recovery.

On June 7, one month after the operation, a vaginal examination was made, when a fleshy mass in which a spicule of bone could be felt was found protruding through the dilated os. The patient then said that for a few days she had felt pain on defæcation and had always to sit down very gently. The mass was removed and the uterus gently curetted. It seemed to be empty and not more enlarged than a two months' pregnancy. The fornices were clear. The mass removed was about 3 in. in length, and contained a few spicules of bone, but nothing could be recognized as any definite part of a foetus. The patient picked up rapidly, put on flesh and felt very well. Her periods occurred on June 27, July 21, and August 16; they were increasingly painful but there was no excessive loss.

The August period started normally, but after the first day the discharge became pink and watery and persisted until September 2, when she was

examined again and a smooth round mass was found protruding from the external os. This was removed and found to be the size of a goose's egg and attached to the anterior wall of the uterus by a pedicle about 1 in. in diameter. The uterine cavity was empty and the wall appeared normal except for roughening where the pedicle had been divided. This tumour was suspected of being a chorion-epithelioma, but a pathologist reported that the mass, which had a structure resembling placenta, showed no evidence of malignant change.

About a week later the patient had severe pain low down in the left side of the abdomen, which persisted until September 20, when she was again seen by Dr. Helen McMillan, who found tenderness of the pelvis.

The patient left home to sail for England on October 9. It was then noted that her temperature varied between 99° and 100°. She had some indigestion and at times pain on defæcation. The uterus was somewhat enlarged and rather tender and a hard ridge was felt between the uterus and the stump of the left appendages. The right fornix and Douglas's pouch were clear.

On October 13, profuse vaginal bleeding began, lasted for three days and was followed by a pink discharge. This continued during the voyage home from India and there was continuous severe pain in the lower abdomen and back.

On her arrival in London, on November 6, there was a hard fixed nodular mass in the abdomen reaching up to the umbilicus and out on each side to the anterior superior spines. There was no dullness in the flanks and no evidence of any involvement of the lungs. There was very great frequency of micturition. I removed the polyp and a circular rough area about 1 in. in diameter was left where the pedicle had been divided. After the operation the frequency of micturition ceased and there was no vaginal bleeding, but the patient got very rapidly thinner and died about three weeks later. There was no evidence of free fluid in the peritoneal cavity or involvement of the lungs.

The polyp is a solid tumour measuring 3 in. by 2½ by 2 in. with partly gangrenous exterior. The cut surface shows a border of reddish tissue which surrounds a large firm, pale area. Two cysts are present in the mass, also a large hæmorrhage. There is some calcification in the pale area.

Dr. Dudgeon, who examined the tumour, reported that it was a chorion-epithelioma.

The points of special interest in the case appear to be the following:—

(1) That the chorion-epithelioma followed an extra-uterine gestation but appeared as an intra-uterine growth.

(2) That three large polypi were removed p.v. from the uterus between June and December and in each case the area of attachment to the wall was of about the same extent.

(3) That the periods returned and were regular and not excessive between the removal of the first and second polypi.

(4) That what appeared to be spicules of bone were found in the first polypus, and in calcified areas in the third polypus.

(5) That there was no evidence of ascites though the main mass of growth seemed to be intraperitoneal.

Dr. CUTHBERT LOCKYER (President) said he thought the presence of bone in the tissue first removed from the uterus suggested that the ectopic gestation had been of the interstitial variety and that some of the products had made their way into the uterine cavity; the section shown was very atypical but no interpretation other than that of chorion-epithelioma could be placed on the specimen. It was regrettable that permission for an autopsy was not forthcoming.

**Dysmenorrhœa due to Hæmatometra in the Rudimentary Horn of a Uterus Bicornis Unicollis.**

By J. D. BARRIS, F.R.C.S.

AT the Congress of Obstetrics and Gynæcology, held in Edinburgh in April, 1923, and reported in the *Journal of Obstetrics and Gynæcology* (vol. xxx, No. 2), Professor Blair Bell, in his opening address upon the subject of "Intrinsic Dysmenorrhœa," mentions that dysmenorrhœa may be due to "divided states of the uterus, due to imperfect fusion of the Müllerian ducts."

I record the following case to-night as it presents a striking example of dysmenorrhœa due to such a cause.

The patient, M. A., a nulliparous single girl, aged 20½, consulted me in June, 1921, on account of severe dysmenorrhœa. Her menses began at the age of 19, and were regular every twenty-eight days: no clots nor membranes were noticed. The dysmenorrhœa was initial in onset, worse on the first and second days of the flow, spasmodic in character and referred to the left iliac fossa mainly; the pain was so severe that it was accompanied by vomiting, diarrhœa and faintness. The pain did not occur at every month in this intense form, but no definite periodicity of the pain was elicited. There was no history of vaginal discharge. Many drugs had been tried by her own doctor to relieve the pain but with no effect.

As she appeared otherwise healthy and well developed I decided to examine her under an anæsthetic, and if necessary to dilate the cervical canal. At the examination it was noted that there was a small hard rounded mass about the size of a bantam's egg attached to the left wall of the uterus; apart from this no abnormality was detected in connexion with the vagina, cervix, uterus or the appendages. The uterine sound passed 2½ in. The cervical canal was dilated by metal dilators up to No. 12. No relief, however, from the dysmenorrhœa was obtained.

I saw her again a year later, in June, 1922, and on examination found that the small mass already noted on the left side of the uterus had considerably enlarged. Laparotomy was advised in order to ascertain the nature of the mass, and in order to remove it if possible.

Upon the abdomen being opened the body of the uterus appeared to be of usual size, and the normal attachments of the right tube and round ligament could be seen. At the corresponding point on the left side of the uterus the left tube and round ligament were found to be absent, while at a slightly lower level there was a rounded firm mass about the size of a tangerine orange. Attached to the surface of the mass the left round ligament was seen, together with the Fallopian tube and ovary.

The mass was removed; no communication between it and the uterine cavity was noted. The left tube and ovary were not excised.

The patient has not suffered from dysmenorrhœa since the operation.

The specimen so obtained consists therefore of the mass only. The absence of the round ligament, Fallopian tube and ovary, and the present shrunken appearance due to the preserving solution, detract from the value of the mass as a specimen, but I did not feel justified in removing it together with the left appendages.<sup>1</sup>

<sup>1</sup> It is for this reason that this is read as a short communication as the interest lies rather in the clinical history.



In the recent state the mass was rounded in shape, smooth in outline, of the same colour as the uterus and covered by peritoneum. It measured 10 in. in circumference and  $2\frac{1}{4}$  in. at its greatest width. At one spot on its outer surface (marked by a coloured rod) the retracted stump of the left round ligament and tube can be seen. It has been laid open to show the cavity. The wall appeared to be composed of dense fibromuscular tissue, and was about  $\frac{3}{4}$  in. in thickness in some places.

The cavity was about 2 in. in length, and contained about  $1\frac{1}{2}$  oz. of dark non-clotted viscid fluid. Examination of this fluid showed it to consist of altered blood and mucus.

Microscopic section of the wall showed unstriped muscle fibres and fibrous tissue, covered in some places by endometrium. The endometrium, where present, is flattened in some places, in others the glands appear dilated and show round-celled infiltration.

Although it is well recognized that hæmatometra of one horn of a bicornute uterus may cause dysmenorrhœa, I have been unable to find any record of an exactly similar case.

Blair Bell, in his address on "Intrinsic Dysmenorrhœa" already referred to, reports only one somewhat similar case (*Journal of Obstetrics and Gynæcology of the British Empire*, vol. xxx, No. 2, p. 147).

His description is as follows:—

"E. F. consulted me in 1913. She was then 29 years of age. She had commenced to menstruate at the age of 15. Her menstrual cycle was  $21.1.1$ , without pain. She married and had two children. When I saw her she had been suffering for some time with severe left-sided dysmenorrhœa and pain on the left side. Six months previously her appendix had been removed and the right ovary fixed in position. On examination nothing abnormal was discovered, but her pain was so severe that I felt further exploration was necessary. When laparotomy had been performed I discovered a small horn jutting out from the left cornu of the uterus. This I removed with the corresponding tube and ovary, which was cystic. From that time the dysmenorrhœa completely disappeared."

But it will be noted that in this case although there was dysmenorrhœa associated with a rudimentary horn of a bicornute uterus, there was, however, no hæmatometra of this horn.

In the discussion which followed Professor Blair Bell's address, Mr. Christopher Martin mentioned (*loc. cit.*, p. 235) a case more closely resembling the one I have recorded to-night, although the uterus was in his case uterus bicollis. His description is as follows:—

"One case he remembered was that of a double uterus. On one side the cervix was patent, and on the other it was occluded, with distension of the uterus with retained menses. There was very severe dysmenorrhœa on the side of the occluded uterus, which was relieved by freely opening the hæmatometric sac, letting out the retained menstrual blood and suturing the lining of the sac to that of the vaginal mucous membrane. The cure was immediate and permanent."

Dr. H. RUSSELL ANDREWS said that he saw Dr. Lewers operate on a case similar to that of Dr. Barris, about twenty-five years ago—one of severe dysmenorrhœa due to an undeveloped uterine horn, which contained some retained menstrual blood.

# **Specimen of a Double Uterus, with both Horns Mal-developed.**

Shown by Sir GEORGE BLACKER, C.B.E., M.D.

THE specimen was removed from a married woman, aged 47, suffering from very severe dysmenorrhœa, but there was no hæmatometra.

The patient has never menstruated and has no vagina. The attacks of pain occurred at definite periodic intervals, and were so severe as to require at times the administration of morphia. As treatment produced little effect, and as the attacks were increasing in severity, the abdomen was opened and the double uterus together with the tubes and ovaries was removed. The right half of the uterus contained a small cavity the size of a pea, which communicated with the lumen of the tube. Surrounding this cavity, but mainly medial to it, is a diffuse adenomyoma. Microscopically the endometrium is normal, and there is no evidence of any effusion of blood either into the cavity or the mucosa. The very rudimentary left cornu was attached to the right by a narrow band of tissue some 2½ in. long, and was represented by a small body about the size of a pigeon's egg. This contained a minute cavity lined by normal mucosa. Both ovaries were normal, and contained well developed Graafian follicles. The case is of interest because of the severity of the pain, and, further, because of the entire absence of any collection of blood in either rudimentary uterine cavity. No doubt the pain was produced by the flushing of the tissues with blood at the successive monthly periods.

## **DISCUSSION.**

Dr. CUTHBERT LOCKYER (President) said he had removed by total hysterectomy a specimen very similar to that shown by Sir George Blacker. The patient had no vagina and there was an adenomyoma in an undeveloped horn of a uterus bicornis unicollis. Severe pain was the indication for operation and the cause of the pain was attributed to the adenomyoma. There was no hæmatometra such as was found in Dr. Barris's specimen. When seen a year after operation the patient reported that she had had no pain since leaving hospital.

Mr. CLIFFORD WHITE said that he had removed a hæmatometra of an accessory uterine cornu measuring 2 in. in diameter from a patient aged 36, in 1913. The rest of the uterus appeared to be normal. The pain in this case occurred during and after menstruation. Three years later it had become necessary to remove the uterus owing to continued pain and menorrhagia. The uterus was found to contain adenomyomata.

# **Some Chemical Observations on the Toxæmias of Pregnancy, with Special Reference to Hepatic Function.**

By COMYNS BERKELEY, M.D., M.C.Cantab., F.R.C.P.,  
E. C. DODDS, M.B., B.S., B.Sc.Lond., and A. L. WALKER,  
B.A.Cantab., M.R.C.S., L.R.C.P.

(ABSTRACT.)

[The paper will be published in full in the *Journal of Obstetrics and Gynæcology of the British Empire*.]

A BRIEF review was given of the existing views on the chemical indications for the induction of labour. These were criticized from the point of view that the tests employed were very elaborate with regard to technique, and that in some cases they gave misleading results.

Attention was called to the findings of other workers on the hepatic side of the question, and a series of tests of hepatic function was described as being applicable to cases of albuminuria.

The tests were for bile pigments and their precursors in blood and urine, consisting of Fouchet's test for bile pigments in blood, Ehrlich's aldehyde test and Schlesinger's tests for urobilin bodies in the urine. That these tests were reliable was proved by a series of negative and positive controls.

These tests were applied in all the cases examined, which were found to fall into three groups.

- (1) Albuminuria without any clinical signs or symptoms.
- (2) Albuminuria with definite physical signs, such as œdema and vomiting.
- (3) Albuminuria associated with eclamptic or pre-eclamptic symptoms.

In the first two groups of cases, the liver tests are negative, but they become strongly positive in the third group. The blood urea and non-protein nitrogen contents of all the cases were recorded, and contrasted with the tests for liver efficiency. From an analysis of the results, it was found that in all cases developing serious symptoms, such as eclampsia, the liver tests were positive, while the more classical tests did not give uniform results. From this it was suggested that the finding of hepatic inefficiency as demonstrated by these tests might be taken as a definite indication for induction of labour. From the series of cases recorded, this criterion seemed more reliable than the information obtained by the other and better known tests.

Various theories were advanced for the ætiology and prognosis of these three groups of cases, but it had to be admitted that at present the material at the observers' disposal was too limited to admit of anything like finality of views.

#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) said that any effort to harness biochemistry to the wheels of obstetric progress was certain to meet with a sympathetic reception by the Members of this Section. The authors therefore needed not to apologize for a further contribution to the literature already published on this subject—a literature which they described as already "vast." The President added that in order adequately to discuss such a paper as the one they had just listened to, it was necessary carefully to study its contents beforehand and for that purpose a copy of the paper had been placed in the library, whilst he himself was indebted to the courtesy of Mr. Comyns Berkeley for sending him a copy which he had carefully considered at home. For the sake of those who had had no chance of studying the points brought out in the paper he (the President) volunteered to summarize the aims and the claims of the authors. (The meeting expressed its approval of this method of procedure as a prelude to a general discussion.)

The following points were included in the recapitulation: (1) The authors' aim was to invoke chemical aid in appraising the significance of albuminuria as a symptom common to many clinical and pathological types met with in pregnant women. (2) After passing in review the work of previous authors, notably in connexion with the blood-urea estimation (de Wesselow), the non-protein nitrogen standard of Myers and the urea concentration test of MacLean, the authors proceeded to express doubt as to prognostic reliability of these tests and to give their reasons for pinning their faith exclusively to tests based on an estimation of the hepatic excretory function (bile pigments in the blood and urobilinogen and urobilin in the urine). Previous experience in investigating the toxic effects of salvarsan on the liver had confirmed Dr. Dodds in his belief as to the validity of these tests and as to their probable utility in cases of albuminuria in pregnancy. The tests selected were Fouchet's (blood), Ehrlich's aldehyde test for urobilinogen and Schlesinger's test for urobilin in the urine. The

blood-urea and the non-protein nitrogen estimations were also made and the results of these, side by side with those of the hepatic tests, appeared on the tables and could easily be compared.

Before employing the above hepatic tests to the clinical material provided by Mr. Berkeley, Dr. Dodds had performed a series of negative and positive controls. The latter proved the selected tests to be more delicate than those usually employed in the wards, viz., Gmelin's, Hays' and the iodine test, and the claim was made that the three tests used would show *early* disturbances of the various hepatic functions and also indicate *early* histologic changes in the liver, whereas the old bile tests were positive only when it was too late for them to be of prognostic value.

Mr. Berkeley's clinical material was arranged in groups: (1) Normal pregnancy, (2) pregnancy with albuminuria, (3) albuminuria plus symptoms of nitrogenous retention, (4) pre-eclampsics and eclampsia.

The pigmentary tests of blood and of urine were negative in all groups save the last, and as the clinician had resolved not to induce labour in the absence of a positive hepatic test, an important group of seventeen cases all with symptoms such as headache, vomiting and œdema was watched and this conservative policy resulted in a normal labour and puerperium in each case. How long the albuminuria had persisted before the onset of labour the authors were careful to point out could not be stated. Neither did they state whether and in how many the albuminuria cleared up before the patient left hospital. Finally, in the seven cases in which a positive test was obtained and therefore (according to the standard adopted) induction was indicated, four patients passed into the convulsive state before induction could be attempted; two had labour induced and on one Cæsarean section was performed.

The President summed up by stating that the subject for discussion was the authors' claim of (1) having provided a means of diagnosing *early* hepatic changes and of (2) having procured by such means a reliable aid to prognosis in conditions as variable as the small red kidney, the large white kidney and the "toxic" kidney.

Dr. O. L. V. DE WESSELOW said that the paper raised many points for discussion, and that the Section was much indebted to the authors for putting on record their observations on a group of diseases which were, in the London area at all events, somewhat infrequent. Personally he had found the occurrence of a high urinary diastatic index infrequent in the toxæmias, but he had worked with the old Wohlgemuth method, which was known to give inconstant results. The authors, working with the improved method suggested by one of them, had also obtained diastatic indices lying within the normal limits in their pre-eclamptic and eclamptic cases. This method would, therefore, appear to be of no practical value in the diagnosis of the condition.

As regards tests of liver function, these were notoriously unsatisfactory. Certain methods, such as the examination of the lipase content of the blood, the nitrogen partition and amino-acid content of the urine, and the cruder methods of estimating altered sugar tolerance had definitely failed. There remained two possible lines of examination: (1) The pigmentary tests, which were to some extent dependent on the patency of the bile-ducts; and (2) certain tests which were more intimately connected with the activities of the actual liver cell, such as the lævulose tolerance test and possibly the glycuronate synthesis. The bearing of the two groups on the actual conditions present in the liver was uncertain and the results were not capable of numerical expression. Of the pigmentary (excretory) tests the tetra-chlor-phenolphthalein had been most studied in these conditions. It depended upon the rate of excretion of this dye when injected into the blood stream, excretion being impaired in conditions of hepatic damage. Liver involvement could be demonstrated in all pre-eclamptic cases, and in some cases of pernicious vomiting which recovered without induction. It was, therefore, too delicate as a test for the indication of induction. In the case of the lævulose tolerance test, he had seen negative results in eclamptic and pre-eclamptic cases, but had obtained definite evidence of derangement of liver function in ante-partum eclampsia after the occurrence of the fits. This method of examination did not appear to be sufficiently delicate and gave no indication in some cases of impending eclampsia. Its chief interest lay in the fact that its employment demonstrated that a considerable degree of hepatic damage was not incompatible with recovery in these cases. He had no experience of the pigmentary tests employed by the authors.

He still believed that a blood-urea content above 40 mgr. per 100 c.c. indicated a serious degree of renal impairment in a pregnant woman. The converse did not hold, since in many eclamptics no very definite impairment of nitrogenous excretion occurred. In pre-eclampsia the clinical signs, and more especially frequent estimations of the blood-pressure, were probably the safest guide to induction. He thought it possible that in the authors' second group of albuminuria with symptoms, some of the cases showing a relatively high blood-urea content were suffering from so-called nephritic toxæmia. Such cases usually passed through the pregnancy without very obvious symptoms, although the child often perished *in utero*. The kidneys were, however, frequently very seriously damaged, and the after-results in such cases might be disastrous. The group of albuminurias of pregnancy without symptoms was probably mainly the expression of the incidence of mild albuminuria in the female population, and in such cases the albumin did not disappear after delivery.

Mr. FRANK COOK said that he had postulated, as a factor in the causation of albuminuria of pregnancy, pressure on the left renal vein, and not on the ureter as the authors had suggested. He had been surprised to note the frequency with which they had obtained evidence of acetonuria, as in his own work he had found a positive Rothera reaction only in cases of severe vomiting with consequent starvation; considerable attention had been paid to the question of ketosis in pregnancy by Harding and his fellow workers. Although the biochemical evidence of hepatic disorder that had been presented would appear to afford indications of considerable practical value, it did not throw any real light on the origin of the matter: hepatic lesions had already been clearly demonstrated, but there was no reason to believe that they represented other than a secondary factor. Unfortunately biochemical research had hitherto reflected only the *results* of derangement. He fully endorsed Dr. de Wesselow's opinion with regard to the importance of observations on the blood-pressure, and suggested that we should concentrate our attention on the circulatory conditions of pregnancy in general and in detail.

Mrs. PILLMAN-WILLIAMS said she would like to ask a question about the catarrhal cases in the positive controls. If, as was general in these cases of obstructive jaundice, no bile pigments reached the intestine, how was the excess of urobilin to be accounted for, as the precursor of this excess of urobilin was prevented by the obstruction from reaching the intestine.

She then brought forward one or two points: First, she had done a large number of blood and urine analyses on normal pregnant women, making three or four analyses in each case (over fifty cases). She had not found, like the authors, a decrease in the urea only, the non-protein-nitrogen remaining high; but she had found a decrease in both the urea and N.P.N. during pregnancy. The figures tended to show a slight rise towards the end of pregnancy (urea average 20 mgm. per 100 cc., N.P.N. average 25.2 mgm. per 100 cc.), but never rose to the average of the cases when non-pregnant (urea 27.6 mgm. per 100 cc., N.P.N. 33.8 mgm. per 100 cc.). The figures for normal pregnancy had never reached 40 mgm. per 100 cc., as found by the authors.

In these normal cases Rothera's test for acetone in the urine was *not* positive. The ammonia coefficient varied from 4 to 10, and the diastatic index varied from 10 to 60; the diastatic index for the blood varied from 2.5 to 16. In following up cases of albuminuria with œdema, several analyses again had been done in each case, values for urea and N.P.N. were on an average 28.2 mgm. per 100 cc. and 30.5 mgm. per 100 cc. Only cases with known nephritic histories had given values above 40 mgm. per 100 cc.

Unfortunately only one eclamptic had been seen in the last two years, but this patient again became pregnant six months later, and had fits again about the thirty-second week. The patient had frequent (fourteen) blood examinations in the interim. Her blood-pressure never fell below 178 mm. Hg. The urea value was never above 28 mgm. per 100 cc. and the N.P.N. value never above 33.4—the albumin cleared up temporarily. At six months her blood-pressure rose to 200 mm. Hg. The lævulose tolerance test showed some liver insufficiency (rise 20 mgm.). She was put on carbohydrate diet. This caused a distinct fall in the blood urea and N.P.N.,

and by the end of four weeks a second lævulose test showed no liver insufficiency. Just over two weeks later, however, the patient was admitted having fits.

Dr. LOUISE MCILROY expressed her admiration for the good piece of research work done by the authors. She said it was work which entailed the expenditure of much time and trouble, and in many cases with incomplete or disappointing results, as those who had carried out similar investigations could testify. From the clinician's point of view the decision as to the method of treatment by induction of labour seemed somewhat drastic when based upon the results of such delicate and variable chemical tests. There was no assurance that such tests were of permanent value, nor that the chemical investigators would not change their minds as to their utility at some future date. It was well to keep a balanced mind and take other factors into consideration. The urea content of the blood and the urea concentration test were of great use when associated with alterations in the blood-pressure. After considerable experience of the diastase test in the urine, the speaker was inclined to the opinion that its value was somewhat unreliable as indicating a severe toxæmia on the one hand or destruction of renal tissue on the other. The epigastric pain complained of by pre-eclamptic patients might have a hepatic origin, as during the war it was found that patients suffering from epidemic jaundice had a similar symptom. In some cases in which post-mortem examinations were obtainable, small punctiform hæmorrhages were found under the surface of the liver.

Dr. J. R. MARRACK said he considered that "acidosis" could not account for the low blood urea in pregnancy. If acids, accumulated in the blood, were neutralized by ammonia at the expense of urea, this would make no difference to the apparent blood urea, as all blood-urea determinations included any preformed ammonia present. Also, as a matter of fact, in the most extreme degrees of acid accumulation the ammonia in the blood was negligible, the neutralization of acids by ammonia being apparently done in the kidneys.

Dr. EVERARD WILLIAMS said that in his opinion this investigation had been undertaken on the basis of mistaken premises, and that the problem had been attacked from the wrong point of view for the following reasons:—

(1) In the condition known as eclampsia there was no evidence of any toxic body in the maternal circulation.

(2) In other diseases in which the presence of a toxin had been proved, it had been pointed out by Langdon Brown that the cells of the body which were affected by the toxin (whether it were exogenous or endogenous in origin) were cells rich in lipoids, and not cells like those of the liver.

For these reasons he did not agree that we should expect to find evidence of liver derangement in pre-eclamptic "toxæmia."

With regard to the tests which had been employed, he objected to them for two quite different reasons:

(1) *The tests depending upon the detection of bile pigments.*—Many authorities doubted whether the metabolism of the bile pigments was confined to the liver cells, while some, including McNee, denied that the manufacture of bile pigments took place in the liver cells at all, and referred this process to the endothelio-reticular system. It would appear, therefore, that until the physiology of the problems was more fully understood, it was highly unsatisfactory to employ these tests as an indication of the functional state of the liver.

(2) *The Lipase test.*—This test must be taken in conjunction with Rothera's test. He thought that the authors of this paper had missed the significance of their results (70 per cent. positive during pregnancy). The positive Rothera test did not indicate a condition of acidosis. What it did indicate was a faulty metabolism of the fats. It was known from the work of Leathes that fat was stored in the liver in the unsaturated form. It was known that the fat of the fœtus was composed of the saturated form. It was known from the work of Goodall that the process of saturating the unsaturated fats was conducted in the maternal liver and not in the fœtus.

It was therefore apparent that the lipase ferment had a very special significance during pregnancy in that the life and growth of the fœtus were dependent upon its

action. From what was known of the behaviour of the body in disease it was to be expected that a ferment so vital in its action would be conserved unaltered, at any rate until the latest stages of disease. He thought, therefore, that it must be recognized that the estimation of the functional activity of the liver in the gravid and in the non-gravid subject constituted two quite different problems, according to whether the pregnancy was normal or abnormal. Tests which were of value in the one condition were not necessarily applicable to the other.

With regard to the other divisions of the paper he would ask the authors two questions: (1) If their statement was correct, that the commonest cause of albuminuria without symptoms, was pregnancy complicated by hydramnios or an ovarian cyst, how did they explain the fact that their cases in Group I did not afford a single example of these complications? (2) Again, if their explanation was correct that the normal blood urea of pregnancy was low, because of the coexisting "acidosis," how did they explain the fact that in the condition of diabetic coma the blood urea might be normal?

Mr. COMYNS BERKELEY thanked the Members of the Section for their kind reception of the paper, and his colleague, Mr. Eardley Holland, for allowing him to include the results of the examination of one of his patients. He (Mr. Berkeley) said that it must be obvious that most of the credit of the paper was due to his colleague, Dr. Dodds. With regard to the query of the President as to why, in certain cases in Group III, in which the hepatic test was positive, labour had not been induced before the onset of eclampsia, the answer was that the eclampsia had supervened at once, before there was time to induce or time to get the report on the blood and urine. He (Mr. Berkeley) agreed with Dr. Everard Williams, who criticized the term *toxæmia* of pregnancy as being entirely inappropriate; but the word *toxæmia* had become sanctified by common usage and although obstetricians heartily wished for a better word, up to the present it had been found impossible to find one. Dr. Williams had also criticized the authors because, having stated that the albuminuria in Group I was due to increased intra-abdominal pressure, as exemplified in cases of hydramnios and ovarian cysts complicating pregnancy, their group did not contain a single example of these conditions. He (Mr. Berkeley), however, would point out that Dr. Everard Williams must have had his attention distracted at an important moment during the reading of the paper, since the authors, whilst stating that increased intra-abdominal pressure was advanced as a cause of albuminuria, had placed it on record that they did not believe in it, because eight of the ten patients in Group I had returned to the hospital some weeks after their labour and all of them still had albumin in their urine. With regard to Professor McIlroy's interesting theory that the epigastric pain, associated in many cases with eclampsia, might be due to hæmorrhages into the liver, which she had noted, he was sorry he could not afford her any information in regard to that as he had never noticed any mention of it in the post-mortem reports.

## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### Carcinoma of the Uterus and Associated Tuberculosis.

By GILBERT I. STRACHAN, M.D., F.R.C.S. (Cardiff).

#### DESCRIPTION OF THE CASE.

THE patient, Mrs. E., aged 40, was a 2-para, the last child being 17 years old; there had been amenorrhœa for five years, but two weeks before consultation a slight uterine bleeding, lasting only a few hours, was seen. The complaint was of pelvic pain of some ten years' duration, and of increasing severity. Occasionally some dysuria was present. Ovariectomy had been performed fifteen months previously, with no relief of symptoms. For several years she had been "troubled with her chest," and had been twice in sanatoria as a suspected case of pulmonary tuberculosis. No tubercle bacilli had ever been found in the sputum, although searched for on many occasions. The family history was of no importance.

Her general appearance was healthy, and abdominal examination was negative. The cervix was found to be occupied by a hard nodular ulcerative condition, which bled only very slightly on manipulation, and which did not extend over the vaginal walls. The uterus was slightly bulky and fairly freely movable.

In view of the degree of involvement of the cervix, total abdominal hysterectomy was decided on, and this was performed on October 20, 1923. On opening the abdomen, the omentum was found to be adherent to the pelvic organs in general and especially to the stump of the left ovary. After separation of this adhesion, the uterus and a right cystic ovary and thickened Fallopian tube were isolated and removed with some difficulty. There was no sign of the left appendages, which presumably had been removed at the previous laparotomy.

On removal, the uterus was found to be distinctly bulky and of a soft consistence. The peritoneal surface was rough from adhesions, but no tubercles were to be seen. The cervix was occupied by the thick warty tumour previously mentioned. The surrounding parametrium was thickened and infiltrated, but no enlarged glands were found. The Fallopian tube was thickened, congested and enlarged, and its surface was covered with adhesions. The ovary was enlarged, cirrhotic and cystic, having the appearance of an ordinary example of chronic oöphoritis.

After fixing for a few days, the uterus was bisected tangentially, and the cut surface presented the appearance seen in fig. 1. The cervix was seen to be occupied by infiltrating hard, grey carcinomatous tissue; above this was the isthmus, which was thin, and above this again the uterine cavity was seen to be



definitely distended and full of pale yellow caseous material. The body and cervix measured 4 in. (10 cm.) in length; the antero-posterior measurement at the fundus was  $1\frac{2}{3}$  in. (4.2 cm.); at the isthmus  $\frac{1}{2}$  in. (1.3 cm.); and at the cervix again  $1\frac{2}{3}$  in. (4.2 cm.). The transverse measurement at the fundus was 2 in. (5.0 cm.), and at the cervix  $2\frac{1}{4}$  in. (5.7 cm.). The uterine walls were  $\frac{1}{3}$  in. (0.9 cm.) in thickness, and on manipulation a sharp line of demarcation was found between the muscular wall and the contained caseous material.

Section of the tube showed the walls thickened and the lumen occupied by degenerate mucosa and much curdy and caseous secretion.

The ovary on section showed only a thick capsule with numerous cysts of various sizes.



FIG. 1.—To show the cervix infiltrated with carcinoma, the waist-like isthmus and the corpus uteri distended with tubercular caseous material.

*On microscopical examination*, sections of the cervix showed typical columns of squamous-celled carcinoma. The main histological point of interest here was the presence of definite signs of cornification and cell-nest formation.

In a section of the uterine wall, on the one side the structureless caseous material was seen; on the other, the muscular layers of the uterine wall, and in between these two layers a third layer consisting of the remains of the mucosa. This was an open loose fibro-cellular meshwork, thickly infiltrated with lymphocytes, and showing at intervals multinucleated giant cells, singly or in clusters. In some places these giant cells were seen to be surrounded by a layer or two of epithelioid cells, outside which was the lymphocytic zone referred to, but in other places the arrangement was not so regular, the giant cells lying free in the fibro-cellular meshwork surrounded by lymphocytes.

The blood-vessels were generally dilated, but the giant cell-systems were not definitely related to the vessels. The lymphocytic infiltration extended to the superficial part of the muscular wall of the uterus, where the muscle-fibres were seen to be loosely arranged and in places separated from each other by lymphocytic collections. Tubercle bacilli could not be identified, although searched for in sections specially stained.

The tubal mucosa was swollen and infiltrated with small round cells. The plicæ were enlarged and in many places fused together, and the surface epithelium was almost entirely lost. Such cells as remained were degenerated and stained poorly. Vascular dilatation was present and the vessel walls were thickened. The muscular layer was œdematous and infiltrated in its inner part with lymphocytes, which, however, were not found in the more peripheral layers. The peritoneal covering was thickened and showed chronic inflammatory changes.

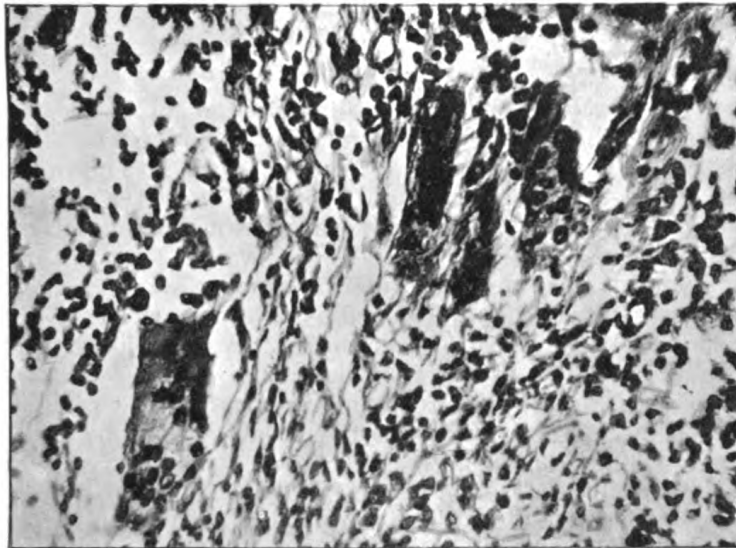


FIG. 2.—Tubercular nodule in the corporeal mucosa showing several giant-cells surrounded by lymphocytic infiltration (high power).

The ovary showed the changes of chronic oöphoritis and needs no detailed description. In neither the tube nor ovary were giant cell systems to be found, and staining by Ziehl-Neelsen's method failed to show the presence of tubercle bacilli.

The physical characters described indicate, in my opinion, the association of old-standing tuberculosis of the uterine wall with carcinoma of the cervix, the isthmus being apparently free from either lesion, and it is the rarity of such a combination of lesions that warrants the recording of the specimen here. So far as can be ascertained, this is the first specimen of its kind from a British source.

The following are the main points of previously recorded cases :—

*Von Franqué's Case* [1].—Aged 43, vaginal hysterectomy for carcinoma of the cervix; 2 cm. above the internal os was a pink nodule the size of a cherry stone, surrounded by normal mucous membrane, with a similar and smaller

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nodule above it. Microscopically, these were found to be incipient tuberculous foci.

Several myomata were present in the uterine wall at the fundus.

*Bass's Case* [2].—Aged 61; post-mortem specimen; necrotic carcinoma of cervix. Uterine mucosa destroyed and replaced by excrescences of various shapes and sizes projecting into the uterine cavity. On the serous surface there were many nodules. Microscopically—a caseous tuberculosis of the corpus uteri with carcinoma of the cervix. Numerous old tuberculous foci in the lungs.

A subserous myoma the size of a walnut was present in the uterine wall.

*A. Stein's Case* [3] was a virgo intacta of 48 years. Total hysterectomy was performed for carcinoma of the cervix. An old caseous tubercular deposit was found in the right tube, and the uterine cavity was a sac lined and filled by caseous material. Microscopically, in the uterus and tube many giant-cell systems were found, surrounded by columns of carcinoma which had spread upwards from the cervix as far as the tube. No tubercle bacilli could be found.

*Wallart* [4] records three cases.

(a) Aged 55 years; curettage performed, and in the curettings were numerous tubercles, besides an adeno-carcinoma. No more details are given.

(b) This case is described at great length. Aged 50; chronic pulmonary tuberculosis. Post-mortem specimen. Length of uterus, 9.50 cm.; thickness of walls from 1.0 to 3.0 cm. Extensive excavated carcinoma of cervix.

Near the right uterine cornu was a soft walnut-sized nodular tumour. This was a myoma. Elsewhere the uterine mucosa was thick and furrowed. Both tubes were thickened and attached by adhesions to the posterior uterine wall. The fimbriae opened into a small abscess cavity in the pouch of Douglas, which communicated by a fistula with the rectum. The ovaries were normal.

Microscopically, tubercles were found in the deeper strata of the tubal mucosa, and in the muscular and subserous coats. No tubercles were found in the region of the peritonitic abscess nor in the mucosa of the corpus uteri, and no tubercle bacilli were found in the tubal wall.

In the cervix tubercular formations were present, intermingled with columns of carcinoma cells, and tubercle bacilli were demonstrated here.

(c) Aged 37. Post-mortem specimen. Tuberculous meningitis and pulmonary tuberculosis. Adenocarcinoma of the cervix was present. About 1 cm. below the right tubal angle a tubercular nodule the size of a lentil was present in the uterine mucosa. No tuberculous signs were present in the carcinoma.

*Ahlefeldt's Case* [5]. Aged 40; sterile. Total hysterectomy for carcinoma of the cervix; myoma the size of an apple in the left cornu.

In the corpus uteri the mucosa was thickened, and microscopically caseated tubercles, in which the tubercle bacilli were demonstrated, were found together with columns of carcinoma. The ovaries and tubes were chronically inflamed but not tuberculous or carcinomatous.

*d'Halluin and Deval's Case* [6].—Aged 35; seven children and three abortions. There was a family history of tuberculosis. Clinically a uterine tumour was found, for which total hysterectomy was performed.

An irregular mamelonated mass, riddled with spaces, was removed, in which it was impossible to say which part was uterus and which part appendages.

Histologically, the general appearance was of a carcinoma of the corpus

uteri, but at one point between the carcinoma columns were found several tuberculous giant-cell foci in which, however, tubercle bacilli could not be demonstrated.

On a review of the eight cases recorded, it is found that myomata were present in three cases, and pulmonary tuberculosis also in three cases, and in one of the latter tuberculous meningitis was present. In the case now recorded pulmonary tuberculosis was suspected, but not demonstrated.

In three cases, as in the present one, the carcinoma was confined to the cervix and the tuberculous lesion to the corpus uteri, while in the other five cases the carcinoma and tuberculosis were intermingled, in four cases in the corpus uteri, and in one case (Wallart's (b)) in the cervix.

In only two cases is it recorded that tuberculosis of the Fallopian tube was present, but in several of the others the tubo-ovarian condition is not mentioned. Also in only two cases were the tubercle bacilli identified, but it is not stated that they were looked for in every case.

In considering the question as to which lesion was primary in these cases, Stein was of opinion that in his case the tuberculosis was primary, as the uterine tubercles (1) were partly fibrous, and (2) gave a negative bacterial result. Wallart considered that in his first case the combination was accidental, and that in the second case the tuberculosis was primary and set up an irritative hyperplasia of the epithelial cells of the cervix so that they finally acquired the proliferative qualities of carcinoma. But besides this, he considers that a certain predisposition to tumour formation is present in these cases and quotes, in support of this, the various cases in which uterine myomata were found.

Ahlefelder also considered that in his case the tuberculosis was primary.

In the case now recorded the matter is not easily settled. From the condition of the corpus uteri, filled as it is with the caseous results of tuberculosis of the mucosa, it may be presumed that the lesion here is of very long duration, and possibly a secondary blood infection from a chronic pulmonary tuberculosis. In the cervix, on the other hand, although some cornification is seen, the epithelioma cells are actively multiplying, and judging from the size of the cell columns the condition is far from recent.

Regarding all the features of the case, it is concluded that in the uterus the tuberculosis was the older lesion, but as the tuberculous and carcinomatous conditions were separated by an unaffected zone of uterine tissue, it cannot be shown that the tuberculosis had any ætiological effect in stimulating the cervical epithelium to malignant proliferation, and so we must regard the epithelioma as an accidental accompaniment or complication of the previously established tuberculosis of the corpus uteri.

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#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) that said the absence of epithelioid cells around the giant cells was not surprising since it was obvious that the tuberculous lesion was very chronic and therefore the presence of typical histological focal "systems" were scarcely to be expected.

Dr. EVERARD WILLIAMS suggested that the atypical giant-cell systems shown in the sections might be extensions of squamous carcinoma into the body of the uterus and not tuberculous systems. The fact that the specimen had been hardened in formalin for several days before the uterus was opened might explain the caseous appearance of the pyometra. Examination of the specimen showed clearly a moderately advanced carcinoma of the cervix, in which extension to the body of the uterus and the formation of a pyometra might be expected.

He presumed Dr. Strachan would allow the specimen to be examined by the Pathology Committee.

### A Case of Pemphigus in Mother and Child Associated with Uterine Sepsis.

By EVERARD WILLIAMS, M.D.

MRS. M. P., was admitted to Charing Cross Hospital under the care of Dr. Lockyer on November 7, 1923, with the following history.

She had been delivered of uniovular male twins on October 27 and was now in the twelfth day of the puerperium. Throughout this, her first pregnancy, she had enjoyed good health. The last regular period ceased on December 18, 1922, and the expected date of the confinement was September 23, 1923.

Labour did not set in, however, until October 24, thirty-one days late according to the calculation. She was attended by her own doctor who states that the labour was prolonged and characterized by primary uterine inertia.

The membranes ruptured at 9.30 p.m. on October 26, and she was delivered of twins on October 27, at 2.20 a.m., and 2.50 a.m., respectively.

The first child was extracted by means of a low forceps operation; the second was born spontaneously. The first was healthy, the second was still-born and the skin was peeling. It had evidently been dead for a few days, but presented no signs of disease.

The third stage of labour was normal in all respects. The weight of the first child was 7½ lb., that of the second 8 lb. The labour lasted seventy-two hours and delivery took place within four hours and fifty minutes of the rupture of the membranes. There was some laceration of the perineum and lower vaginal walls, chiefly on the left side, but no stitches were inserted. The puerperium was complicated. On the evening of the second day, viz., October 28, the mother complained of pain in the region of the shoulders and an examination revealed the presence of bullæ in that situation. Fresh crops of bullæ subsequently appeared, gradually spreading over the thorax, face, abdomen, and extremities, in that order. The mucous membranes of the mouth, eyes and vulva were involved in the eruption. The patient became febrile and felt ill. The lochia were reported as normal in character and amount until the seventh day when they were noted as being offensive. The child was in good health until the fourth day, when bullæ appeared on the chin and round the mouth. The cord was clean and dry; there was no jaundice and the stools were normal. Lactation was continued for a time, but the child gradually became ill and died, covered with bullæ, four days later, on November 4.

The mother was seen in consultation by Dr. Lockyer on November 6 and admitted to hospital on the following day. On examination, the patient was obviously very ill. Temperature 104.5° F., pulse 140, respirations 32. The

skin of the face, arms, back, chest and abdomen showed general epithelial desquamation, a number of moist scabs, and many small bullæ containing clear fluid. The mucous membranes of the eyes, mouth, and vulva were also involved. The uterine fundus was midway between the umbilicus and the pubes, and the lochia were profuse and offensive. There was incontinence of urine and fæces.

Pelvic examination showed small lacerations of the perineum and lower vaginal walls, chiefly on the left side, but the sphincter muscle was intact. The cervix was lacerated and patulous, the uterus was bulky and there was some tender induration on the left side of the pelvis.

Examination of the urine showed 0.1 per cent. albumin, a considerable amount of pus, and an organism which proved to be *Bacillus coli communis*.

Examination of the blood showed 30,000 white corpuscles per cubic millimetre, of which 92 per cent. were polynuclear cells. Aërobic cultures on two occasions failed to produce any growth. Examination of the uterine discharge, taken with the usual precautions from the canal of the cervix, showed streptococci morphologically resembling the *Streptococcus pyogenes*, together with Gram-positive and Gram-negative bacilli.

The patient was seen by Dr. MacLeod, who confirmed the diagnosis of acute pemphigus and gave a fatal prognosis. The treatment adopted in addition to the usual measures employed in puerperal sepsis, consisted of three injections of polyvalent anti-streptococcal serum, followed by daily injections of sensitized polyvalent streptococcal vaccine. Continuous intra-uterine irrigation with a solution of potassium permanganate and sodium sulphate, after the formula of Dr. Mervyn Gordon, by means of a soft rubber catheter, punctured as in a Carrel-Dakin tube and coiled round the cavity of the uterus, was employed. The patient was nursed in the Fowler position on a rubber ring, fixed on a bed utensil. The strength of the patient, however, gradually failed, fresh bullæ appeared, the abdomen became progressively more distended and the course of the temperature was of a hectic type. She died on November 24 after an illness lasting twenty-seven days.

Post-mortem examination showed the usual findings in a case of death from toxæmia. The mucosa of the stomach and intestines was not involved in the eruption. The uterus was distinctly subinvolved and showed clearly the penetration of the tissues by the permanganate solution. There was a small anterior parametric abscess containing about 1 oz. of pus. The uterine appendages were normal.

Sections through the uterine wall show the usual findings of an acute inflammatory reaction—considerable œdema of the tissues and collections of plasma cells. There are widespread deposits of a brown pigment indicating the penetration of the permanganate solution.

A section stained by Weigert's method shows large numbers of streptococci in short chains in the blood-vessels.

This would appear to be a case of acute pemphigus originating in the mother and transmitted to the child. In the case of the mother there was the additional complication of the puerperal streptococcal infection, which was probably responsible for the fatal termination. Whether this is an instance of a multiple or of a single infection cannot be settled at the present time, owing to the uncertainty of the ætiology of acute pemphigus.

The natural deduction is that there was a streptococcal infection of the genital tract and of the skin, but it is far from certain that this took place. The determination of the bacteriology of the skin lesions is by no means a

simple investigation, owing to the secondary infection of the bullæ with the saprophytic organisms of the skin, and it could not be worked out in this case. Blood cultures in these cases are generally sterile. Dr. MacLeod believes all cases of acute pemphigus are streptococcal in origin.

A recent investigation by Eberson at the Mayo Clinic has produced evidence for believing that pemphigus may be due to an infection with an anaërobic Gram-positive bacillus. As his paper has been published since the death of this patient occurred, no opportunity for corroboration was afforded in this instance.

I have examined the literature of the cases recorded in the English and American journals since 1914, but they show the same confusion of bacteriological findings as the earlier cases, which is apparently due to the secondary infection of the lesions with skin organisms.

My thanks are due to the President (Dr. Lockyer) for his kindness in allowing me to undertake a part of the treatment, and for his courtesy in allowing me to record the case.

#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) remarked that as he had never before seen the condition of acute pemphigus in a puerperal woman he expressed the wish that Dr. Everard Williams should communicate the case to this Section, and he congratulated him upon the completeness of the clinical report. Dr. Williams had been careful in pointing out that it could not be proved in this case that the bacteriology of the genital infection and that of the skin were the same—but the inference was so strong as to make it very difficult to avoid the conclusion that the case was one of a single and not a double infection. He (Dr. Lockyer) said that judging from the history given by the general practitioner, the child became infected by suckling, as the bullæ appeared first on its chin and around its mouth.

The extreme contagiousness of acute pemphigus was well known. In 1918 Maguire reported eighteen cases of "Acute Contagious Pemphigus in the New Born" which occurred at Richmond. The mortality was 44·4 per cent., eight of the children dying. The source of infection was a midwife and the site of entry the umbilicus. In the discussion which followed the reading of Maguire's paper at the Obstetrical Society, Vincent Dickinson mentioned a similar outbreak which occurred in 1901-2, at Parma, involving fourteen children, of whom "nearly all died." In Maguire's series, bacteriological examination of the contents of the bullæ in two instances gave cultures of *Staphylococcus pyogenes aureus*. MacLeod obtained pure cultures of *Streptococcus pyogenes* in a case of pemphigus neonatorum. Edgar, in his "Practice of Obstetrics," said it "appears to be due to the ordinary excitors of suppuration." Why these pyogenic germs attained to such virulence on rare occasions was not so easy to explain, but an environment of poverty and dirt seemed to favour its appearance, for it had been followed in the same family by impetigo contagiosa (MacLeod).

Dr. L. COLEBROOK suggested that the case described as one of "pemphigus" differed only in respect of the number of bullæ from the more usual type of puerperal septicæmia. Two of the last three cases of puerperal septicæmia seen by him showed a few bullæ on the limbs. The formation of bullæ was perhaps dependent on the digestion of tissues by streptococci which had lodged in the skin capillaries, and recent evidence suggested that some streptococci were much more active than others in respect of proteolytic digestion. He thought it highly probable that the case was one of streptococcal septicæmia.

Mr. JOHN HEWITT, M.B. (Glasgow), read a short communication on "Pelvimetry" by Dr. SAMUEL CAMERON and himself.

## **The Causes of Acute Infections of the Uterus, including Puerperal Sepsis, and Septic Miscarriages, and their Treatment by Drainage.**

By REMINGTON HOBBS, M.D.

(Introduced by EARDLEY HOLLAND, F.R.C.S.).

IN a paper dealing with the causes and treatment of uterine infection, especially those associated with puerperal sepsis, it is usual to quote statistics of the number of deaths as issued by the Registrar-General. As many cases are diagnosed appendicitis, pneumonia, &c., which afterwards prove to be acute infections of the uterus, it seems a waste of time to give accurate percentages. Again, too arbitrary a division of such conditions such as acute endometritis, septic miscarriages, puerperal sepsis, septicæmia, salpingitis, &c., into distinct diseases, may lead to confusion, especially as regards treatment.

The entry and pathway of the various organisms, with possibly a few exceptions, and the cause of the rise of temperature, are the same, namely, from the genital tract. In all, the bacteria may travel from the original site to the adnexa, or if the patient's resistance is low, to the general blood-stream, leaving little or no evidence of a local barrier. It will be the object throughout this paper to try to prove that the fundamental treatment of these conditions is the same. The method of procedure does not deviate really from the line of ultra-conservatism already practised in the acute stage of inflammatory disease, but it could have been pushed still further with no further risk and far better results.

There is no doubt that in time a rational treatment will be evolved for the proper drainage of all acute infections of the uterus. The safest way will be found to drain the uterus at the earliest possible moment, as soon as the constitutional signs are established.

### CAUSES.

The cause alleged by the majority of books dealing with the puerperium is the presence of organisms in the uterus, but this is only a partial truth, which indicates a failure to visualize all that happens in that organ after infection. The origins of puerperal sepsis seem to be more complex. Organisms are not the only factor, nor, from a clinical standpoint, the most important. Previous to the onset of labour, the uterus is free from organisms, being sealed off at the os. On the other hand, the walls of the vagina, as well as of the cervix, may be teeming with organisms of various types.

Dr. Robert Donaldson carried out a bacteriological investigation of a number of patients with puerperal sepsis or infected abortion at St. Mary Abbot's Hospital, cultures being made from the vagina, cervix and urine of each. His findings were only confirmatory of what has been proved on many occasions by various bacteriologists. They indicate that whatever organisms inhabit the vagina, the same organisms are waiting at the door of the uterus, and are ready to enter when that door is unsealed, unless Nature has some means of preventing their entry.

Is there some other factor besides mere infection of the raw surface? Let us consider some important facts which have been observed in uterine infection, which strongly suggest that some other factor must be at work besides mere infection of the raw surfaces :—



- (1) The temperature often falls after the bowels are open.
- (2) When the lochia are partially suppressed, the temperature rises; when they are re-established, the temperature falls.
- (3) Patients placed in a semi-Fowler position after delivery are less inclined to experience a rise in temperature than those in the recumbent positions.
- (4) The temperature often falls after an intra-uterine irrigation.
- (5) Foul lochia, if profuse, are often unaccompanied by a rise of temperature, or increase of pulse-rate, or other symptoms of septic absorption, such as headache, flushing, &c.
- (6) The withdrawal or escape of pus from the uterine cavity decreases the degree of temperature.
- (7) After the stitches are removed from an cedematous perineum, an escape of pus is often seen and is followed by a fall of temperature.
- (8) The removal of pieces of placenta or membrane from the cervical canal, is followed by a fall of temperature a degree or more.
- (9) The retention of septic products in the uterus often does not cause a rise of temperature unless drainage is imperfect.
- (10) The uterus may contain a septic foetus without the patient showing the signs of fever, but when labour pains start and the foetus is passing through the cervical canal, the temperature rises; and it falls when the uterus has expelled its contents.
- (11) If a septic uterus is curetted or swabbed out with a strong antiseptic, symptoms of septic absorption sometimes follow.
- (12) If uterine hæmorrhage is dammed back by vaginal plugging, a rise of temperature may soon follow.
- (13) In a case of septic endometritis, the temperature will often rise a degree or more just before a menstrual period and fall when menstruation is established.

#### HOW STASIS MAY BE PRODUCED.

From these observations it seems evident that there must be a damming back or stasis produced: (a) in the uterine wall, (b) in the cervical canal, (c) by a loaded rectum, (d) by a swollen perineum, (e) by the position of the uterus as affected by the position of the patient.

(a) *The Uterine Wall*.—Can stasis or damming back of the uterine secretions take place primarily in the uterine wall? It seems doubtful, with a cascade of lymph flowing through the uterus to its cavity, that any bacterial infection could possibly kill the tissues unless some strong local irritant were applied or severe injury were inflicted such as by curetting, &c. It is thought that the flow of lymph through the uterine wall is not interfered with, except by a local irritant or injury, and that in most cases the obstruction is first brought about lower down by a mechanical cause, either in the cervical canal by a loaded rectum or by a swollen perineum.

(b) *The Cervical Canal*.—In considering obstruction at the cervical canal, let us compare the uterus with the bladder.

During the menstrual periods, the products of menstruation are discharged by the relaxation of the circular fibres round the internal os, and the contraction of the muscle fibres of the uterus, much in the same way as the urine is voided from the bladder. If there is no obstruction from the cervical canal the uterine contents are discharged without pain, but, if the canal is congenitally abnormally narrow, there is pain, the condition being known as obstructive

dysmenorrhœa. Again, if the mucous membrane of the urethra swells so as to occlude its canal, there is pain and obstruction to the flow of urine; as a consequence of the congestive stricture the urine has to be drained off. Further, the muscular coat of the bladder, in cases in which there is any obstruction to the flow of urine, becomes hypertrophied and vesiculated, and alters in shape; and a retro-strictural pouch forms. It is therefore quite conceivable that if the mucous membrane of the cervical canal swells, the canal is blocked—as a result the secretions in the uterus become pent up, and in consequence of the obstruction the uterine wall hypertrophies, and is altered in shape.

After examining a large number of cases of gonorrhœal endometritis, septic miscarriages, and puerperal sepsis, we have formed the opinion that, unless the cervical canal remains distinctly open, one is sooner or later faced with a combination of symptoms and signs which, if present in any other viscus of the body, would be recognized as those of obstruction of that viscus.

#### SYMPTOMS AND SIGNS OF OBSTRUCTION.

*Symptoms.*—(1) Those of constitutional disturbance with raised temperature and increased pulse-rate; (2) pain, which is a cardinal symptom of an obstructed viscus.

*Signs.*—(a) At the site of obstruction: (1) Redness and swelling of the mucous membrane, with narrowing and tortuosity of the cervical canal; (2) the canal may contain pieces of placenta, membrane, mucus, blood clot, or polypi.

(b) Above the obstruction: (1) The uterus becomes tender (this is the cardinal sign of an obstructed viscus), enlarged, and boggy; (2) it is often altered in shape, and becomes anteverted, retroverted, or falls to one side; (3) actual pus can often be drawn off from the uterus.

(c) A loaded rectum: The earliest writers were evidently aware that a loaded rectum played a great part in the production of fever during the puerperium. They had arrived at the conclusion that if the bowels became freely open, either naturally, by medicinal means, or by washing out, in many cases the fever decreased.

If the fever be due to absorption from a loaded bowel, there would surely be more cases of pyrexia during the puerperium, since many women tend to become constipated during the early puerperium, unless very careful attention is paid to them during the later months of pregnancy and the early puerperium. After careful observation on a large number of cases, the following anatomical and clinical findings are submitted: If a cadaver be examined when the body is in the dorsal position, the uterus lies in front of, and is actually touching, the wall of the rectum, about two inches of the lowest part of the anterior wall of the rectum having no uterus in front of it. If foreign bodies, such as stones, be introduced into the lowest two inches of the rectum, it will be noticed that the overloaded rectum does not distend posteriorly, being prevented by the sacrum, but protrudes anteriorly and consequently pushes the posterior vaginal wall forwards, so that the anterior and posterior walls of the vagina lie in close proximity. It is therefore apparent that large scybalous masses in the lower part of the rectum could mechanically displace the soft cervix, could get in front of the cervical canal, and further prevent to a varying extent the descent of the puerperal uterus. Clinically, if the vagina is examined in a case of loaded rectum, the examining finger is felt to pass over a distinct hillock and reaches

the cervix above and behind the lump. In one hundred cases, examination was made of the level of the fundus of the uterus below the umbilicus on the third day before and after the bowels had acted (the precaution being taken to empty the bladder beforehand) and it was distinctly observed on each occasion that the uterus descended to an appreciable extent after the bowel was unloaded. During 1918 the records of one hundred consecutive patients confined in our maternity ward were examined. There were in eighteen cases slight rises of temperature occurring between the second and fifth day; in eight it rose on the third day, in six on the second day, in three on the fourth, and in one on the fifth. Galabin, in his "Manual of Midwifery," states that in the cases of puerperal pyrexia reported to the Collective Investigation Committee, the days of onset stand in the following frequency: third, second, fourth, first, and fifth. It will be seen that on the second and third days, when the bowels are likely to become loaded, that the temperature rose on fourteen occasions. In a series of 122 cases seen during the earlier part of 1923, the bowels were carefully washed out for the first three days of the puerperium, with two or three pints of saline. In most a very good result was obtained on the third morning, and a good result was often obtained on the second day. Of this number the temperature rose slightly on six occasions. They included fifteen forceps cases, eighteen with stitched perineums, and nine with very offensive lochia. That the rectum plays an important part in damming back the uterine secretions, was well demonstrated in a patient whose chart will be shown. She was admitted with fever on the third day after delivery; 5 c.c. of offensive lochia were drawn off from the uterus, which was drained for two days, and the temperature fell to normal. Nothing further was done, but the temperature again rose after forty-eight hours. The woman had the appearance of sepsis, and the lochia were not so free. P.V., the lowest part of the rectum was found to be filled with scybalous masses, the cervix being below and behind the faecal mass. It seemed so evident that a loaded rectum was impeding the secretions coming down from the uterus, that the patient was taken to the treatment room and the scybalous masses were removed with the finger. The patient was taken back to bed and placed in the semi-Fowler position, and immediately the temperature began to drop and the lochia became more profuse. It seems beyond doubt that rises of temperature in some cases may be caused by a loaded rectum obstructing the lochial flow. From the foregoing observations, we have formed the opinion that in a large number of cases the cause of the rise of temperature is absorption of toxins from the pent-up secretions in the uterus, and not absorption through the wall of the bowel. It should, however, be remembered that the uterus is the infected organ, and ought to be drained as well as the rectum emptied.

(d) Swollen Perineum: If the perineum becomes oedematous after labour, it certainly provides a barrier to the proper exit of the lochia. This oedema is especially in evidence after rupture and stitching. An escape of pus is often noticed after insertion of the finger into the lower part of the vagina or after removing the stitches.

#### THE POSITION OF THE UTERUS AS AFFECTED BY THE POSITION OF THE PATIENT.

It is now well established that one of the best means of promoting the drainage of the uterus is obtained by placing the patient in the semi-Fowler position as soon as possible after delivery.

For the purpose of discussion, we will consider three types of case: (1) Puerperal sepsis; (2) septic miscarriages; (3) acute endometritis associated

with constitutional disturbance. In the first two the question of retention products will be considered, whereas in acute endometritis the uterus will be taken to be empty. In all, if sepsis intervenes, we may be dealing with (1) uterine infection; (2) uterine infection plus infection of the tubes, peritoneum or parametrium; (3) uterine infection plus blood infection. It will be our endeavour to prove that in order to give the patient the best chance of recovery, it is necessary to drain the genital tract thoroughly.

#### PUERPERAL SEPSIS.

##### (1) *Diagnosis.*

It is, in our opinion, of paramount importance that pyrexia occurring during the puerperium should be considered as due to absorption from the genital tract, and since our methods have proved that entrance into the uterus does no damage, it follows that the first duty of the practitioner is to see that the tissues of the genital tract and the uterus are draining. After a day or two, there may be another source for the rise of temperature, but by procuring drainage no time has been wasted. It is true that constitutional disturbance may apparently be accompanied by normal lochia, but no reliance can be placed upon this sign. Constitutional disturbance, more or less severe, may be the only indication of early puerperal infection.

##### (2) *Routine Examination of the Patient.*

A thorough examination is made of the patient, including examination of the bladder, vagina, uterus, and blood. Smears from the vagina and cervix, and cultures from the urine and blood are examined bacteriologically.

##### (3) *The Advisability of Exploring the Uterus.*

It has been suggested that the two following complications are indications for exploring the uterus:—

(i) Hæmorrhage, which may be (a) primary, or (b) secondary; (ii) sepsis.

(a) *Primary Hæmorrhage.*—Excessive hæmorrhage occurring just after labour, when the uterus shows no signs of retracting, is certainly a reason for investigating the interior of the uterus, for the purpose of removing any retained piece of placenta. Exploration of the uterus for pieces of membrane or blood clot has not appeared necessary, since they have come away by drainage.

(b) *Secondary Hæmorrhage.*—This is easily dealt with by drainage and intra-uterine treatment with glycerine, especially if some tincture of iodine is added to the glycerine. This stimulates the uterus to contract, which brings away any pieces of retained products, and acts in a far quicker and more efficacious manner than ergot or other uterine stimulants. If it is thought advisable to explore the uterus, it is considered absolutely essential that the uterus should be drained for the next few days.

(ii) *Sepsis.*—For some time we have believed that uterine exploration with the finger, curette or swab, is not only unnecessary for the removal of retained products, but is also extremely dangerous. With suitable drainage, these septic products are sooner or later expelled, when, as the result of the drainage, the uterus recovers its tone: if presenting at the os, they can easily be seized with suitable forceps and gently twisted round and removed.

THE CONSEQUENCES OF NOT THOROUGHLY AND METHODICALLY DRAINING  
THE UTERUS UNTIL IT HAS BECOME INVOLUTED.

Patients are left with feelings of constant tiredness, backache, and leucorrhœa. They may have menorrhagia and metrorrhagia, or, what is far worse, periodic attacks of fever associated with uterine colic, obstruction, salpingitis, and peritonitis. These cases might be described as suffering from subinvolution fever, which often entails great difficulty in diagnosis. A smear taken from the cervical canal is often pronounced to be negative, so that we may be thrown off our guard, but on careful inquiry the patient will tell us that her periods are more profuse and more painful since the time of her previous confinement. Cases have been seen in which there was apparently no cervical erosion and no discharge, but the uterus was enlarged and boggy. It seems certain that this is one of the explanations of the periodical attacks of pain and fever with which we are very often confronted. The patients in these cases quickly recover, and further feverish attacks are obviated, if thorough intermenstrual treatments are adopted, with the express purpose of putting the endometrium into a healthy condition, and reconstituting, if possible, their normal menstrual functions. The trouble has been caused by a chronic endometritis, resulting in periodical attacks of so-called acute endometritis.

*Case of E. R.—History.*—Full time pregnancy sixteen years ago. Temperature rose on the third day after delivery. Occasional hæmorrhages and pain for three months. The effect on the periods: they were very excessive and lasted six or seven days, with pain a few days before and during the period. She has often stayed in bed for one week during her periods for years. She has had attacks of pain and rise of temperature seven times, staying in bed for weeks on each occasion.

*Effect of Treatment.*—Treatments were given between her periods. First occasion, three intra-uterine applications of pure glycerine with drainage. The effect on her next period—there was no pain before, but some pain during the period. Second occasion, three intra-uterine applications of pure glycerine. No pain before, during, or after period. Third occasion, two intra-uterine applications of pure glycerine, and one of tincture of iodine and glycerine, equal parts. Effect on periods—no pain before, during or after periods, and the loss much the same as she had before pregnancy.

SEPTIC MISCARRIAGE.

Septic miscarriage is comparable with puerperal sepsis, differing only in that the former is more liable to be due to retained products of conception. No attempt is made to remove any retained products, except in the case of pieces that present at the os, or when there is severe hæmorrhage. By far the worst cases admitted to St. Mary Abbot's Hospital have been those in which attempts have been made to remove the retained products, either by the finger or by the curette. The cause of the rise of temperature is the retained septic secretions, and not necessarily the products themselves. This fact has often been demonstrated by first draining the uterus, when the temperature falls before the expulsion of the contents. After treating many cases of septic miscarriage by drainage, we are now sure that our simple method, i.e., the insertion of a small terminal-eyed catheter into the cavity of the uterus with a little glycerine, does not injure Nature's barrier to infection, allows septic secretions to escape, and helps the temperature to fall before the expulsion of the septic products.

If a curettement has been performed, there is all the more reason for immediate drainage, whether the infection is still confined to the uterus or the inflammation has spread to the surrounding tissues, or the organisms have infected the blood stream. Severe hæmorrhage is the only symptom that may demand the emptying of the uterus.

## ACUTE ENDOMETRITIS.

It has been generally held that in endometritis, associated with acute symptoms and signs (whether the inflammation is limited to the endometrium or has spread to the adnexa), no treatment should be directed to the cavity of the uterus.

For the moment let us trace the sequence of events after infection of the cervix, and see what evidence there is of the spread of the infection to the endometrium. A careful examination and inquiry of the patient is made with regard to any alteration of the character or quantity of her menstrual periods, following infection. As a result of these examinations and inquiries, the cases will fall into two distinct categories. In one, there is no alteration in the character of the menses; in the other, there is a distinct alteration in the period following infection, in the direction of pain and increased flow. But it is highly probable that in both classes the endometrium becomes infected at the subsequent period.

In the first type, where there is no pain or increased flow, there is obviously no obstruction to the menstrual flow through the cervical canal; the only evidences of endometrial disease consist in the character of the purulent discharge, which is thin and watery, and the lack of response to the treatment; any cervical erosion fails to heal until the endometrium has been treated.

In the second type, it is only too obvious that the endometrium is infected, as shown by increased pain and menorrhagia. The cases are often described as examples of congestive dysmenorrhœa.

The symptoms in both types may continue indefinitely, or the patients may quite suddenly present themselves with those acute symptoms and signs which are described as acute endometritis, with or without tubal infection, &c. The medical attendant has to determine whether he is dealing with simply a bacterial infection of the lining membrane of the uterus, or whether there is some other equally or more potent factor at work. If there is, and no effort is made to relieve the associated condition, the patients are left with pain and discomfort indefinitely. Let us examine a typical case and treat it.

*Case I, M. C.*—The patient was taken suddenly ill with pains in the lower abdomen and removed to a London hospital, where she remained a month, receiving douches, glycerine tampons, &c. She was discharged and, feeling no better, was admitted to the Infirmary.

*Condition on admission.*—Pains and tenderness in the lower abdomen, temperature 100° F., pulse 102. P.V.: Slight erosion of cervix, uterus enlarged, boggy, tender and retroverted with distinctly impaired mobility.

*Treatment.*—After most careful preparation, a tiny tube, well lubricated with glycerine, was gently inserted up to the fundus uteri. A few drops of sterilized glycerine were injected, and the tube left in for drainage.

*Result.*—The next morning the patient stated that the pain had left her in two hours after the treatment was given, and that she had the first good night's rest for one month, and that she was a thousand times better. Her temperature and pulse were normal.

There is another fairly common type of case, which often puzzles the surgeon as to whether he should open the abdomen or not. There is pelvic peritonitis, and it is doubtful whether the inflammation has spread by way of the uterus, or whether it comes from the appendix. Most of these cases can wait four or five hours—in fact, it takes nearly that time to prepare the patient for operation. The time can usefully be employed in inserting into the uterus a tiny terminal-eyed catheter, with a few drops of glycerine. If there is a

rapid decline in the symptoms and signs, surely this is a proof that the uterus contains pent-up secretion, and that it is the primary focus. The procedure is a valuable aid in the diagnosis of the case.

*Case II, E. L., aged 35.—Acute endometritis, pelvic peritonitis and right salpingitis.*

*History.*—The patient had been treated for a similar attack at a London hospital six years before, where operation was recommended. Her periods had been excessive for a long time, with yellow discharge between times. She was suddenly seized with intense abdominal pain, and felt sick the day before admission.

*Condition on Admission.*—Well marked tenderness and rigidity over the lower abdomen, and much pain in the right iliac fossa. Uterus enlarged, impaired mobility. Marked fullness in the right iliac fossa.

4.15 p.m.—Treatment. Temperature 101° F. Pulse 112.

8.30 p.m.—General condition much improved. Tenderness and rigidity much diminished. Pain much less. Temperature 99° F. Pulse 86.

#### *What are the Cardinal Symptoms and Signs of these Conditions?*

(1) Constitutional disturbance. (2) Pain. (3) The mucous membrane of the cervical canal is seen to be swollen, and bleeds easily if touched with a rigid instrument. In other words, the canal is occluded. (4) The uterus is enlarged, tender, boggy, and distorted, and often contains pent-up secretions which can be demonstrated.

In the opinion of many observers, the name "acute endometritis" conveys only a partial truth, for the cases really present all the symptoms and signs of an obstructed viscus. They are examples of uterine colic and absolute uterine obstruction. It has been proved that when treated as such, by inserting a drainage tube and glycerine, far better and quicker results can be obtained.

#### THE OBJECT AND METHOD OF DRAINING THE UTERUS.

Having shown that the symptoms and signs of acute infections of the uterus are due to retention of septic discharges, our first duty is to drain the uterus. We will consider drainage under two headings.

- (1) The insertion of a drainage tube (drainage of the uterine cavity).
- (2) Introduction of drugs to promote exosmosis from the endometrium (drainage of the uterine tissues).

##### (1) *Drainage Tube.*

Experience from many cases indicates that with the exception of the first few days of the puerperium, septic discharges of the uterine cavity do not drain completely through the external os. Drainage in most cases is only partial, for although there may be copious discharge from the external os, there is always pent-up secretion in the uterine cavity. Drainage might be obtained by dilatation of the cervix or by inserting and stitching in a rigid tube, but we are of opinion that such drastic treatment of a delicate organ like the uterus would open up fresh channels of infection by splitting the cervix, and break down any protective layer of granulation in the interior. Moreover, the presence of a foreign body in the uterus causes uterine contractions, and if the tube were left in too long, the only result would be further to exhaust the muscular power of the myometrium which has already been weakened by the sepsis. In our opinion this complication must be avoided. We find that if a soft rubber tube, well lubricated with glycerine, be gently and slowly introduced through the cervical canal, any secretion in the uterine cavity will

drain away by its side. If this tube is left in from three to six hours, the tube, acting as a foreign body, produces uterine contractions, which force out the secretions beside the tube, but at the end of that time, that is, before the uterus is exhausted, the tube should be removed. By this means we find the muscular tone of the uterus is increased.

### (2) *Drugs.*

With regard to the drugs used in treatment we are less dogmatic. Consider the interior of a septic uterus. In most cases it is lined by a layer of septic exudate, and in some cases it is in a state of coagulation necrosis. The layers deep to the exudate, and even the blood sinuses may be teeming with organisms. Powerful antiseptics and styptics would dissolve off the granulation tissue, and only open up fresh channels of infection, whilst still failing to reach the deeper layers. What we have aimed at, is to produce an exudate from the tissues, trusting that this fresh exudate is rich in bactericidal properties. The most useful drug is glycerine, which causes a profuse outpouring of lymph from the uterus, and at the same time is a mild stimulant to the uterine muscle. In cases in which there is free secretion in the uterine cavity, we find a preliminary gentle irrigation with 5 per cent. saline solution washes away all free septic matter, and allows the glycerine to act on a relatively clean surface. In some cases we add a small quantity of tincture of iodine to the glycerine. This is not used for its antiseptic properties, but as a stimulant to the uterine muscle. We find that the introduction of glycerine containing a small percentage of iodine will arrest moderate hæmorrhage from the uterus.

### POINTS IN DRAINING THE UTERUS.

(1) *Position.*—It is best to place the patient in a semi-Fowler position, as in that position the uterus stands the best chance of draining. If the uterus becomes retroverted, the patient should lie on her stomach several times during the day.

(2) If the perineum has been sutured, all stitches should be removed.

(3) The rectum should be empty.

(4) No attempt should be made to dilate the cervix—for this is unnecessary ; or to catch hold of, or to pull on the cervix with a volsellum, for this manipulation opens up fresh channels for infection, and also alters the position of the inflamed organ—a result which is not desirable.

(5) Presenting pieces of membrane or placenta should be twisted off.

(6) *Kind and Size of Drainage Tube.*—A soft india-rubber, terminal-eyed catheter is far preferable to any rigid catheter. The catheter should be smaller than the cervical canal, as drainage is obtained by the secretions from the uterus trickling down by its side. A tube that fits the canal tightly causes injury and allows secretions to become pent-up. Moreover, a large tube causes painful uterine contractions.

(7) The solution used should be syringed in front of the catheter, so as to make a bed for the easy upward progress of the drainage tube.

(8) The uterus should be freed from any residual fluid or pus.

### INSTRUMENTS REQUIRED.

(1) *Catheter Introducer.*—This is a light, handy instrument and, owing to its small size and the bend in the shank, the operator's visual field is not obscured. When the catheter has been inserted, its lumen is not obliterated, so that



the fluid can be syringed through whilst the catheter is retained *in situ* by the holder.

(2) *Terminal-eyed Catheters*.—Nos. 4 to 8 are used according to the size of the cervical canal. These catheters are marked up to 6 in., and by this means the length of the portion introduced into the uterus can be readily estimated.

(3) *A modification of Sims' Speculum* for introduction into the posterior fornix.

(4) *Anterior Vaginal Wall Retractor*.—This instrument is so constructed that when introduced into the anterior fornix it does not obscure the vision of the operator.

(5) *2 c.c. or 10 c.c. Record Syringe*.

#### TECHNIQUE.

(1) The external parts and vulva are washed with ether soap and water and saline solution.

(2) The vagina is treated in the same way.

(3) A Sims' speculum is introduced into the posterior fornix and an anterior vaginal retractor into the anterior vaginal fornix, and by a series of gentle movements the cervix is manipulated into a central position. The speculum must not be pushed too far back, and the anterior vaginal retractor must be held horizontally. By this means, a good view is obtained of the cervix, which becomes readily accessible for the purpose of passing the catheter.

(4) A terminal-eyed catheter of suitable size is lubricated with glycerine and passed through the cervix to the upper part of the uterus, into which the sterilized glycerine is then gently syringed. The catheter is then detached from the Record syringe, and any remaining fluid is allowed to flow out. This is repeated if the uterus contains much pus.

(5) The catheter is pushed gently up to the fundus, a piece of tape about 4 in. long being attached to it. The end of the catheter is pushed into the vagina and the end of the tape left protruding from the vulva. By pulling on the tape, the catheter can easily be removed by the nurse.

(6) The patient is kept in bed in the semi-Fowler position and made to drink plenty of hot water.

(7) The treatment is repeated as often as is considered necessary.

#### MODIFICATIONS OF TREATMENT FOR TYPE OF UTERUS.

Type 1, *Nulliparous*; type 2, *after Labour or Miscarriage*.

Type 1, *Nulliparous*.—These patients are extremely sensitive, and should be approached with great care. The rapid introduction of a large catheter or of fluid in excess of a few drops, or any strong styptic, is liable to be followed by a degree of shock which in some cases is certainly alarming. The sensitiveness of the individual can only be determined by trial. It is wise, therefore, to give the initial treatment under a hypnotic; sensitive patients may need a general anæsthetic.

Type 2, *After Labour or Miscarriage*.—These patients are relatively insensitive, and unless strong preparations are indicated no analgesic is necessary.

#### FURTHER CONSIDERATIONS.

(1) In all acute cases, i.e., those with pyrexia, drainage should be done immediately and repeated as often as the symptoms indicate.

(2) In general, the larger the uterus and the more dilated the cervical canal, the more glycerine can be used.

(3) In the acute gonorrhœal nulliparous uterus, where the canal is small, a few drops (only ten to fifteen minims) should be injected.

(4) The sequence of treatments is regulated by the amount of reaction to the drug used, it being remembered that if iodine is used with the glycerine, suitable periods must elapse between the treatments. If the secretions become sanious, a maximum amount of reaction for the time being has been produced.

(5) The treatment should be continued until the secretions from the uterus become transparent like glycerine. They should be continued until the uterus is firm and involuted, and there is no cervical erosion. The optimum result of treatment has been reached only when the menstrual periods have again become normal.

(6) If the temperature has been increased by the treatment, it may mean that the uterus is not draining. The smallest tube should then be dipped into glycerine and gently inserted into the cavity of the uterus, so as to evacuate the retained secretions. This also holds good if there is an increase of the pain. If the treatment is properly carried out no increase of temperature should be produced.

#### RESULTS.

##### *Effect of Treatment on Patients.*

During the last three years, 526 cases have been delivered in our own maternity department. Thirty-eight of those were followed by sepsis in the puerperium. There were five deaths. One patient died from puerperal sepsis following plugging of the vagina. The other four died soon after delivery from causes other than sepsis. All the other cases of sepsis were treated by immediate drainage without a single complication arising.

During the last four years, over 3,000 intra-uterine treatments have been given in every kind of septic infection of the uterus, and in all complications arising from those infections. During the last twelve months, 201 cases were treated, and 1,126 intra-uterine treatments given. The following is a summary of the types of cases :—

|                                                                                                                 |     |     |     |    |
|-----------------------------------------------------------------------------------------------------------------|-----|-----|-----|----|
| Puerperal sepsis without complications                                                                          | ... | ... | ... | 48 |
| " " with complications                                                                                          | ... | ... | ... | 5  |
| " " with septicæmia                                                                                             | ... | ... | ... | 6  |
| Deaths 5, all with septicæmia. One case with septicæmia recovered.                                              |     |     |     |    |
| Septic miscarriages without complications                                                                       | ... | ... | ... | 51 |
| " " with complications                                                                                          | ... | ... | ... | 4  |
| " " with septicæmia                                                                                             | ... | ... | ... | 3  |
| Deaths 2, with septicæmia. One patient with septicæmia recovered.                                               |     |     |     |    |
| Acute endometritis including infections with gonococcus, streptococcus and staphylococcus without complications | ... | ... | ... | 4  |
| Acute endometritis with complications                                                                           | ... | ... | ... | 31 |
| Chronic endometritis without complications                                                                      | ... | ... | ... | 34 |
| " " with complications                                                                                          | ... | ... | ... | 2  |

The complications were mainly salpingitis and pelvic peritonitis. There were also three cases of arthritis, twelve of thrombo-phlebitis, four with acute mania, and two with pneumonia. The latter complications are not included in the above statistics.

In all cases of acute endometritis associated with pain, relief was rapid. All moderate hæmorrhages were controlled by intra-uterine treatments, but it was found necessary to explore the uterus on twelve occasions. Exploration was always followed by immediate drainage, and no untoward results occurred.

Although in many of the cases of salpingitis the inflamed tubes attained the size of a tangerine orange, in no case was a tube removed. In two cases in

which the patients were admitted in a moribund condition, general peritonitis was undiagnosed, the inflammation being thought to be limited to the pelvis.

In those cases of subinvolution which have followed septic miscarriages or puerperal sepsis, and which have been left with excessive menstrual periods with marked pain, the patients have been greatly relieved, and their periods have become normal, after thorough drainage of the uterus.

In all cases of femoral thrombosis, arthritic conditions, and acute mania associated with sepsis, the uterus received a thorough course of the treatment. Of the four cases of puerperal mania, three recovered, but the fourth had to be certified insane. In the latter case, there was a bad family history of insanity.

#### RESULTS OF POST-MORTEM EXAMINATIONS.

Histological examinations were made on eight of the fatal cases of septicæmia. In all cases the uterus was removed before any decomposition had taken place. Sections of four cases were cut by Sir Bernard Spilsbury, and of four by Dr. E. H. Kettle. In six of the cases, the superficial tissues of the interior of the uterus were free from infection, but in thrombosed vessels in the deeper tissues streptococci were seen. Of the two remaining cases, one was necrotic, and swarming with organisms and abscesses all over, while the other was free from organisms both superficial and deep. In all, during life, there was evidence of infection of the uterus, as in all cases examination of the cervical canal showed organisms to be present. In all cases except one, the foul discharge cleared up under treatment, but in the remaining case, the one with the uterine wall full of abscesses, the discharge persisted. It is obvious that the patient received insufficient treatment. From these pathological findings, it was quite evident that all the patients required local treatment, and in no case as far as one can judge, was treatment followed by exacerbation of the constitutional symptoms. As a result of the removal of the infection from the superficial parts of the uterus by treatment, it is evident that negative smears from the cervix may be misleading.

#### CONCLUSIONS.

(1) Careful treatment of the endometrium, with draining of the cavity of the uterus, does not produce an extension of existing inflammation, but lessens it.

(2) The cavity of the uterus can be approached repeatedly until the uterus becomes firm and the discharge is abolished.

(3) One treatment is not sufficient to disinfect an infected uterus.

(4) It is safer to drain the uterus after swabbing or curetting, and after the application of strong styptics to its interior.

(5) In a large number of cases of uterine infection with pelvic inflammation, the cause of the pain and discomfort lies not so much in the tubes, &c., as in the uterus, because it is inflamed, heavy and obstructed.

(6) Inflammatory conditions spreading from the uterus to the adnexa, are not only no bar to, but are an indication for, the need for treatment of the endometrium, since the primary focus of infection resides in it. This is proved by the facts that the pain disappears and the physical signs subside much more quickly when the uterus is drained, than when it is left alone.

(7) Unless these lesions outside the uterus are of the grossest kind (large collection of pus in the tubes, pelvic abscess, general peritonitis) no abdominal

operation should be performed, at any rate until thorough and methodical treatment of the endometrium has been tried.

(8) Exacerbations of salpingitis have proved far less frequent if the uterus has been left in a healthy condition.

(9) After treatment of the infected endometrium menstruation becomes painless, and the excessive flow becomes normal in amount.

(10) If the infected tubes are removed, and the uterus is not rendered healthy, patients are liable to attacks of acute endometritis and pelvic peritonitis.

(11) Primary hæmorrhage is the only indication for exploring the uterus after labour or miscarriage. Secondary hæmorrhages can usually be dealt with by suitable intra-uterine treatment.

(12) Cases of retained products producing septic discharges can be treated by drainage alone, and do not necessitate operation. If the uterus be suitably drained and allowed to recover its tone to some extent, the septic products are expelled naturally, or present themselves at the os, when they can be gently twisted out.

(13) When bacilluria complicates the puerperium, it should be borne in mind that inflammation in the uterus is present in some form, and that we must not be misled by the presence of a negative smear and a partially draining uterus.

(14) At no distant date a method will be found for dealing with the organisms which have spread from the uterus to the blood-stream, but even that will be of little avail unless the uterus has been freed from its infection.

#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) congratulated Dr. Remington Hobbs on the use he was making of the clinical material afforded by a Poor-law institution in carrying out a systematic research on the value of drainage and injection of glycerine in cases of uterine sepsis. The material provided by our infirmaries was so often wasted that it was refreshing to find even a single instance of its being utilized for clinical investigation.

Mr. TREVOR DAVIES said that Dr. Hobbs had in his opinion made out a good case that, in certain types of puerperal infection, stasis played a very important part. The application of the principle of drainage to an infected uterus had everything to recommend it. There was no doubt that stasis resulted when the body of the uterus was acutely bent, either backwards or forwards, on the cervix. They had all met with cases in which the correction of the displacement had resulted in a profuse flow of offensive lochia with prompt fall of the temperature and rapid relief of symptoms. A loaded rectum undoubtedly displaced the whole uterus upwards but it might do more than this, as Dr. Hobbs had pointed out, and his explanation of the rise of temperature so frequently met with in these cases, was, he (Mr. Davies) believed, a more rational one than that usually accepted.

A swollen perineum might give rise to retention of lochia in the vagina with subsequent rise of temperature, but in many of these cases the cause of the fever was to be found in the perineum itself. Removal of stitches in the perineum allowed drainage of the infected perineal wound and rapid relief of symptoms.

The treatment Dr. Hobbs had described was, in his (the speaker's) experience, especially valuable in cases of septic abortions and in cases of local sepsis in the uterus following full time delivery. In cases in which a portion of placenta was retained in the uterus his (Mr. Trevor Davies') opinion was still in favour of manual removal, but when the uterus was empty irrigation and drainage should be the treatment adopted.

Undoubted relief of pain followed the treatment in cases of pelvic peritonitis, and resolution of the inflammatory swelling was hastened as a result of treatment. Cases

of "septicæmia," if treated early enough, would certainly benefit as a result of treatment. Unfortunately once the symptoms had declared themselves, the organisms responsible were entrenched in the uterine sinuses, veins and lymphatics, beyond the reach of anything applied to the interior of the uterus. Dr. Hobbs' selection of glycerine for irrigation purposes was an attempt to deal with this difficulty. Dr. Hobbs' method of treatment was a distinct step forward in the difficult problem of the treatment of puerperal sepsis.

Mr. GORDON LUKER drew attention to the risk and danger of introducing a second infection into the uterus if any intra-uterine manipulations were performed without an anæsthetic, and he said it was desirable that special precautions should be taken to avoid this risk.

### Sarcoma of the Fallopian Tube.

By J. BRIGHT BANISTER, M.D.

E. C., AGED 45, was admitted into Chelsea Hospital for Women on December 19, 1922, having complained for two months of increasing abdominal distension and constant dragging pain in the lower abdomen. This pain was relieved on lying down. She also complained of rather rapid loss of weight. Constipation was increasing. No vaginal symptoms.

*On Examination.*—Nothing abnormal was detected in the chest.

The abdomen below the umbilicus was filled by a hard, fixed mass, which was dull on percussion.

Pelvic examination revealed a normal vagina and cervix, but the uterus was indistinguishable from the abdominal tumour and the whole was quite fixed. The pouch of Douglas was filled by an immovable mass, apparently connected with the abdominal tumour.

*Operation, December 22, 1922.*—The tumour was found to consist of an enlarged fibroid uterus, a thickened and adherent left Fallopian tube and a mass of glands adherent to the aorta at its bifurcation, and to the left iliac vessels.

A left salpingo-oöphorectomy and a subtotal hysterectomy were performed.

The subsequent progress was satisfactory for sixteen days, with the exception of moderate fever for the first four days. Early on the seventeenth morning the respirations rose to 40 and the pulse to 150, and death occurred within two hours, probably from pulmonary embolism.

*Pathological Report on a Specimen removed. December 22, 1922.*—Uterus 7.5 by 7 by 8 cm. removed by subtotal hysterectomy, together with the appendages of the left side. The peritoneal coat is smooth and the uterus, as a whole, globular in shape. On section, the wall of the uterus contains numerous interstitial and subserous fibromyomata up to 5 cm. in diameter. The Fallopian tube, 13 cm. in length by 3 cm. in its greatest width, is swollen and congested and the fimbriated extremity is puckered and indrawn. On section, the tube is found to be filled with whitish, soft, granular growth, in which are numerous hæmorrhagic areas. The walls of the tube are infiltrated with growth. The ovary, 6.5 by 2.5 cm., is filled with similar deposits of growth.

Sections show the growth to be a small round-celled sarcoma. Professor Shattock has confirmed the diagnosis of a primary neoplasm of the tube.

Dr. CUTHBERT LOCKYER (President) congratulated Mr. Banister upon his good fortune in discovering a very rare specimen. He agreed that its histology was that of a small round-celled sarcoma devoid of myeloid cells. The growth appeared to have entirely destroyed the muscular coat, and it had permeated into and filled up the stroma

[December 6, 1923.]

of the plicæ so that nothing remained of the original tubal structure except the columnar epithelium which lay protected in crypts formed by adjacent plicæ. The muscularis was represented by thick-walled fully formed intact arteries and a few scattered muscle fibres. The small deposit in the corresponding adherent ovary was more ambiguous, since the cells arranged themselves radially around some of the capillaries after the manner of an endothelioma, whereas in the tubal growth the vessels appeared as isolated bodies in the mass of malignant cells which had destroyed all else of the original tissues. It was a pity that it had not been possible to obtain leave for an autopsy, as the question of the primary seat of the disease and of metastases could not be known without post-mortem examination. Quénu and Longuet in the *Revue de Chirurgie*, 1901, (p. 746) mentioned six cases which they accepted as primary sarcoma of the tube. Four were lacking in clinical data. Three (Dixon Jones) were obtained at autopsies, one a round-celled sarcoma, one a spindle-celled sarcoma, and the third a melanosarcoma. Dixon Jones claimed to have traced the gradual transformation from muscle-cells into the cells of the "myelomata." The patients in all three cases died of internal hæmorrhage, extra-uterine gestation being diagnosed in each case. Janvrin's first case, described as myosarcoma, arose in the muscularis and contained cartilage. Gottschalk's case had the structure of a spindle-celled sarcoma. Speaking of tubal sarcoma generally, it would appear that it was unilateral; it had been known to vary in size from that of a nut to that of an orange. It had produced metastases in the liver, under the peritoneum and in the ovary (present case). Its histology was very variable; myeloid cells were usually found, simple mesoblastic deposits, such as cartilage, might be present, carcinoma and sarcoma might be concomitant (Spencer's fourth case and Clara Eglinton's). There was nothing to show that the neoplasm was preceded by salpingitis, and a noteworthy macroscopic feature in Mr. Banister's case was that the fimbriæ were intact and unaffected.

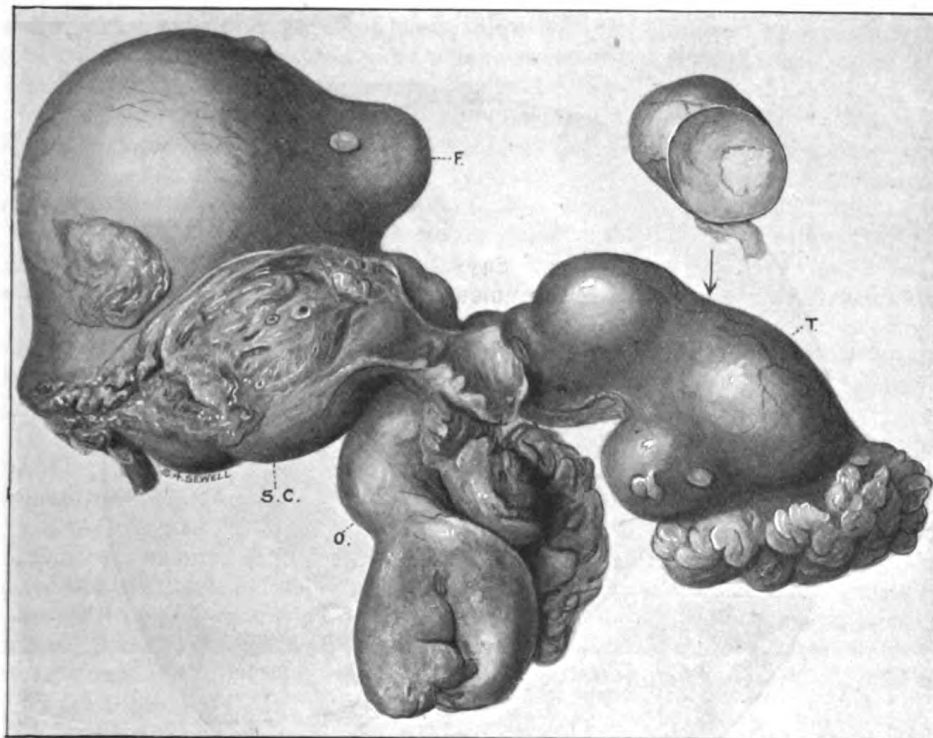


FIG. 1.—F, Fibroid of uterine body. T, Sarcomatous Fallopian tube. S C, Supravaginal cervix. O, Ovary invaded by sarcoma.



FIG. 2.  
Section through wall of tube.  $\times 80$ .

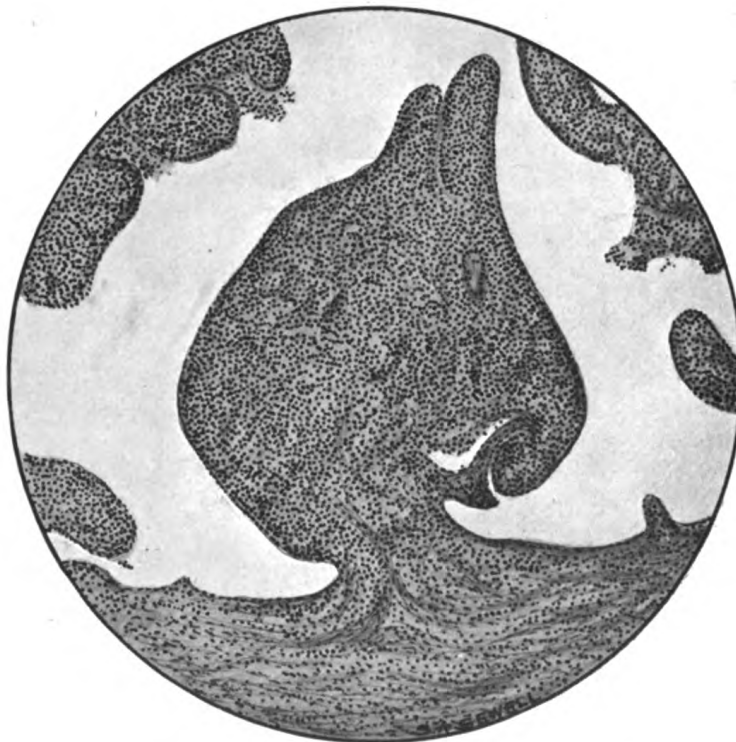


FIG. 3.  
Section through mucous membrane of tube.  $\times 80$ .

## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

### Changes in the Polarity of the Foetus during the later weeks of Pregnancy.

By A. LOUISE MCILROY, M.D., D.Sc., and DOROTHY LEVERKUS, M.B., B.S.

(From the Obstetrical and Gynæcological Unit of the Royal Free Hospital, London School of Medicine for Women.)

THE progress of ante-natal work has thrown fresh light upon the perplexing subject of abnormal foetal presentations, and leads us to question theories advanced by text-book writers and others as to their causes.

According to most authorities pelvic presentations occur 1 in 30 in all labours, 1 in 62 in labours at term (Pinard), 1 in 32·22 (Jellett).

In the Obstetrical Unit pelvic presentations occurred in 1·74 per cent. of all labours of patients who had been under observation in the ante-natal clinic and in the wards. These include cases in which podalic version was performed during pregnancy and at the onset of labour.

Owing to ante-natal work the subject of pelvic presentations has assumed fresh importance. The object of these investigations has been to ascertain the frequency of spontaneous version of the foetus and its influence upon the prognosis and treatment of obstetrical cases.

Very little information has been hitherto available in the literature. To those engaged in ante-natal research it must be obvious that pelvic presentations occur much more frequently during the later weeks of pregnancy than was hitherto generally supposed.

Also, attention has been drawn to the facility with which the foetus tends to right itself in the majority of cases to a head presentation. It is difficult to arrive at any definite conclusion as to the cause of spontaneous version and the persistence of pelvic presentations.

The foetus, with very few exceptions, accommodates its long axis to the vertical axis of the uterus, therefore, we are mainly concerned with changes from cephalic to pelvic presentations and vice versa.

As the foetus has relatively much more power of movement in the earlier weeks of pregnancy when its position cannot be ascertained with any degree of accuracy, the present investigation is confined to the later weeks of pregnancy, more especially from the twenty-eighth week onwards. The normal attitude of the foetus is one of flexion, which enables it to occupy the smallest space possible. The pressure, from the uterus, as the foetus grows in size, tends to maintain it in the bipolar position and in an attitude of flexion. As a head presentation is much more frequent than a pelvic presentation there must be some reason for this fact and various theories have been adopted to support its ætiology.

(1) *The Relationship of the Shape of the Uterus to the Foetus in its Position of Flexion.*—The uterus in its normal pregnant condition is broader at the



fundus than in the region of the lower segment. The nearly full-time fœtus is broader at its pelvic pole than at its cephalic pole. Therefore the most usual position is for the pelvic pole to occupy the fundus, giving the normal cephalic presentation in the neighbourhood of the maternal pelvis.

The uterine walls, as pregnancy advances, tend to maintain this position by their uniform pressure upon the fœtus, or to rotate the fœtus into the most adaptable position possible. If either factor in this relationship is found to be abnormal then a pelvic presentation may result. This is shown by the occurrence of pelvic presentations in cases of hydrocephalus, fœtal goitre, and prematurity, where the head is out of proportion to the body, or where changes in the shape of the uterus have occurred as the result of pathological complications.

(2) *Gravity* (Matthews Duncan) is said to be a factor in maintaining the head presentation, the head floating lower in the liquor amnii than the pelvic pole, the centre of gravity of the fœtus being in the region of the shoulders.

(3) *Activity on the part of the fœtus itself* which enables it, by its own energy, to kick itself out of the region of the maternal pelvis and rotate itself so that the head should occupy the lower zone of the uterine cavity.

If the pelvic pole of the fœtus is in the fundus of the uterus the fœtus has no bony resistance to kick against, and therefore it is more difficult to bring about a rotation which will enable it to occupy the lower portion of the uterus with its pelvic pole.

Many other theories are given for the causation and persistence of pelvic presentations, but as they concern more or less the ætiology of pelvic deliveries they cannot be adapted in many cases of pelvic presentation which occur during pregnancy and which are of a more or less transient variety. Of these theories the following may be mentioned.

(1) *Prematurity*.—In early pregnancy the fœtal head is larger in proportion to the fœtal pelvis than when nearer term, therefore it may adapt itself more easily to the fundus of the uterus although this to some extent contradicts the theory of gravitation of the head.

The frequency of pelvic presentations at delivery is greater in premature cases than at term (1 in 30 as compared with 1 in 62 at term, Pinard).

For the purpose of the present investigation, 1,900 cases were taken which had been followed through the Ante-natal Clinic and completed delivery in the wards. These cases were under the care of the Unit Senior Staff, which included Dr. Gertrude Dearnley, Dr. Catherine Houlton and Dr. Edith Hall.

Pelvic presentations were found during pregnancy at one time or another in 600 cases. Of these thirty-three were delivered as pelvic presentations. In eighteen out of these thirty-three the fœtus was premature. It must be taken into consideration, however, that pregnancy was interrupted or that premature labour occurred in a number of cases owing to pathological conditions of the mother.

Of all cases investigated in which the pelvic pole had presented during pregnancy, twenty-five were delivered prematurely, sixteen of which were primigravidæ and nine multiparæ. They were delivered with the head in advance after the performance of version or the occurrence of spontaneous version.

It would seem as if prematurity was not the main or primary factor in the causation of pelvic polar presentations, but rather that labour had occurred or was induced at a stage when the fœtus was occupying an inverted position in the uterus, just as in a large number of cases we have found the pelvic pole of the fœtus lower at various stages of pregnancy.

If nothing interferes with its continued occupation of the uterus the fœtus will, in the majority of cases, right itself some weeks before labour is due.

(2) *Multiparity*, because of the roomy cavity and lax uterine walls, is said to influence the persistence of pelvic presentations.

Winckel found one in eighty cases of primigravidæ, and one in twenty-three cases of multiparæ. Of the total 600 cases investigated during pregnancy we found that 340 were primigravidæ and 147 in their second pregnancy, and the remaining numbers diminished in quantity as the number of previous pregnancies increased. In the pelvic presentation cases at delivery, the primigravidæ numbered nineteen and the multiparæ fourteen.

The statistics are open to error in the matter of multiparity, as the majority of the cases attending the clinic are primigravidæ and the next in frequency are in their second pregnancy.

If the uterus is lax and its cavity enlarged it should tend to influence the effect of gravity to some extent in bringing about a head presentation. Once a head presentation is established, however, the pressure of the uterus on the fœtus may not be sufficient to keep it in position. In the pelvic deliveries, although the primigravidæ predominated in number, eight of these were premature. This fact, as already stated, complicates the investigation of possible causes.

(3) *Obstruction at the pelvic inlet by tumours, placenta prævia, &c.*, is said to be a cause of pelvic presentations; it may influence pelvic deliveries, but it cannot be said to affect the spontaneous version of the fœtus during pregnancy.

Three cases of fibroid tumours occurred among the 600 cases, and in each instance delivery took place by the cephalic pole. Spontaneous version occurred several weeks before the onset of labour. There were eight cases of placenta prævia (primigravidæ three, multiparæ five), four being delivered with the head presenting.

TABLE I.—PLACENTA PRÆVIA.

*Eight Cases.*

*Cephalic Pole Delivery.*

|                                                                     |  |
|---------------------------------------------------------------------|--|
| 1 case spontaneous version 30th week, delivery by Cæsarean section. |  |
| 1 " " " 37th " " " "                                                |  |
| 1 " " " 26th " " " normal                                           |  |
| 1 " " " 30th " " " normal                                           |  |

*Pelvic Pole Delivery.*

|                                                                       |  |
|-----------------------------------------------------------------------|--|
| 1 case version was performed 38th week, delivery by Cæsarean section. |  |
| 1 " " " 26th " " " as footling                                        |  |
| 1 " " " 32nd " " " "                                                  |  |
| 1 " " " 28th " " " "                                                  |  |

(4) *The age of the mother* had no influence upon spontaneous version. The statistics corresponded to the average reproductive age. Five primigravidæ were between 36 and 41 years. None of the others was over 33.

(5) *The sex of the fœtus* corresponded with the average cephalic rate and had no influence upon spontaneous version.

(6) *A minor degree of pelvic contraction* was observed in a considerable number of cases, but did not seem to bear much relationship to the polar rotation of the fœtus, nor did it influence pelvic deliveries to any great extent, as out of thirty-three pelvic polar deliveries only two cases of pelvic contraction were observed. In every case careful measurements were made at the first examination. The proportion of pelvic contraction cases was about the same as that found in those in which spontaneous version did not take place.

It is possible that pelvic contraction may favour obliquity of the uterine and

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fœtal axis, but further research is being carried out with regard to pelvic contraction.

(7) *Twins*.—Eleven cases of twins were investigated but not included with the other cases. In five cases both fœtuses were born head first; in five cases one fœtus was born head first and the other breech first, and in one case both fœtuses were born breech first. In one of these labour occurred before term. There was one case of triplets in which labour was premature; two of the fœtuses were born head first and one breech first.

It would seem that for better adaptation of two fœtuses in the uterus, less room is taken up when one presents by the head and the other by the breech.

(8) *Hydramnios* was present in four cases of twin pregnancy, but in four only of the single pregnancy cases. It is, however, very difficult to estimate with any degree of scientific accuracy the amount of liquor amnii in any given case either during pregnancy or in labour.

(9) *Deformities of the fœtus* obviously tend to change its normal presentation. In one case only was hydrocephalus observed, and in one spina bifida.

(10) *Other factors* were investigated with regard to their influence upon polar rotation of the fœtus, but gave no evidence of their importance. These were *length and weight of the fœtus* and *length of the umbilical cord*. Before discussing the influence of these factors, it might be well to give some account of the behaviour of the fœtus with regard to polar rotation in the later weeks of pregnancy.

Spontaneous version from the pelvic to the cephalic pole occurred in 451 cases.

TABLE II.—SPONTANEOUS VERSION FROM PELVIC TO CEPHALIC POLE.

| Weeks   |           |     |     |     |     | Cases |
|---------|-----------|-----|-----|-----|-----|-------|
| Between | 20th—24th | ... | ... | ... | ... | 6     |
| "       | 22nd—32nd | ... | ... | ... | ... | 7     |
| "       | 24th—30th | ... | ... | ... | ... | 45    |
| "       | 26th—32nd | ... | ... | ... | ... | 45    |
| "       | 28th—30th | ... | ... | ... | ... | 29    |
| "       | 28th—32nd | ... | ... | ... | ... | 47    |
| "       | 28th—34th | ... | ... | ... | ... | 57    |
| "       | 30th—34th | ... | ... | ... | ... | 61    |
| "       | 32nd—34th | ... | ... | ... | ... | 55    |
| "       | 32nd—36th | ... | ... | ... | ... | 26    |
| "       | 34th—36th | ... | ... | ... | ... | 52    |
| "       | 36th—38th | ... | ... | ... | ... | 11    |
| "       | 38th—40th | ... | ... | ... | ... | 10    |
|         |           |     |     |     |     | 451   |

Spontaneous version occurs most frequently between the thirtieth and thirty-sixth weeks, as a rule before the thirty-sixth week, and most frequently between the thirty-second and thirty-fourth weeks.

Version was successfully performed in sixty-nine cases, the greatest number occurring between the thirty-second and thirty-fourth weeks.

TABLE III.—VERSION SUCCESSFULLY PERFORMED ONCE IN PELVIC PRESENTATIONS.

| Week     | Primigravidae |   | Multiparae |    | Total |
|----------|---------------|---|------------|----|-------|
| 26th ... | ...           | 0 | ...        | 1  | 1     |
| 28th ... | ...           | 1 | ...        | 0  | 1     |
| 30th ... | ...           | 4 | ...        | 3  | 7     |
| 32nd ... | ...           | 8 | ...        | 7  | 15    |
| 34th ... | ...           | 6 | ...        | 15 | 21    |
| 35th ... | ...           | 2 | ...        | 2  | 4     |
| 36th ... | ...           | 5 | ...        | 6  | 11    |
| 38th ... | ...           | 3 | ...        | 3  | 6     |
| 40th ... | ...           | 1 | ...        | 2  | 3     |

Anæsthetics were given in four of these cases. The infants were all born alive. Spontaneous rotation to a pelvic presentation occurred again in five cases, but the fœtus righted itself before onset of labour.

Attempts at version failed in twenty-seven cases, seventeen of which were primigravidæ and ten multiparæ. Spontaneous version occurred subsequently in thirteen cases.

Version was again attempted and successfully performed in eight cases. Failure was found to be due to extended legs in six cases. Anæsthetics were given in eleven cases in which the legs were extended, but version was successful in only five of them. Anæsthesia, therefore, is of little help in rotating the fœtus if the legs and back are extended.

#### TRANSVERSE PRESENTATIONS.

Twenty-four cases were diagnosed as transverse, of which eight were in primigravidæ and sixteen in multiparæ.

Spontaneous version occurred in nineteen cases, in the majority of these between the thirtieth and thirty-fourth weeks, and in two cases at the onset of labour. Version was performed in five cases: one at the thirtieth week, one at the thirty-second week, one at the thirty-fourth week, and two at the thirty-sixth week.

In one of the latter spontaneous version took place to the transverse, and later, spontaneous version to head presentation.

All twenty-four cases ended in vertex deliveries except one, which was a case of hydramnios with pelvic pole delivery. Three cases showed a minor degree of pelvic contraction, one being delivered by Cæsarean section and two by induction of labour.

*Influence of Length of the Fœtus on Spontaneous Version.*—The fœtuses were measured at birth, and the average lengths were found to correspond closely with those of other cases in which the cephalic presentation remained throughout pregnancy. The average lengths were divided into four main groups, which gave in order of frequency 20 in., 21 in., 19 in. and 22 in.; the longest fœtus measured was 25 in.

It was found that the length of the fœtus had little or no influence upon failure to perform version.

In one case of transverse presentation, version was easily performed at the thirty-sixth week, the fœtus at birth being 24 in. in length. Version failed in one case at the thirty-third week, and delivery occurred at term by the pelvic pole. The length of the fœtus was 18 in.

In cases in which failure to perform version occurred the fœtus was found in every case to be of average length at birth. In cases in which version was easy or occurred spontaneously, the fœtus was up to the average length or longer. In some cases in which attempts at version failed, spontaneous version occurred a few days later, when the fœtus was longer. We therefore came to the conclusion that, contrary to our expectations, the length of the fœtus has little if any influence upon its polar rotation.

*The Weight of the Fœtus.*—The infants born at term were of average standard weight. The most frequent weight found was 6 lb. 12 oz.; the next frequent weight found was 7 lb. 12 oz.; the next 7 lb. 4 oz.; the next 7 lb. 8 oz. Spontaneous version occurred in:—

|   |                                               |
|---|-----------------------------------------------|
| 1 | case at the 38th week, weight 10 lb. at birth |
| 1 | „ 38th „ „ 9 lb. 12 oz. at birth              |
| 1 | „ 32nd „ „ 9 lb. 6 oz. at birth               |

Version was performed easily in one case at the thirtieth week, weight at birth 9 lb. 8 oz. In one case version was performed at the thirty-fourth and again at the thirty-sixth week, weight 9 lb. 9 oz. These weights were well above the average and might be expected to influence version.

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In twelve cases in which attempts at version failed, the weight at birth was average or below average. Spontaneous version occurred later in eight of these cases and pelvic delivery in four.

### LENGTH OF THE UMBILICAL CORD.

A short cord has no influence upon the rotation of the fœtus or on failure to perform version.

Spontaneous version occurred in one case of a cord 10 in., two cases of 12 in., one case of 13 in., and nine cases of 14 in. Version was performed easily in one case of a cord 10 in., two cases of 12 in., one case of 13 in., one case of 14 in., and one case of 15 in. Spontaneous version occurred or version was performed in all short cord cases. When version failed the cords were of normal length. In one case of a cord of 12 in. attempts at version were discontinued at the thirty-sixth week, as the patient was faint. Spontaneous version took place at the thirty-eighth week.

As in cephalic presentations, long cords have a greater tendency to loop round the neck or body. The occurrence of loops in the cord was not more frequent than in the control cases.

### PELVIC POLAR PRESENTATIONS.

Thirty-three cases were delivered as pelvic presentations. Of these eleven were in primigravidæ at term and four in multiparæ at term, eighteen were premature, of which eight were primiparæ and ten multiparæ.

Details are appended :—

TABLE IV.—PELVIC POLAR PRESENTATIONS AT DELIVERY. PRIMIGRAVIDÆ AT TERM.

| Case | First diagnosed                | Management                                                                         | Cause         | Results to child                                      |
|------|--------------------------------|------------------------------------------------------------------------------------|---------------|-------------------------------------------------------|
| 1    | 37th week ... ..               | Version not performed as uterus very sensitive                                     | Unknown       | Alive. 7 lb. 10 oz.                                   |
| 2    | 28th week. Vertex<br>32nd week | Not seen later                                                                     | Unknown       | Alive. 7 lb. 10 oz.                                   |
| 3    | 34th week ... ..               | Not seen later                                                                     | Extended legs | Alive. 6 lb. 12 oz.                                   |
| 4    | 30th week. Vertex<br>32nd-36th | Version failed at 38th week under anæsthesia                                       | Unknown       | Alive. 6 lb. 2 oz.                                    |
| 5    | 30th week ... ..               | Diagnosis doubtful at 36th week                                                    | Extended legs | Alive. 6 lb. 12 oz.                                   |
| 6    | 36th week ... ..               | Not seen later                                                                     | Unknown       | Alive. 6 lb. 12 oz.                                   |
| 7    | 32nd week ... ..               | Version failed at 36th week                                                        | Extended legs | Alive. 5 lb. 11 oz.                                   |
| 8    | 34th week ... ..               | Version 34th week, again pelvic pole at 40th week; version failed under anæsthesia | Unknown       | Prolapsed cord, still-birth 8 lb., cord length 18 in. |
| 9    | 28th week ... ..               | Version failed 34th week<br>" " 36th "<br>" " 38th "<br>(under anæsthesia)         | Extended legs | Stillbirth                                            |
| 10   | 32nd week ... ..               | Diagnosed as vertex later                                                          | Extended legs | Alive. 6 lb. 13 oz.                                   |
| 11   | 28th week ... ..               | Diagnosed as vertex at 39th week                                                   | Extended legs | Alive. 7 lb. 12 oz.                                   |

TABLE V.—PELVIC POLAR PRESENTATIONS AT DELIVERY. MULTIPARÆ AT TERM.

| Case | First diagnosed | Management                                                                                            | Cause         | Result to child     |
|------|-----------------|-------------------------------------------------------------------------------------------------------|---------------|---------------------|
| 1    | 38th week       | Version failed                                                                                        | Extended legs | Alive. 6 lb. 8 oz.  |
| 2    | 32nd week       | Version 34th week, spontaneous version again 38th week followed by spontaneous version to pelvic pole | Unknown       | Alive. 8 lb. 6 oz.  |
| 3    | 33rd week       | Diagnosed as vertex later                                                                             | Unknown       | Alive. 5 lb. 6 oz.  |
| 4    | 34th week       | —                                                                                                     | Unknown       | Alive. 6 lb. 12 oz. |

TABLE VI.—PELVIC POLAR PRESENTATIONS AT DELIVERY.  
*Premature. (a) Primigravidæ.*

| Case                             | First diagnosed | Management and complications                                                              | Result                                                  |
|----------------------------------|-----------------|-------------------------------------------------------------------------------------------|---------------------------------------------------------|
| 1                                | 30th week       | Version failed under anæsthesia at 36th week. Footling presentation at labour 38th week.  | Child alive. 5 lb. 8 oz.                                |
| 2                                | 30th week       | Toxæmia severe with œdema, version not attempted as fear of injury to placenta 38th week. | Dead. Hydrocephalus, spina bifida.                      |
| 3                                | 34th week       | A.P.H. 34th week.                                                                         | Stillbirth.                                             |
| 4                                | 32nd week       | Version failed 35th week, with anæsthesia, cause unknown, patient syphilitic.             | Stillbirth.                                             |
| 5                                | 30th week       | Version 30th week, not seen again until in labour at 36th week, extended legs.            | Alive. 4 lb. 1 oz.                                      |
| 6                                | 36th week       | Version failed 37th week, extended legs 38th week.                                        | Alive. 5 lb. 12½ oz.                                    |
| 7                                | 33rd week       | 38th week prolapsed cord, small pelvis, Cæsarean section.                                 | Alive. 4 lb. 13 oz.                                     |
| 8                                | 38th week       | Version at 38th week, central placenta prævia, Cæsarean section.                          | Alive.                                                  |
| <i>Premature. (b) Multiparæ.</i> |                 |                                                                                           |                                                         |
| 1                                | 28th week       | Toxæmia A.P.H. 28th week, sudden onset of labour, extended legs, cord prolapsed.          | Lived four hours.                                       |
| 2                                | 28th week       | Cardiac disease, 36th week.                                                               | Alive. 5 lb. 4½ oz.                                     |
| 3                                | 26th week       | 27th week, cause unknown.                                                                 | Stillbirth.                                             |
| 4                                | 26th week       | Placenta prævia, hæmorrhage at 26th week.                                                 | Stillbirth.                                             |
| 5                                | 30th week       | 33rd week, cause unknown.                                                                 | Did not survive.                                        |
| 6                                | 28th week       | Version at 32nd week, again rotated, version failed at 36th week and labour began.        | Alive. Spina bifida. 6 lb. 15 oz.                       |
| 7                                | 32nd week       | A.P.H. central placenta prævia, induction of labour 32nd week.                            | Stillbirth.                                             |
| 8                                | 32nd week       | Placenta prævia A.P.H. Version and foot brought down.                                     | Lived fourteen hours. Mother died of broncho-pneumonia. |
| 9                                | —               | 25th week induction, pulmonary hæmorrhage.                                                | Stillbirth.                                             |
| 10                               | —               | Placenta prævia, version and delivery 28th week.                                          | Lived two days.                                         |

In the cases at term pelvic presentation was due in seven to extended legs, version having failed in three cases, and two were not diagnosed before delivery. The causes of the remaining cases could not be ascertained, some were not seen more than once or twice during pregnancy, and in others the fœtus again rotated after version had been successfully performed.

Of the premature cases, eight were due to placenta prævia or toxæmia, three to general pathological conditions, and one to contracted pelvis with prolapsed cord in which Cæsarean section was performed.

In three cases labour followed within a few days after version had been attempted or performed.

Seven *fœtal deaths* occurred in the cases of head presentations due to fœtal distress and application of forceps at term.

In all of these spontaneous version had taken place some weeks before term. The other deaths were due to early onset of labour where the fœtus was not viable.

#### EXTENDED LEGS.

The chief factor which influences the persistence of pelvic presentations is seen in extended legs, where the lower limbs act as a brace to prevent the natural curve of the spine taking place. This is one of the most obstinate conditions with which we have to deal, and the most common cause of failure to perform version.

X-ray examinations have demonstrated the extension of the lower limbs and back. If the spine of the fœtus is curved and the limbs flexed rotation easily takes place. Any interference with this attitude may influence the persistence of pelvic presentations. In some cases it cannot be overcome by anæsthesia, which to a certain extent renders the performance of version easy by relaxing the resistance of the abdominal and uterine walls.

Although manipulations under anæsthesia cannot in every case alter the abnormal attitude of the fœtus, it may alter later spontaneously to a head presentation.

In diagnosing pelvic presentations during pregnancy, special attention must be paid to the mobility, roundness and hardness of the head in the fundus of the uterus, and to the groove of the neck, together with the absence of this pole from the lower uterine zone. It must be remembered, however, that in cases of extended legs the pelvic pole is compact and hard, and easily mistaken for the cephalic pole, more especially when it is deeply engaged in the cavity of the maternal pelvis.

We do not find the localization of fœtal heart sounds round the umbilicus to be a reliable guide.

The diagnosis in a large number of the cases was confirmed or contradicted by X-ray examinations, and accurate information as to the attitude of the fœtus was obtained, more especially in cases of persistent pelvic presentations in which the fœtal spine was seen to be extended and attempts at version failed, owing to the rigid condition of the spine and lower limbs.

We are much indebted to Dr. Ulysses Williams and Sister Cooper for their keen enthusiasm and help in procuring skiagrams.

So far no harmful results to the fœtus have been observed as a result of exposure to X-rays.

The weeks in which spontaneous version occurred were charted as near as possible to a particular week, and although, no doubt, errors and mistakes in diagnosis have occurred, the observations have been carried out with the

expenditure of much time and care on the part of the staff. There are gaps in the diagnosis in some cases owing to the patients failing to attend at regular intervals for abdominal examination.

Calculations as to the date of pregnancy were based on the menstrual period as being, of all the unreliable methods, the most reliable. Confirmation was gained by estimating the size and height of the uterus, and the relation of the presenting part of the maternal pelvis.

Briefly, with regard to *treatment*, since spontaneous version occurs so frequently it is as well to leave the foetus alone to adjust itself in the earlier weeks, and as spontaneous version as a rule takes place between the twenty-eighth and thirty-fourth weeks no attempt at interference should be made before the latter date. If version is easily performed it would in all probability have taken place spontaneously, and experience has shown that in some cases of failure spontaneous version has occurred subsequently.

If the pelvic presentation persists in a primigravida after the thirty-sixth week version is advisable, as it may prevent complications in labour, or failure may at least give an indication of the difficulties ahead.

In the case of multiparæ version is seldom indicated as in a number of cases where it has been performed the foetus has again rotated on its axis.

In cases of elderly primigravidæ in which version has failed owing to extension of the legs, delivery by Cæsarean section should be considered, as the results to the foetus are otherwise unsatisfactory.

The chief dangers of version are the following: (a) bringing on of premature labour by sensitizing the uterus; (b) strangulation of the cord; and (c) separation of the placenta by undue manipulation with consequent formation of retro-placental hæmatoma and death of the foetus.

#### SUMMARY.

This investigation has not clearly disclosed the factors which influence pelvic presentations.

Deformities of the foetus can be eliminated to a large extent, as only one case of hydrocephalus was observed.

The length and weight of the foetus, or the length of the umbilical cord have been shown to be of little value as determinant factors in spontaneous version or in the prevention of version.

Excessive liquor amnii may be a factor, but as the percentage of cases was small, much importance cannot be attached to it. Twins, for their better accommodation, have a tendency for one foetus to occupy the lower portion of the uterus with its pelvic pole. The occurrence of pelvic deliveries is frequently due to the premature onset of labour. Deformities of the pelvis are not a marked factor, as only two were found in thirty-three pelvic-pole deliveries.

Other cases of pelvic contraction corresponded in frequency with those found in the control cases.

There were twenty-one cases of induction of labour, mainly for foetal and pelvic disproportion or for cardiac and pulmonary diseases, eight in primigravidæ and thirteen in multiparæ. With two exceptions the disproportion cases resulted in head deliveries.

It was observed that in a considerable number the occiput failed to rotate to the front early in labour, and in many of these cases manual rotation was required.

Globularity in the shape of the uterus has been assigned as a cause of pelvic presentations but our investigations furnish no new information regarding



this hypothesis. It is difficult to estimate the degree of the globularity of the uterus during pregnancy with any fixed standard; in pelvic pole presentations it is difficult to separate the effect from the cause.

In cases of extended legs there can be little globularity present.

The influence of placenta prævia is difficult to estimate owing to the frequency of interrupted pregnancies and the complication of treatment by version to bring about a footling presentation.

There were only two cases in which death of the fœtus might be associated with a pelvic presentation. Tumours occurred in three cases and in all these spontaneous cephalic version took place.

Prematurity is closely associated with pelvic presentations at delivery owing to the onset of labour occurring before the fœtus has had time to right itself by spontaneous cephalic version. According to our experience this frequently takes place.

The onset of premature labour occurs, as it were, at a time when the fœtus is more or less in an unstable position. Had pregnancy advanced the fœtus would have been more likely to settle down in a permanent head presentation, say after the thirty-fourth or thirty-sixth week.

To sum up: While abnormal presentations can in some cases be assigned to specific causes, no reason can be ascribed for the great majority. Why a fœtus which begins with a vertex presentation should be spontaneously transposed into a breech presentation, or why one beginning with a breech should be spontaneously transposed into a vertex, or why a vertex should become a breech and then be re-transposed into a vertex is at present an inscrutable mystery. The development of ante-natal work is likely to extend our knowledge of these strange happenings.

We desire to direct special attention to the table containing details of the cases of spontaneous version from breech to vertex (p 92.) This proves that, in most instances, Nature may be trusted to do what is needful. If, however, spontaneous version is deferred beyond the thirty-sixth week, it is usually wise to resort to version. It may be necessary to perform version in some cases of pelvic presentations, in order to estimate the relation of the fœtal head to the diameters of the maternal pelvis.

As a result of the research no definite conclusions can be reached with regard to the ætiology of the changes in rotation of the fœtus. Further observations are required, and this paper is in the nature of a preliminary communication.

The position of the uterus, the condition of the maternal spine and pelvic curves may be of value for investigation. The force may be found in the fœtus itself. Its intra-uterine movements may, perhaps, be explained by the complicated theory of relativity.

The results of these investigations show the value and importance of ante-natal work. It is only by careful and continuous examination of the pregnant woman from an early stage that we can hope to discover the causes leading to obstetric abnormalities—a prolific cause of morbidity and death in childbirth.

#### DISCUSSION.

Dr. J. S. FAIRBAIRN asked Professor McIlroy whether the diagnosis of presentation made as early as the twentieth week had been confirmed by X-ray examination, as few cases till many weeks after that period of pregnancy admitted of a certain diagnosis by abdominal palpation. Her observation regarding the difficulty of version with extended legs was now so generally accepted that in some ante-natal clinics, failure to perform version was considered the most reliable evidence of extension of the legs.

Dr. R. A. GIBBONS said that Professor McIlroy had read an excellent and most interesting paper, the tables alone showing an enormous amount of work. He would only remark that one point Professor McIlroy had brought out was, he thought, of great practical importance. From these statistics it was seen that spontaneous version from breech presentations to vertex was so frequent before the thirty-sixth week that it was really not necessary to attempt reposition in cases of breech presentations until that date or after. In a certain number of cases under his care he had experienced no difficulty in external version after the thirty-sixth week.

Mr. VICTOR LACK gave details of a case of breech with extended legs in which spontaneous version was observed. A primigravida was seen at the thirty-sixth week of pregnancy, a breech presentation was diagnosed, external cephalic version was attempted and failed. An X-ray photograph confirmed the diagnosis, and showed a breech with extended legs. The next day the patient was sent to the in-patient department for further attempts at version, under an anæsthetic if necessary. On examination the vertex was found to be presenting, and this again was confirmed by an X-ray photograph.

Mr. ALECK BOURNE said that he had seen accidental hæmorrhage follow an attempt to perform external version. He had failed to turn the child after strong manipulation, and the hæmorrhage which followed was attributed by him to some separation of the placenta caused by the abdominal manipulations. Examination of the placenta and membranes afterwards showed that it was normally situated. He further stated that he had noticed a great tendency towards spontaneous version after unsuccessful attempts at artificial external version. So frequently did spontaneous version follow abdominal manipulations which failed to turn the child that he always considered it worth while to try to turn however unsuccessful it might appear to be, as the disturbance of the child's lie seemed to promote spontaneous version. He considered the optimum time for artificial external version to be thirty-six weeks.

Professor MCILROY (in reply to Dr. Fairbairn) said that the diagnosis mentioned was only confirmed by X-ray in cases in which there was any doubt.

### **The Effect of Pregnancy on a Patient with Renal Glycosuria.**

By GEORGE GRAHAM, M.D.

THE case which I am going to describe is important for two reasons: (1) because the patient has a renal glycosuria and has been under observation for thirteen years; (2) because she became pregnant and gave birth to a healthy child.

The patient first came under the observation of Sir Archibald Garrod when she was eight years old in 1911. She was brought to him because she had enuresis, both nocturnal and diurnal. Sugar was discovered in the urine during the course of a routine examination. She was admitted to St. Bartholomew's Hospital for treatment, and used to pass between 20 to 30 grm. of sugar in the urine, whatever changes were made in the diet. She was not dieted very strictly, because she used to become drowsy and pass acetone bodies in the urine whenever the carbohydrates were reduced to small amounts. After some months she was discharged from hospital, and a bad prognosis was given. Two years later she was, however, still alive, and was readmitted for examination. The urine of the other members of the family was examined for sugar; that of the father and mother did not contain any sugar, but that of the brother, aged 12 (only two in the family), contained sugar. He was, therefore, also admitted. Both children on similar diets excreted between 20 and 30 grm. in the day, whatever the diet. The blood sugar was estimated

at that time by Dr. R. L. Mackenzie Wallis, under use of his own method, and was 0.06 per cent. Various other tests were performed and are described elsewhere.

In 1915, at Sir Archibald Garrod's request, I determined the sugar tolerance of the girl after a 50 gm. dose of sugar. The fasting value was 0.1 per cent., and one hour later it was 0.18 per cent., but had fallen to 0.12 per cent. in two hours and to 0.09 per cent. in three hours. The urine in the hour before the test contained 0.8 gm., and 2.8 gm. were excreted in the first hour, 4 gm. in the second hour, and 4 gm. in the third hour. The percentage of the sugar in the urine was 4 per cent. The blood-sugar was not estimated at the end of thirty minutes, which is now our usual practice; but the shape of the blood-sugar curve shows that the sugar tolerance is normal, and as she was passing sugar before the test began, although the blood-sugar was 0.1 per cent., it was clear that the threshold of the kidney for sugar is lower than usual. The exact point at which a person begins to pass sugar in the urine is usually 0.18 per cent., or thereabouts, and if sugar appears in the urine when the blood-sugar is lower than this level, the threshold of the kidney for sugar is said to be lower than usual.

The condition of renal glycosuria was first described in 1896, and although at least 200 cases are now recorded in the literature, some expert workers do not quite accept it as a definite entity. The percentage number of healthy persons who have a renal glycosuria is not yet established, but Folin says that he expects to find at least one out of every hundred students who excrete sugar in the urine, and there are at present two students at St. Bartholomew's Hospital who do so. In one, the condition was diagnosed correctly as soon as it was discovered, but the other boy was taken away from his boarding school, sent to a day school, and dieted for three years, before he entered the hospital.

These patients do not all behave in exactly the same way, as some of them pass sugar all the day long, while others only pass sugar after meals. This probably means that those who pass sugar all the time have the threshold of the kidney for sugar set at a low level, e.g., 0.1 per cent., while those who pass sugar only after the meal have the threshold set above 0.1 per cent., and anywhere between 0.1 per cent. and 0.18 per cent. Of the thirty-four patients whom I have tested, twenty-nine belong to the class who always pass sugar, and five only to the class who pass sugar only after meals. MacLean, on the other hand, has found that the majority of his patients have passed sugar only after meals, and the minority pass sugar all the time. The discrepancy between our observations may be due to the fact that nearly all my patients had been treated for diabetes for some time before I saw them, whereas MacLean's patients have usually been sent to him after an examination for life insurance.

The amount of sugar which these patients pass also varies considerably, since some pass only 2.5 gm., while others pass 5-10, and a few 20-30 gm.

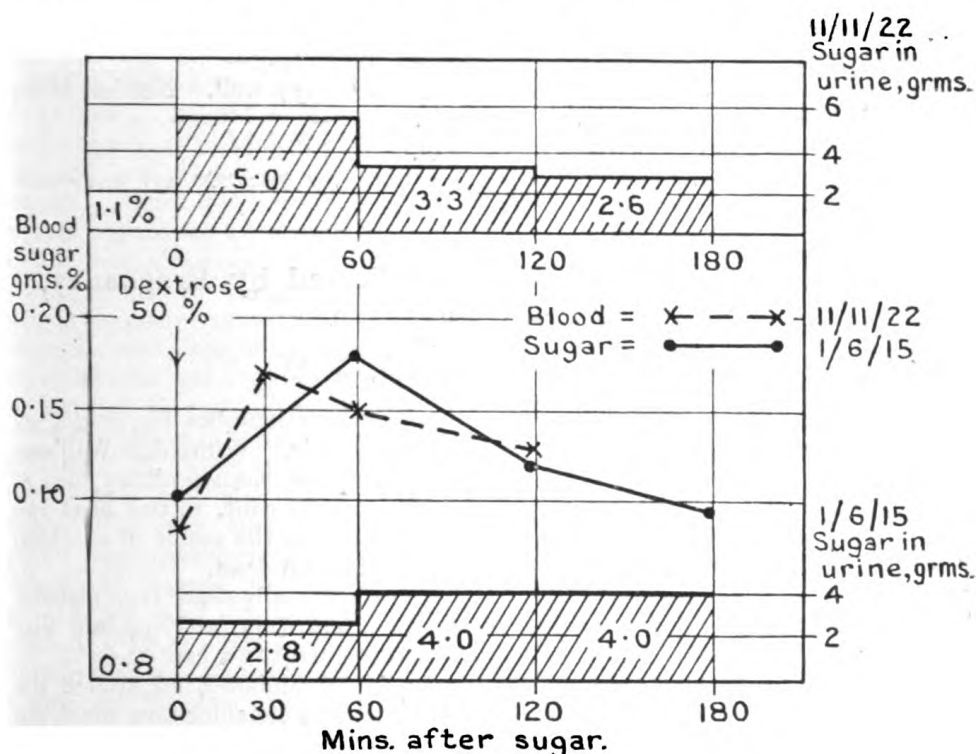
The condition of pregnancy seems to alter the threshold of the kidney, for dextrose often appears in the urine of a pregnant woman although the blood-sugar is not raised above the normal. It has indeed been suggested as a test for pregnancy, and it is alleged that it may give a positive answer as early as the second or third month.

When the patient became pregnant it therefore became an interesting problem to see what effect the condition would have on her. She first went to see Dr. Mary Blair, who found that she had a contracted pelvis and that there was a great deal of sugar in the urine. As she usually passes urine containing 4 per cent. of sugar the condition did appear alarming. When I heard of it

from Dr. Blair I asked Professor Fraser to take her into St. Bartholomew's Hospital so that I could repeat the sugar tolerance curve when she was six months' pregnant. She seemed quite well and had been eating an ordinary diet, including sweets, ever since 1915, although she had been told that she must not eat sweets.

The fasting value of the blood-sugar was 0.08 per cent.: 30 minutes after a dose of 50 gm. it had risen to 0.17 per cent.; had begun to fall after 60 minutes to 0.15 per cent., and in 120 minutes to 0.113 per cent.

The actual shape of the curve differs a little from that of 1915, as the 30-minute point was then omitted, and the 60-minute point was 0.18 per cent. instead of 0.15 per cent. The curve obtained in 1922 is perfectly normal and justifies the deduction made in 1915. The sugar excretion was a little different,



as 5 gm. were excreted in the first hour, 3.3 gm. in the second hour, and 2.6 gm. in the third hour.

She came into hospital under Dr. Barris for the confinement, but I did not do another sugar tolerance curve. The blood-sugar at 11 a.m. was 0.12 per cent., and the total sugar excreted in the twenty-four hours was 36 gm., which is just a trifle higher than the 30 gm. which was the average amount which she excreted in 1913.

The delivery was quite normal and there were no complications of any kind. The urine of the babe was examined on the third day. It was thick with a deposit of urates, and at first I thought that it contained sugar, as the Benedict's solution was reduced slightly, but on filtering away the urates there was no reduction.

## 102 Graham: *Diabetes Mellitus complicated by Pregnancy*

It is believed that patients with renal glycosuria have passed sugar since birth, and as the condition occurs in families it was hoped that we might have the opportunity of proving the point. The youngest subject whom I have examined with a renal glycosuria was a child of 11 months in whom sugar had accidentally been discovered. The child has another rare condition—erythro-cedema—and the sugar may be present as a complication of that disease.

The woman is now very well and still passes much sugar.

The effect of a pregnancy on this patient with renal glycosuria is negligible, and if the diagnosis of renal glycosuria is properly established, a pregnancy is unlikely to have any evil effect.

I know of two other cases: one, described by Riesman, in which the patient had three pregnancies without any ill effect; and the other, the sister of a patient of mine, who has had two pregnancies, during one of which the sugar was discovered. She suffered much from the dieting which she received, but the child survived. She no longer diets, and is very well. Neither of the children has a renal glycosuria.

### REFERENCE.

Graham. *Quarterly Journal of Medicine*, Oxford, 1916, x, p. 295; 1923, xvi, p. 236 (with full bibliography.)

## A Case of Diabetes Mellitus complicated by Pregnancy, Treated with Insulin.

By GEORGE GRAHAM, M.D.

THE outlook of a patient with true diabetes mellitus complicating pregnancy has always been considered as being very unfavourable. Whitridge Williams reports the results of sixty-six cases; 27 per cent. of the women either died at the time of labour or within two weeks of it, and 23 per cent. in the next two years. Of the children, 12 per cent. were born dead as the result of abortion, and of those which came to term, 33 per cent. were born dead.

It is impossible to say how many of the women really had true diabetes mellitus in a severe form, as the data are lacking, but it is fairly certain that the outlook of the diabetic patient who becomes pregnant is a bad one.

The introduction of insulin in the treatment of diabetes has altered the prospects of the diabetic patient, and it is important to consider how much the outlook of the pregnant woman has been altered.

I have had the opportunity of watching one woman who had been treated with insulin before conception and has given birth to a healthy child, and I thought it might be of interest to report the case, although it is only an isolated one.

The patient, aged 34, had already had one child. She was quite well until she had a sudden onset of thirst, and became very irritable, in October, 1922. The sugar was discovered by Dr. Philps about fourteen days after the onset of the symptoms. She was dieted by removal of the carbohydrates of the diet, but not very drastically, and she also did not adhere closely to the prescribed diet. I first saw her with Dr. Philps in June, 1923, as she had become very thin and weak. She then looked ill, and had obviously lost a great deal of weight. She was very constipated, and the abdomen was moving rather deeply with respiration, as though she was approaching coma. The knee-jerks were active, and there was no other sign of disease. The urine contained a great deal of sugar,

and gave a brisk reaction for aceto-acetic acid. The blood-sugar was not estimated then, as the diagnosis was not in doubt. She was treated drastically with two starvation days and five units of insulin on the first day. The urine was sugar-free after the second day, and the diet was gradually increased up to: protein 57 grm., fat 118 grm., sugar 16 grm., caloric value 1,360; calories per kilo 30. The blood-sugar one week later was 0.11 per cent., and insulin administration, 10 units, was begun on the tenth day in order to give the islands of Langerhans as little work to do as possible. She improved greatly in health and strength during the next eight weeks while she was in bed, and gained 1 lb. in weight in spite of the low caloric value of the diet.

The blood-sugar remained between 0.1 per cent. and 0.12 per cent., and the diet was increased to protein 70 grm., fat 126 grm., sugar 21 grm., calories 1,500 on August 29. Sometime about the third week after the insulin treatment was begun the patient became pregnant in spite of precautions. On August 29, 1923, she had missed two menstrual periods, but she seemed very well, and the blood-sugar was normal. I regret now that I did not test out her sugar tolerance with a dose of 50 grm. of dextrose for the sake of comparison with her present condition and with that of other patients. Until December 10, 1923, she kept very well, and was then six months pregnant, and was 9 lb. heavier than in August. There did not seem any indication to interfere, especially as the parents wished for another child. Unfortunately she was developing a severe coryza that afternoon, and the blood-sugar was 0.19 per cent. She was told to stay in bed and increase the dose of insulin so long as she was ill, but for various domestic reasons she did not do so. The illness, which was perhaps influenza, made her quite ill, and by January 4, 1924, she had lost a good deal of weight and felt very weak. The blood-sugar was 0.24 per cent., although she had had 15 units of insulin six hours before the blood was collected.

The question of terminating the pregnancy was considered in consultation with Dr. P. R. Bolus and Dr. F. G. K. Philips, but it was decided not to do anything at that time as it was thought too dangerous while the diabetes was so severe. She was therefore kept in bed on a much reduced diet, and the insulin was increased to 15 units in the morning and 5 at night. On this régime she ceased to pass sugar in the urine, but the fasting blood-sugar was 0.2 per cent. on January 12. On January 26 the blood-sugar had fallen to 0.14 per cent., and the dose of insulin was reduced to 10 units in the morning and 5 units at night. The diet was kept constant at protein 57 grm., fat 117 grm., sugar 16 grm., caloric value 1,360. This is the same diet as she was having in July, 1923, and at that time she was only having 10 units of insulin in the day and the blood-sugar was normal. The insulin requirements of the patient had therefore increased by 5 units, but whether this was the result of the pregnancy or only of the influenza from which she was suffering cannot be stated with any certainty. As she was feeling better but still weak the diet was increased to protein 74 grm., fat 138 grm., sugar 23 grm., caloric value 1,600. Labour began on March, 3 1924, and was conducted by Dr. Bolus. Scopolamine and morphia were given, and the labour terminated very easily. The child was quite healthy, and the patient was not unduly disturbed. During the first ten days of the puerperium the diet was increased by 40 oz. of milk, and 15 extra units of insulin were given to look after the extra sugar. Protein 114 grm., fat 174 grm., sugar 65 grm., caloric value 2,250. The extra sugar was given with the object of having more sugar in the body in a form in which it could be used, so that the patient might be able to deal with any minor septic complications which might arise. Fortunately there were no complications at all.

The increase in the sugar caused her to excrete sugar again, as the insulin was not quite sufficient, but it was thought better to allow her to excrete sugar for a few days than to run any risk of overdosage, as the insulin requirements might have been much less as the result of the termination of the pregnancy. On the tenth day the milk was reduced to 4 oz. and the insulin to 18 units in the morning and 8 units at night.

She made a good recovery, and has felt very well and much stronger.

On March 26, 1924, the blood-sugar was 0.2 per cent., although she was not passing any sugar, and as it was still at this figure on April 5, the milk was stopped altogether and the insulin increased by another 2 units = 28 units.

On reviewing the case it is clear that the sugar tolerance has diminished considerably during the last ten months, but whether it is due to the pregnancy or to the influenza it is impossible to say.

The prognosis of any case of true diabetes mellitus is affected in various ways, and one of the most serious is the incidence of infections of any kind. If, therefore, a diabetic woman becomes pregnant, it seems probable that she will be liable to many more dangers than before, even if the strain of the pregnancy does not cause any ill effects. The special danger is that of sepsis during parturition, as the patient's resistance to this will probably be very small.

Although the present case shows that a pregnancy can be carried through successfully on a diet of low caloric value with the help of insulin, it seems to me that the patient runs a grave risk of the diabetes being made considerably worse.

#### DISCUSSION.

Mr. ALECK BOURNE asked Dr. Graham whether pregnancy lowered the renal threshold for sugar and so produced renal glycosuria in pregnancy as a normal possibility. He had recently observed three cases of renal glycosuria, in two of which the patients were in normally good health, while one was obviously ill, and in all of these the sugar had disappeared after delivery.

Mr. GORDON LUKER asked whether there was any real justification for the accepted belief that diabetic patients nearly always died in labour or just after. He had personal experience of one patient who did perfectly well and had a healthy living child. She was treated during the latter half of her pregnancy according to modern dietetic principles. He thought that pregnancy with diabetes mellitus was very rare, and generally ended in early miscarriage. He would incline towards conservative treatment with Cæsarean section and sterilization at, or near, term.

Dr. GEORGE GRAHAM (in reply) said that the pregnancy almost certainly had an effect on the threshold of the kidney. This change had been put forward as a definite test for pregnancy. The test was carried out by giving a dose of sugar, watching the blood-sugar curve and testing the urine for sugar. It was stated by the earlier workers that the test was correct in 100 per cent. of the cases, but other workers said that it was only true in 50 per cent. He had not himself tested the point.

He thought that the impression that diabetes mellitus was such a fatal disease for pregnant women rested on the old observations of Matthews Duncan, who certainly collected a series of cases in which the outlook was very bad. It must be remembered, however, that the treatment of the disease had been much improved since Matthews Duncan's time. Whitridge Williams' recent figures were not good, and it was probable that with insulin treatment these figures could be improved upon. All the same a pregnancy was a great risk for the diabetic woman to take.

## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### Presacral Tumours of Congenital Origin.

By ALEXANDER GALLETLY, F.R.C.S. Edin.

MRS. V. W., aged 43, was admitted to the Middlesex Hospital, under the care of Mr. Victor Bonney, on May 12, 1923. She gave the following history : At the age of 13, pain of an aching character was felt at the bottom of the spine, and it was about the same time that her parents noticed a fullness of the right buttock. Owing to the gradual increase in size of the swelling, advice was sought at Birmingham a year later, and operation advised. In the same year the swelling was removed through a right gluteal incision by the late Mr. Jordan Lloyd. Eighteen years later, at the age of 32, the swelling reappeared, and a second operation for its removal was performed through a similar incision by Sir Gilbert Barling. She married in 1917 at the age of 37, and in September, 1920, a full-time living child was born, without complications with the exception of a badly torn perineum. She became pregnant for a second time in September, 1922, and it was about this time that she noticed the reappearance of the swelling in the right buttock, which steadily increased in size, but it was not until the end of April, 1923, when she was about eight months' pregnant, that she consulted her doctor. On admission to hospital the following month, the fundus of the uterus had reached the infrasternal angle. The child's head was floating above the pelvic brim, and could not be made to engage. On vaginal examination, a cystic swelling could be felt filling the pelvis, pushing the posterior vaginal wall forwards against the symphysis pubis and the rectum against the left side of the pelvis. The cervix could not be reached. The cystic swelling in the pelvis was continuous with a large cystic swelling in the right buttock and coccygeal region (see fig. 1). Owing to an accident to Mr. Victor Bonney, Mr. Comyns Berkeley performed Cæsarean section on May 30, and a healthy living child was delivered. It was then seen that a retroperitoneal cyst was firmly wedged in the pelvis. The appendages of both sides were noted to be healthy. With the intention of obtaining better access to the cyst the body of the uterus was removed. At the anæsthetist's request, however, no attempt was made to remove the cyst on account of the already rather extensive loss of blood. It was therefore tapped from above and about four pints of light brown fluid withdrawn. After a rapid recovery the patient was sent to the Middlesex Hospital Convalescent Home seventeen days later. On September 3, three months after the operation, she was again admitted to the Middlesex Hospital, where it was found that the cyst had refilled. On deep palpation above the symphysis pubis the cystic swelling with a smooth upper surface could be felt rising from the pelvis.



On September 5, under a spinal and general anæsthetic, Mr. Victor Bonney opened the abdomen through a mid-line subumbilical incision. The cyst was seen to fill the true pelvis, but did not rise above the level of its brim. The



FIG. 1.—Diagram showing gluteal swelling.

upper surface was smooth and covered by peritoneum. The bladder was raised and the rectum pushed to the left side of the pelvis.

The peritoneum covering the cyst was divided transversely, the incision

extending from the one side of the pelvis to the other. The peritoneum was gently separated from the cyst wall and the cyst separated from surrounding structures in the pelvis as far as the fingers could reach. The ureters were isolated on each side and the right internal iliac artery was ligated with the object of diminishing bleeding from its branches at the next stage of the operation. The abdominal incision was temporarily closed and the patient placed in the left lateral position with the thighs slightly flexed. An incision was made extending from the lower end of the sacrum outwards and to the right for 8 in., passing  $1\frac{1}{2}$  in. above the ischial tuberosity. The gluteus maximus was retracted outwards after the division of some of its innermost fibres and the isolation of cyst wall. The surrounding structures had formed a false capsule, in the wall of which there were masses of yellow pultaceous material ranging from the size of a pea to that of a hen's egg, each with a definite capsule. These masses were only loosely attached to surrounding structures and could easily be enucleated. The cyst was isolated as far as its exit from the pelvis through the sacro-sciatic foramen, and it was during the attempt to isolate it from the latter structure that the cyst wall ruptured and about four pints of light brown fluid escaped. It was then seen that most of the fibres of the right sacro-sciatic ligament had been divided at the previous operations and that the coccyx and the right side of the lower end of the sacrum had been removed. The cyst wall was now easily removed and an excellent view afforded of the pelvis and its contents from below. A rubber drainage tube was inserted and the gluteal incision closed.

The patient was again placed on her back and the abdominal wound, which had been temporarily closed, opened. The edges of the divided peritoneum were brought together and the abdomen closed without drainage. On discharge from hospital she walked well. Micturition was normal, and the bowels acted regularly. Quite recently she wrote to say she feels very well, and can do her housework as well as look after her two children.

The cyst (fig. 2) consists of a pelvic portion and a larger gluteal portion, the one constricted off from the other by the margin of the sacro-sciatic foramen. The lining of the cyst was everywhere smooth except where its wall lay in contact with the rectum. At this spot there were several intracystic jelly-like tufts. The wall of the cyst varied in thickness from 1 mm. to 1 cm. The intra-pelvic portion was thicker than the extra-pelvic portion. The thickest part was that from which the intracystic tufts took origin.

On microscopic examination of sections taken from different parts of the cyst, it was found that its wall consisted of fully formed fibrous tissue and its inner surface was covered by squamous epithelium which, in places, was showing keratinization (fig. 3). No trace of hair follicles, sebaceous glands or sweat glands could be found. In the wall of the cyst were to be seen groups of cells containing pigment and in the neighbourhood of the intracystic tufts the squamous epithelium was replaced by a layer of granulation-like tissue in which were numerous foreign body giant cells (fig. 4). The intracystic tufts themselves were found to be composed of groups of tubules lined by columnar cells of intestinal type lying in a stroma of cellular fibrous tissue (fig. 5). No evidence of muscle, fat, bone or cartilage.

The specimen is a presacral cyst and it is to arrive at a conclusion regarding its probable source of origin that I propose at the outset to divide, from the clinical aspect, presacral tumours of congenital origin into:—

- (1) Those which are always present at birth.
- (2) Those which are not as a rule discovered until puberty or later.



FIG. 2.—The pelvic portion of the cyst is opened posteriorly to show the intracystic tufts.

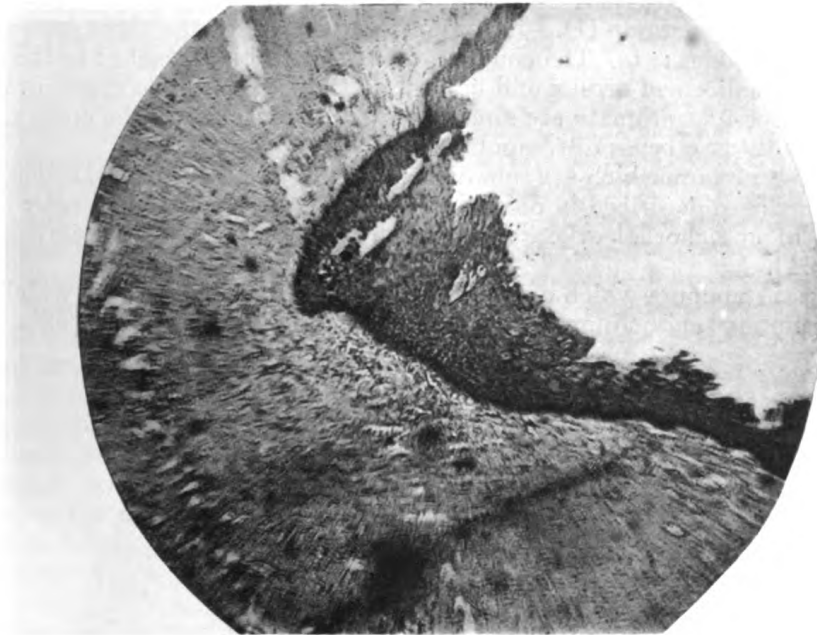


FIG. 3.—Section showing cyst lined by squamous epithelium.

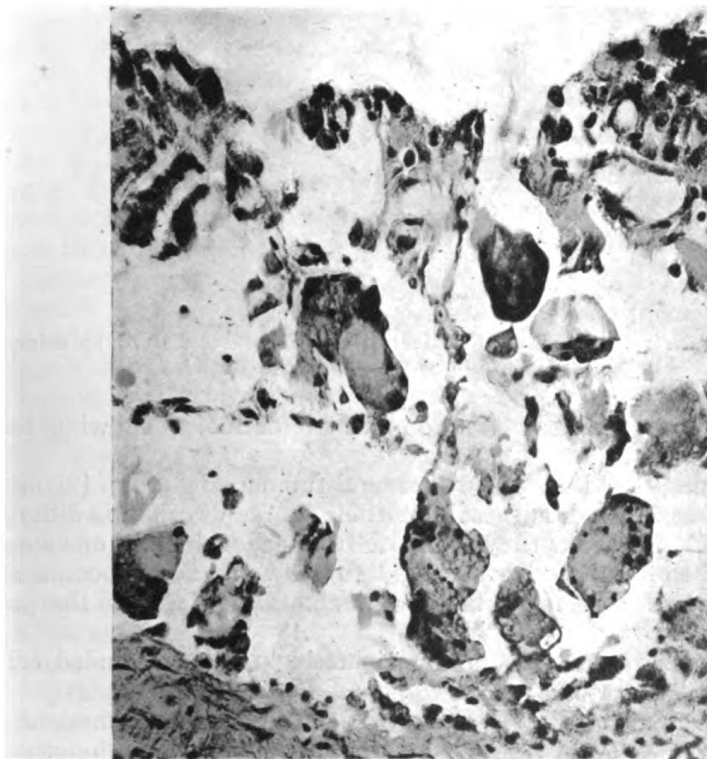


FIG. 4.—Section showing foreign body giant cells.

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The so-called congenital sacro-coccygeal teratoma is the only presacral tumour belonging to group (1). It contains derivatives of the three embryonic layers and resembles: (a) The common ovarian dermoid, attached to the wall of which are malformed organs and limbs; (b) the rare multiple cystic ovarian teratoma. These teratomata are almost invariably attached to the coccyx.

In group (2) two types of tumours occur.

(A) Solid tumours which are subdivided into: (1) Those of neural origin, one of which has been described in detail by Law [11], to which I shall refer later. (2) Those of notochordal origin which have recently been fully described by Stewart [24].

(B) Cystic tumours which are subdivided into: (a) Simple cysts which are lined by columnar epithelium or by squamous epithelium resembling skin with

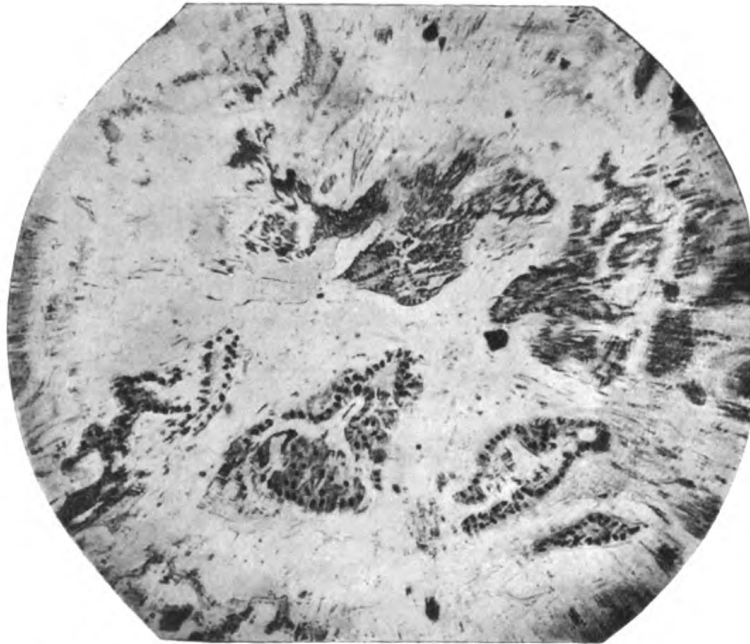


FIG. 5.—Section through an intracystic tuft showing tubules lined by columnar epithelium of intestinal type.

or without skin appendages; (b) Complicated dermoids showing teeth and probably other structures.

On the assumption that these presacral tumours in group (2) are not as a rule present at birth, I suggest that their source of origin is different from those in group (1), and that they originate from cells which at one stage of the development of the embryo have ceased to grow and have become seclused. In some cases puberty seems to be the stimulus which renews the growth of the seclused cell.

A common example of a tumour originating from a seclused cell is the ovarian dermoid or teratoma.

On the inner side of each Wolffian ridge a thickening of mesoderm takes place, covered by a layer of thickened epithelium which is continuous with the

epithelium lining the body cavity. This thickened mesoderm is known as the genital ridge and its epithelial covering as the germinal epithelium.

By the continued growth of the genital ridge a mesentery is formed and the future sex gland is represented by a core of connective tissue covered by germinal epithelium. In the female the sex cells or primitive ova are found between the cells of the germinal epithelium, and the latter, by a process of invagination, carry the primitive ova into the ovarian stroma and thus the primitive Graafian follicles are formed. Beard, who carried out his researches on the skate, denies that the sex cells are derived from the germinal epithelium. He believes that before there are any signs of an embryonic area and during the later stages of segmentation of the ovum, the sex cells are set aside from the cells which are going to form the future embryo, and that these sex cells arrive at their destination in the sex gland by what he calls the germinal path. Beard states that these sex cells are distinguished from the cells which are going to form the embryo by the absence of mitotic figures, their irregular shape and amœboid movements and by their altered staining reactions. That the cells formed in the early stages of segmentation of the ovum are totipotent and capable of producing individual embryos is confirmed by the experiments on certain lower animals, notably the frog, the jelly fish, the newt and the sea-urchin.

Driesch succeeded in isolating the blastomeres of sea-urchins' eggs in the two- and four-cell stages and obtained normally-formed specimens. As Bland-Sutton [3] puts it: "Since the discovery that mechanical stimulation is sufficient to start the segmentation in an ovum it has become a commonplace experiment in the laboratory to irritate the eggs of frogs and produce what are called 'fatherless frogs.' Ovarian dermoids might be not inaptly called fatherless embryos."

Should the sex cells, which are potential fatherless embryos, fail to reach the sex gland and be secluded elsewhere in the retro-cœlomic tissues it is easily understood how chance teratomata may arise.

It is common knowledge that teratomata of the ovary are almost never present in children under 12 months old and very few cases are reported occurring in girls under the age of puberty.

Granted that the ovum is the equivalent of a totipotent blastomere, and that an ovarian teratoma originates by parthenogenesis from a secluded ovum, it is not surprising that other teratomatous growths derived from cell-seclusions are not, as a rule, present at birth. Everyone is agreed that the simple dermoid cysts which occur in the middle line of the trunk and neck and at the junction of the facial clefts, the result of included epithelium, are not always present at birth. These teratomata, the result of cell-seclusion, Bonney [4] termed "Endogenous Teratomata." When the other site of election for teratomata, the sacro-coccygeal region, is examined, it is found that teratomata, which contain derivatives of all three layers of the embryo, are, so far as my knowledge goes, always present at birth. Such a fact is most striking, the more so when we consider the deep relations of the ventral surface of the sacrum and how easily such a tumour could evade detection at birth. It must therefore be assumed that the starting point for such teratomata is somewhere near the tip of the coccyx and they follow the path of least resistance and appear on the surface. As well as the congenital sacro-coccygeal teratoma there is another teratoma which shows derivatives of the three layers of the embryo and is always present at birth, and that is the teratoma which occurs in the region of the basi-sphenoid and is known as epignathus. It grows from

the roof of the pharynx and projects into the mouth or less frequently grows upwards into the cranial cavity in the region of the sella turcica. Such teratomata, which are always present at birth, must be analogous to congenital malformations such as conjoined twins, bicephalous monsters, &c., the result of dichotomy of the foetal axis. These Bonney termed exogenous.

That dichotomy of the foetal axis offers an explanation for conjoined twins and bicephalous monsters, is supported by the experiments of Speman, who was able to produce two completely separated embryos up to all stages of monstrosity by tying a fine hair round the developing ovum of the newt parallel to its long axis while it was still in the gastrula stage. If dichotomy is complete, equal and symmetrical the result is monochordal twins. If dichotomy is all but complete and the dichotomized parts are equal symmetrical conjoined twins are produced. Superior dichotomy gives rise to a double-headed monster, inferior dichotomy to supernumerary legs, and so on. Superior and inferior dichotomy have been given as an explanation for epignathus and the congenital sacro-coccygeal teratoma respectively.

More recently the explanation, which has gained most popularity amongst pathologists for such teratomata, is that given by Adami [1], who suggests that they are due to the continued growth of totipotential cells from the superior and inferior growing points after the "anlage" of the foetal axis has been laid down. The anterior growing point is situated in the neighbourhood of the sella turcica, the posterior growing point is situated between the coccyx and the anus. Such an explanation would account for the predilection which congenital teratomata have for the sacro-coccygeal region and for the much less common site, the basi-sphenoid. It will be noted also that such a neoplastic formation is analogous to a congenital malformation and therefore must be present at birth. There are, therefore, two great groups of teratomata:—

(1) Endogenous teratomata taking origin from secluded cells not as a rule present at birth.

(2) Exogenous teratomata taking origin from: (a) Dichotomy of the foetal axis; (b) anterior or posterior growing point cells, always present at birth.

There are several possible sources in the sacral region for tumours taking origin from secluded cells: (1) The termination of the neural tube; (2) the termination of the notochord; (3) the neurenteric canal; (4) the post-anal gut.

If the surface of the ovum of the developing chick is examined the embryonic area is represented by a dark oval-shaped area. There appears a dark line which runs forward from the caudal extremity of this embryonic area and ends about its middle. This is known as the primitive streak, which later deepens from before backwards to become converted into a groove. At this stage two ridges, termed the neural ridges, appear near the cephalic end of the embryonic area where they are continuous and grow backwards on either side and eventually join posteriorly by cutting their way through the cephalic extremity of the primitive groove. This cephalic extremity thus cut off from the remainder of the primitive groove, deepens until a communication is established with the primitive hind gut. The neural ridges unite in the middle line to form the neural tube. The communication between the neural tube and primitive hind gut is known as the neurenteric canal. It will be seen then that the neurenteric canal is formed by an ectodermal invagination.

The proctodeal invagination which is going to form the anal canal joins the hind gut on its ventral aspect a short distance in front of its extremity. That part of the hind gut which lies distal to this junction is known as the

post-anal gut. Both the neurenteric canal and the post-anal gut disappear at a later stage of the development.

Law [11] described a tumour which was obstructing labour in a girl 16 years old. On section the tumour was found solid and consisted of embryonic and fully formed nerve tissue undergoing malignant changes. The origin of such a tumour might be ascribed to a totipotential growing point cell in which the other two embryonic layers had been suppressed. Such an explanation is unlikely in the absence of evidence to show that such tumours are present at birth. Law's tumour was not attached to the coccyx, but took origin higher up in the pelvis and was attached for 1 in. to the periosteum of the middle of the ventral surface of the sacrum. It was not visible on the surface. Such a tumour probably originates from cells which have become differentiated and are no longer totipotential. Law suggests that the tumour springs from some misplaced remnants of the terminal portion of the neural tube or, in other words, from secluded cells.

Another tumour which occurs in the sacro-coccygeal region and also at the base of the skull and which is not always present at birth but of congenital origin, is the chordoma or notochordal tumour. Stewart collected the recorded cases and found that the sacral tumours were closely connected with the dorsal or ventral aspect of the sacrum. In size they range from that of a tangerine orange to that of an adult head. They are lobulated, homogeneous and on section show areas of hæmorrhage. Microscopic examination reveals characteristic mucin-containing cells. They are described at ages ranging from a seven-months' foetus to 55 years. The average age is 47. They are of low malignancy; metastases are rare. Direct spread is common. Such a tumour has been produced experimentally in animals by trephining the ventral aspect of a vertebra before the notochord has disappeared.

Middeldorpf was the first observer to describe a post-anal gut tumour. His specimen was removed by Kraske, in 1884, from a 1-year-old female child. Surrounded by fat, it contained a loop of gut, the blind end of which was loosely attached to the rectum. It discharged its contents through a fistula which opened on the surface between the anus and the coccyx.

On section the gut was found to differ in no way from large intestine. It contained a mucus-secreting columnar epithelium with Lieberkühn's follicles, submucous coat with solitary glands and circular and longitudinal muscle fibres. A serous coat was missing.

If the origin of Middeldorpf's tumour is to be ascribed to the post-anal gut, it is not a tumour in the ordinary pathological sense. It is the persistence of that part of the large gut which normally disappears. No similar tumour has been recorded. On the other hand several congenital sacro-coccygeal teratomata have been reported in which were loops of gut (small and large).

Simple presacral cysts, lined by squamous or columnar epithelium, probably originate from cells of the neurenteric canal. Up to 1899 Skutsch [23] collected seventeen cases of simple dermoid cysts arising in the pelvic connective tissue, including two of his own. They all occurred in females with the exception of one. The two cases recorded by Skutsch were not noticed till after a miscarriage.

Five of the cases he collected complicated pregnancy.

Ord and Sewell [17] recorded a case of a simple skin-lined cyst occurring in a man aged 28. It filled the pelvis and extended into the abdomen as high as the level of the umbilicus. The rectum, the bladder, and small and large intestine were attached to its anterior surface.



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Rutherford [22] and Lund [13] have also recorded similar cases. Rutherford's case occurred in a woman aged 34, who had a normal delivery ten years previously. A cystic swelling in the right buttock, extending into the pelvis for three inches behind the rectum, made its appearance shortly after the birth of her child. A cyst, the size of an orange, was removed successfully by Kraske's method. On microscopic examination the cyst was found to be lined by squamous epithelium and sweat glands.

Lund's [13] case occurred in a woman aged 24. A presacral skin-lined cyst, which she noticed for two and a half years, was partially removed by Kraske's method. The portion of the cyst wall adherent to the rectum was left behind.

Recently Turner Warwick showed me sections taken from the wall of a single cyst which was removed from a man 40 years old. It had been noticed for years, and lay between the sacrum and rectum, and was lined by columnar epithelium, ? ciliated.

Dermoid cysts containing teeth and other structures, which do not appear until puberty or later, are very rare.

Fancourt Barnes [2] recorded such a cyst in a primigravida, aged 28, who was admitted to hospital with a presacral cyst which was obstructing delivery. The cyst, which was the size of a child's head, had displaced the vagina forwards against the symphysis pubis. Porro's operation was performed (the sixth in this country) and a healthy male child delivered. The cyst, after its removal, was found to be lined by skin, and contained teeth in the nodular thickenings of its wall.

Such complicated presacral dermoids probably originate from wandering sex cells or secluded blastomeres.

### CONCLUSIONS.

There are several possible sources of origin of presacral tumours of congenital origin:—

(1) Dichotomy of the foetal axis with production of (A) monster formation; (B) a separated embryo, which later becomes parasitic and (a) blends with the autosite; (b) is included in the autosite; (2) a growing-point cell; (3) a wandering totipotent sex cell; (4) a secluded notochordal cell; (5) a secluded cell of the neural tube; (6) the post-anal gut: (a) the persistence and continued growth of an embryonic remnant; (b) secluded cells of the post-anal gut; (7) the neurenteric canal. In considering the origin of this specimen I should at once exclude 1, 4 and 5.

(1) *Dichotomy of the Foetal Axis*.—All such formations are easily recognized at birth.

(2) *A Growing-point Cell*.—The posterior growing point is situated near the surface between the coccyx and the anus and teratomata resulting from its growth are analogous to congenital malformations, and for these two reasons would be present at birth and easily recognized. Further, the structure of these congenital sacro-coccygeal teratomata resulting from a posterior growing-point cell differs so greatly from that of this specimen that I consider it unlikely that the posterior growing point can be its origin.

(3) *A Wandering Totipotent Sex Cell*.—This source cannot be definitely excluded, but in structure this specimen does not resemble the common ovarian dermoid, nor would it explain the predilection which similar cysts have for the pelvis.

- (4) *A notochordal tumour.* } In no way does this specimen resemble a  
 (5) *Nerve tumour.* } chordoma or a tumour of neural origin.  
 (6) *Post-anal Gut.*—(a) That the cyst is not a loop of gut is obvious; (b) that secluded cells of the post-anal gut are included in the specimen is suggested by the fact that epithelium of intestinal type was discovered in the wall of the cyst, where it lay in contact with the rectum.  
 (7) *Neurenteric Canal.*—That the teratomatous cyst took origin from secluded cells of the neurenteric canal is suggested by the facts that: (a) it was not noticed at birth; (b) after the removal of the original cyst, presumably of a similar nature, other cysts developed. It is not unreasonable to conclude that they took origin from other secluded cells of the neurenteric canal; (c) its structure resembled what would be expected to result from an ectodermal invagination which had effected a communication with the hind gut.

#### SOME RECORDED CASES OF CONGENITAL PRESACRAL TUMOURS.

WHITE (1901) [25]. Female child, aged 8 years. A congenital sacro-coccygeal teratoma between rectum and sacrum. A multilocular dermoid cyst with bone. Removed.

CAMPBELL (1909) [5]. Child 8 months. Congenital sacro-coccygeal tumour. Coccyx absent. Removal.

DAVIS (1910) [6]. Sacro-coccygeal cyst in female child. Almost filled the pelvis and pushed rectum to one side. Attached to coccyx. Removal.

LEDIARD (1912) [12]. In female infant 2 days old. Sacro-coccygeal teratoma between coccyx and anus. Pedunculated. Pedicle 2 in. wide. Tumour removed. Fistula for seven years.

PRESTON MAXWELL and GORDON LEY (1914) [14]. Girl, aged 16. Congenital sacro-coccygeal teratoma. Two dermoid cysts embedded in fat. Attached to wall of one cyst was a loop of large gut unattached to patient's gut. Removed by a two-stage operation. Tumour adherent to coccyx.

WOOLSEY (1918) [26]. Male infant. The congenital sacro-coccygeal teratoma displaced the rectum and scrotum forwards causing retention of urine and swelling of legs. Tumour pressed on iliac veins. Partly extirpation. No recurrence up to time of publishing.

O'KELLY (1918) [16]. Boy, 11 months. A congenital sacro-coccygeal tumour displacing rectum forwards. Bits of organs represented. Removal.

YOUNG (1914) [28]. Female child 11 months. Congenital sacro-coccygeal tumour lying between sacrum and coccyx attached by narrow pedicle of bone. Extirpation.

GWYNNE WILLIAMS (1915) [9]. Female child, aged 8 years. Sacro-coccygeal teratoma between anus and coccyx. Extirpation with coccyx. Tumour adherent to lower end of sacrum.

MONGOMERY (1922) [15] Boy, 5 months. Congenital sacro-coccygeal tumour with fully formed scapula inside.

WOOD [27]. Female infant, 2 months. Sacro-coccygeal cyst weighed 8 lb. No microscopic report.

JOBSON [10]. Girl, aged 15. Congenital sacro-coccygeal [tumour with base as broad as buttocks and reached to knees. No operation.

The following presacral tumours showed malignant changes:

PRINGLE (1907) [21]. Presacral tumour fixed to sacrum. Noticed thirty-five years previously. Two metastatic growths in groin. One appeared thirty-three years and the other twenty years ago.

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GOLDTHWAIT (1920) [8]. An apparent cystic presacral tumour in a woman 44 years old. Difficult childbirth nine years previously. X-ray showed erosion of sacrum extending through to dorsal surface.

PEARSE (1921) [20]. Male, 63 years. Presacral tumour of neural origin excised by parasacral route with recurrence in scar of operation. Cystic. Treated by excision and X-rays. No recurrence.

Kiderlen collected from the literature 122 cases of presacral congenital teratomata (up to 1899).

Parin quotes the following sixteen cases of congenital presacral tumours recorded in the literature from 1900 to 1909 inclusive.

(1) BAYER (1901). Congenital sacro-coccygeal cystic tumour in a boy 9 weeks old. The tumour was situated in the right gluteal region, and was lost upwards in the sacro-sciatic foramen. Examination per rectum showed an upward continuation of the tumour between the rectum and sacrum. Extirpation of the tumour. Cure. The tumour consisted of derivatives of all three germinal layers (*Nakayama*).

(2) LINSER (1901). Second case. Seven months old boy. Tumour as large as a fist, situated mainly on the posterior surface of the sacrum but reaching upwards to the end of the lumbar vertebrae and downwards to the anus. The posterior rectal wall protruded as far as the promontorium. During operation (without anaesthesia) it appeared that the posterior rectal wall was exposed to a great extent. The sacrum was lacking for the most part, coccyx lacking. Complete cure. Teratoid mixed tumour.

(3) LINSER. Third case. Girl, 3 months old. The tumour was only for a small part on the posterior surface of the sacrum; downwards it stretched almost as far as the anus. A large part of it was between the sacrum and rectum. Operation without anaesthesia. Fœtus in fœtu. Cure.

(4) DOBROMYSSLOW (1902). First case. Ten years old child. Tumour on anterior and posterior side of sacrum. Operation. Teratoid mixed tumour. Cure.

(5) DOBROMYSSLOW. Second case. Fifty-four years old man. Congenital tumour in shape of a horse-shoe on anterior and posterior side of sacrum. Fistula. Operation. Permanent faecal fistula. The tumour represented a second parasitic embryo (numerous cysts, glands, thyroid, adrenals, sympathetic ganglion, an embryonic respiratory apparatus).

(6) PENZA (1903). The congenital tumour developed in the hollow of the sacrum. Operation in sixth year of life. Elements of all three germinal layers, and a kind of rudimentary eye found.

(7) N. PETROW (1903). Sixteen-year-old girl. Congenital tumour between sacrum and rectum, reaching partly into the cavity of the false pelvis. Partial extirpation of the tumour together with the coccyx; later scraped out. Healing. The cystic walls were covered with one or more layers of epithelium; many-layered pavement epithelium, ciliated epithelium, lymphatic follicles, glands, transversely striped muscles, nerves and bones were present.

(8) ENGELMANN (1904). Infant, 3 days old, boy, with a tumour larger than a fist, which had pressed the sacrum and coccyx upwards at the back, and the anus downwards. During operation the peritoneum was torn and had to be sewn. Death. Mixed tumour. (Cartilaginous and osseous tissue, transversely striped muscle-fibres, glia, ganglion-cells, nerve-fibres, various epithelia, skin.)

(9) ENGELMANN. Girl, 6 weeks old. Tumour the size of an apple. Had pressed sacrum slightly upwards, the anus downwards. At the operation it appeared that the tumour was adherent to the rectum and sacrum. Cure. Mixed tumour (cartilage, smooth and transversely striped musculature, various epithelia, glands, intestine-like formation, glia, nerve-fibres, ganglion cells).

(10) DE GAETANO (1907). Three-year-old girl; voluminous tumour of the recto-sacro-coccygeal space. The tumour was ulcerated in various places. Operation fairly difficult on account of adhesions with rectum. Death twenty hours after operation. The extirpated tumour weighed 1.155 kg. Teratoma.

(11) ROLANDO (1909). A large tumour of the hip-joint extending into pelvis with suppuration in a 6-months-old baby. Operation with favourable recovery. (Artificial

anæmia by Momburg's method). The extirpated tumour represents a parasitic tumour (enterocysts; the process in the pelvis contains a well differentiated limb).

(12) HEINZMANN (1909). Girl, 6 weeks old. Cystic sacral tumour. Extirpation Cure. After ten years another large tumour formed which was extirpated. (Numerous cysts covered partly with cylinder, partly with pavement epithelium; in the walls smooth muscle fibres, intestinal glands, intestinal follicles.)

(13) MOTSCHALOW (1909). Nine-year-old boy. Congenital tumour with fistula in sacro-coccygeal region. Part of tumour between rectum and sacrum. Operation. Recovery. Teratoma.

(14) MUCHANOW (1909). Girl, 6 months old. Congenital tumour in sacro-coccygeal region, partly on the anterior surface of sacrum. Operation. Cure. Teratoma.

(15) CHWOROSTANSKI (1909). Congenital tumour, as large as a child's head, partly presacral, in a girl 2 months old. Operation. Death half an hour after operation. Cystic mixed tumour.

(16) FEMALE, aged 20 years. Presacral teratoma since birth. Fistulæ behind anus since age of 10 years. Extirpation. Multiple cysts lined with all kinds of epithelium. Loop of intestine resembling small intestine. Other parts of tumour show structures resembling duodenum, fundal region of stomach, large intestine, salivary glands, pancreas, urinary bladder, trachea, nerve tissue, &c.

Parin then cites the following cases of mixed tumours of the sacro-coccygeal region becoming malignant and setting up metastases.

(1) CZERNY. Fifty-five-year-old female patient. A congenital sacral teratoma, which had ulcerated in her forty-sixth year, was removed. In it were found skeletal formation; cysts, which were covered partly with ciliated epithelium, partly with epidermal cells; nerves; intestinal glands. The ulcerated surfaces showed the structure of a carcinoma. Epithelial masses formed after the operation, and their examination confirmed the belief that they were malignant.

(2) FRANK. A case of mixed tumour in the ischio-rectal fossa on the right side, consisting of connective, adipose, muscular, myxosarcomatous and cartilaginous tissue, and cysts, which had undergone a sarcomatous transformation (myxosarcomatous metastasis in the same place on the left side).

(3) RUDOLPHY. A case of malignantly degenerated cystic tumour in a child. The cystic walls were covered with cylindrical epithelium, and in them were smooth muscle. In the lung metastatic nodules were found at the post-mortem.

(4) GRAMM. One-and-a-half year old girl. Congenital tumour as large as a man's head on the buttocks, reaching into the false pelvis. The tumour had undergone a malignant transformation (disintegration of the tumour and numerous metastases in the inguinal fold). The post-mortem showed the tumour to consist of tissue rich in cysts, and parts of cartilage and bone (teratoma). Multiple metastases in the lungs.

(5) HINTERSTOISSER (1908). Sacral tumour in new-born infant. The histological examination revealed pieces of cartilage and bone, smooth and transversely striped muscles, gland-like formation and soft masses of the structure of a round-cell sarcoma. Two years later there was a local recurrence at the site of the operation. Death during second operation. Histological examination of the recurrent tumour and metastases in lungs and liver showed the structure of a large-celled alveolar sarcoma.

(6) ANT. BERGMANN (IV). Although the microscopical examination of the extirpated congenital coccygeal tumour showed no malignant properties (many-layered cubical epithelium with which the hollow spaces were covered, smooth and transversely striped muscle fibres and bone trabeculæ), yet a recurrence occurred with metastases in the flexures of the groins. Death. Metastases in the lungs. Mixed tumour with malignant transformation into angio-sarcoma. (*Prof. Ghon.*)

In most cases, however, the sacral teratoma shows no malignant formation. In *Heinzmann's* case, cited above (No. 12), there was a recurrence of the teratoma but without any malignant change.

TABLE BY SKUTSCH OF DERMoids IN THE PELVIC CONNECTIVE TISSUE.

| No. | Author and source                                                                                    | Age, birth, clinical condition                                             | Size, site, consistence of tumour                                                                                                                     | Treatment                                                                                                                                                                                                       | Result                                                                   | Remarks                 |
|-----|------------------------------------------------------------------------------------------------------|----------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------|-------------------------|
| 1   | BIRKETT, <i>Guy's Hosp. Rep.</i> , 1859, 3rd ser., v, 252                                            | 25. Female. Prominence in anal groove noticed for two years                | Larger than a walnut. Between rectum and coccyx somewhat to the right. Cyst with atheromatous pulp                                                    | Incision, extirpation                                                                                                                                                                                           | Good healing                                                             |                         |
| 2   | MANNEL, G., "Ueber die Tumoren des Douglas'schen Raumes, Habilitationsschrift," Marburg, 1864, p. 63 |                                                                            | Large as a pigeon's egg. Cystic tumour pressed flat. To the left between peritoneum and levator ani muscle                                            |                                                                                                                                                                                                                 |                                                                          | Post-mortem preparation |
| 3   | MANNEL, G., as No. 2                                                                                 | Female                                                                     | Size of hen's egg. Cystic tumour, site as No. 2                                                                                                       |                                                                                                                                                                                                                 |                                                                          | Post-mortem preparation |
| 4   | DEALMA, (Czerny's clinic), <i>Archiv f. Gynäkol.</i> , 1875, vii, 305                                | 18 Nullipara. Violent abdominal pains. Trouble in urination and defecation | To the left behind rectum reaching upwards to between navel and symphysis. Contents: Dermoid fluid with hairs                                         | Test puncture from rectum, two weeks later puncture and aspiration. Suppuration and enlargement of the cyst. Incision by cutting between anus and sacrum. Laceration in rectum sutured. Vesico-vaginal fistula. | Long-continued suppuration. Urinary fistula closed by operation          |                         |
| 5   | WEINLECHNER, <i>Bericht der k. k. Krankenanstalt, Rudolph Stiftungs in Wien</i> , 1876, p. 296       | 36. Female. Noticed for one year                                           | To the right projecting in the region of the tuber ischi, two dermoid cysts, the smaller a peripheral cyst, the larger twice the size of a man's fist | Excision of smaller cyst. Peripheral part of larger one cut away, the basal part destroyed by saltpetre fumigation                                                                                              | After two months there remained only a remnant the size of half a walnut |                         |

|   |                                                                                                                    |                                                                                 |                                                                                                                     |                                                                                                                                                                                                                                                                                   |                                                                                                                    |                                                                                                                                                                                                                                                                                                                                                                                                                                                                |
|---|--------------------------------------------------------------------------------------------------------------------|---------------------------------------------------------------------------------|---------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------|----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| 6 | SOLOWJEW, <i>Medizinskoje Obozrenje</i> , 1883; <i>Cent. f. Chir.</i> , 1884, p. 96                                | 29. One abortion. Irregular hæmorrhages, nervousness                            | On the left side behind rectum, filling the true pelvis                                                             | Puncture through the vagina. After a few days incision on the left of vagina extending to anus. Only partial removal from fear of injuring the peritoneum                                                                                                                         | Cavity closed after two months. Occurrence of pregnancy                                                            |                                                                                                                                                                                                                                                                                                                                                                                                                                                                |
| 7 | EMMET, <i>Amer. Journ. of Obst.</i> , 1884, p. 852                                                                 | 22. Repeated attacks of pelvic peritonitis. Dysmenorrhœa                        | Underneath peritoneum of Douglas' pouch                                                                             | Laparotomy. Extirpation. Left Fallopian tube also removed. Left ovary not found on account of adhesions                                                                                                                                                                           |                                                                                                                    |                                                                                                                                                                                                                                                                                                                                                                                                                                                                |
| 8 | TRZEBICKY (Mikulicz's Clinic), <i>Wien. med. Woch.</i> , 1885, pp. 394 and 422                                     | 35. Multipara, noticed a tumour in gluteal region for one and a half years      | Larger than a child's head in the left gluteal region, consisting of two non-communicating parts. Hairs in contents | Taken for a burrowing abscess. Extirpation after six weeks. The tumour had numerous diverticula                                                                                                                                                                                   | Healing in ten weeks with suppuration. Closure of cavity by pressure with rubber balloon from the vagina           |                                                                                                                                                                                                                                                                                                                                                                                                                                                                |
| 9 | V. BIERNACKI (Gussow's Clinic), "Eine retrorectale Dermoidcyste als Geburtshinderniss," Dissertation, Berlin, 1887 | 28. First pregnancy. Admitted in labour. No pain previously. Hindrance to birth | Tumour between rectum and sacrum reaching down to 4 cm. above anus. Hairs in contents                               | Puncture per rectum, forceps applied in suprapubic region. Incision of cyst. Incision of abscess on right labium. Fistula formed behind vagina. Futile attempt by detaching posterior wall of vagina from the rectum, to get at the suppurating focus. Later incision from rectum | Discharged on 59th day. After another two months swelling of right nates. Incision, no pus. Good health thereafter | E. mentions two cases of retrorectal cysts causing hindrance to birth. 1. JACKSON, in PUCHELT, "Commentatio de tumoribus in pelvi partum impediētibz," Heidelberg, 1840. 2. MOLLER, R. "Ueber Beckentumoren als Komplikation von Geburten," Dissertation, Berlin, 1869. Third case, p. 26. Sanger does not include these two cases, because it is not shown that they were dermoids. Probably they were such. In Case 2, the incision released yellowish fluid |

TABLE BY SKUTSCH OF DERMoids IN THE PELVIC CONNECTIVE TISSUE—(Continued).

| No. | Author and source                                                                           | Age, births, clinical condition                                                                                                                             | Size, site, consistence of tumour                                                                                                                              | Treatment                                                                                                                                                    | Result                                                    | Remarks                                                                                                                                                                 |
|-----|---------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------|----------------------------------------------------------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| 10  | SINGER, M., <i>Arch. f. Gynäk. u. Obstet.</i> , 1890, xxxvii, p. 100                        | 42. Nine births. The tumour formed a hind-rance to birth even at the sixth birth; craniotomy. At the last birth puncture from rectum then forceps           | The size of a child's head. On the right side, rectum pushed to the left. Situated above levator ani                                                           | Puncture and aspiration from rectum. After nine months, cyst larger, and nearer the perineum. Perineotomy. Extirpation                                       | Uneventful healing                                        |                                                                                                                                                                         |
| 11  | PAGE, <i>Brit. Med. Journ.</i> , February 21, 1891, i, p. 406                               | 47. Seven births. Two abortions, twenty years ago after the fifth birth the tumour was noticed. At the last birth eleven years ago craniotomy was necessary | Reaching to height of navel. Rectum pushed forward. Uterus elevated. Weight with contents 3 lb. Hairs in contents                                              | Exploratory puncture from rectum gave diagnosis of dermoid cyst. Fever, operation with 6 in. transverse incision midway between anus and coccyx. Extirpation | Cure                                                      | P. believes that one or two analogous cases are reported in a London Medical Society                                                                                    |
| 12  | SCHULZE (Operator Krevet), <i>Deutsche med. Woch.</i> , 1895, p. 352                        | 83. Seven births. Last birth very slow. Midwife noticed a tumour at fourth birth                                                                            | Two cysts; one hen's egg size above, one pigeon's egg size below levator ani                                                                                   | Exploratory puncture from anus. After two months, at sixth week of pregnancy, operation with median incision in perineum. Extirpation of both cysts          | Uneventful recovery. Normal course of pregnancy and birth |                                                                                                                                                                         |
| 13  | HOEFER (Fehling's Clinic), "Ueber Dermoidcysten des Beckenbindegewebes," Diss., Halle, 1896 | 37. Three normal births, last one a year ago. Tumour noticed for six years                                                                                  | Size of child's head, left gluteal region protruding. Vagina displaced to the right. The tumour reaches upwards into the separated folds of the broad ligament | Exploratory puncture gave diagnosis of dermoid cyst. Perineotomy. Extirpation                                                                                | Fever in first few days. Cure                             | H. mentions a case of MARCHAND's. (22. Bericht d. Oberhessischen Ges. f. Natur- u. Heilkunde), who in the post-mortem of a child found a tiny dermoid of the lig. latum |
| 14  | COLONNA, <i>Gaz. med. di Torino</i> , 1896, p. 200                                          | 28. One birth normal six years ago                                                                                                                          | Rectum pressed to the right and forward. Spherical tumour reaching down to 4 cm. above anal orifice. Uterus pressed upward.                                    | Parasacral incision. Extirpation                                                                                                                             | Cure                                                      |                                                                                                                                                                         |

|    |                                                               |                                                                                                                                                                                                                                     |                                                                                                                                                                                                                                                                   |                                                                                                                                                                                                                                                                                                           |                                      |
|----|---------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|--------------------------------------|
| 15 | DE QUERVAIN, <i>Arch. f. klin. Chir.</i> , 1898, lvii, p. 129 | 58. Man. Retention of urine, false passage. Patient had used a catheter for nineteen years. Fæces pressed flat.                                                                                                                     | Cyst lay close on sacrum, upper section of rectum pressed forward and to the right. To the front the cyst took up the whole expanse of the true pelvis. Reached down as far as level of third sacral vertebra, upwards as far as one finger's breadth below navel | Incision over Poupart's ligament, puncture of the cyst, drainage. After three and a half weeks extraperitoneal detachment of the upper parts by way of the enlarged skin incision. Tamponade, repulsion of cyst as far as it was detached. After another three weeks extirpation from parasacral incision | Pregnancy continued. Normal delivery |
| 16 | SKUTSCH (1), <i>Zeit. f. Geb.</i> , 1899, xi, p. 353          | 28. Female. Married at 20. Four children. One miscarriage, nine months ago. Is four months pregnant. Pressure symptoms of bladder and rectum, vagina and rectum pressed flat against pubic bone. Bulging of perineum, uterus raised | Presacral cyst filling true pelvis containing one litre of brownish thin fluid                                                                                                                                                                                    | Partial extirpation through an incision 11 cm. long extending from a little way in front of labium majus to a point midway between anus and tuber ischii on left side                                                                                                                                     | Permanent fistula                    |
| 17 | SKUTSCH (2)                                                   | 22. Female. Multipara. One normal confinement fourteen months previously. One miscarriage. Vagina pressed against symph. pubis, rectum against right side of pelvic wall. Ovaries normal in size, and freely movable.               | Immovable cyst filling left half of pelvis and part of right. Extends upwards to pelvic inlet and downwards to bulge perineum                                                                                                                                     | Extirpation with difficulty through a similar incision to that in 16                                                                                                                                                                                                                                      | Cure                                 |



## 122 Galletly : *Presacral Tumours of Congenital Origin*

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### DISCUSSION.

Mr. VICTOR BONNEY said that the gratitude of the Section was due to Mr. Galletly for the trouble he had taken in compiling his paper, which contained a very full list of references. Such papers were of permanent value, because they formed a storehouse of knowledge upon which future workers in the same field could draw.

He agreed that this cyst was of neurenteric origin; evidently there were a number of cell seclusions along the line of the canal which had successively become active.

In a paper he wrote seventeen years ago he stated that all endogenous teratomata should be regarded as derived from secluded cells, the structure of the tumour produced depending on the potentiality of the cell at the time it was secluded. "Secluded" and "seclusion" were the correct words to use, not "included" and "inclusion," for in his view it was impossible for one ovum to become included in the cell mass produced by a second ovum. Nor was it necessary to suppose such an origin for a tumour containing all tissues, for a blastomere secluded in the early stages of segmentation was equally capable of producing such.

Mr. A. C. PALMER said that Mr. Galletly's interesting and instructive paper dealt with a very rare type of tumour. With regard to presacral tumours, Mr. Palmer had had experience of two only. These were removed by Dr. Russell Andrews at the London Hospital. The tumours were similar, each being rather smaller than a lemon, situated in front of the left half of the sacrum behind the peritoneum. In each case the lining and contents were similar; the lining consisting of squamous epithelium, and the contents of material resembling thin dermoid butter. The skin appendages were not present in the wall.

With regard to sacro-coccygeal tumours Mr. Palmer said he had seen one. This was seen in a case of obstructed labour. The tumour was removed piecemeal by Mr. Eardley Holland before delivery of the fetus. It was reported on by the pathologists as a myxosarcoma. The third type of tumour mentioned by Mr. Galletly, namely, those at the anterior end of the cord, seemed to be still more rare. Mr. Palmer had seen one growing from the base of the sphenoid in a macerated fetus in which the hard palate was absent. The growth appeared as a thin-walled bag about the size of a chestnut. It was lined by smooth epithelium and contained blood-stained fluid.

## Section of Obstetrics and Gynæcology.

President—Dr. CUTHBERT LOCKYER.

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### Wounds of the Gravid and Non-Gravid Uterus ; A Study of Uterine Scars.

By Professor J. M. MUNRO KERR, M.D., F.R.F.P.S.

THE contributions to be made to-night are based on material collected in the Gynæcological Department of the Royal Infirmary under my charge. They are concerned more particularly with the healing of wounds in the uterus, a subject of great clinical importance at the present time.

As gynæcological surgery has advanced and its technique improved, we have come to appreciate the possibilities of conservative procedures in dealing with the uterus. We are gradually perfecting myomectomy and widening its application ; we remove the rudimentary horn of a bicornute uterus and leave the normal horn to functionate. To-night I shall describe the resection of a uterus bicornis unicollis followed by two normal pregnancies. We favour, at least the majority of gynæcologists favour, conservative Cæsarean section rather than sterilization of the patient.

All these procedures have for their object the conservation of the uterus so that it may perform its all-important function of retaining the product of conception and expelling the fœtus at term.

Apart altogether from the satisfaction it must always give to the surgeon to conserve rather than remove an important organ, account has to be taken, and we gynæcologists sometimes do not sufficiently appreciate this, of the importance women attach to retention of the full functional activity and possibilities of their reproductive organs.

We are bound, therefore, to give very special consideration to conservative methods in all surgical procedures we employ in operating upon the uterus. In healthy women during active reproductive life we must ever try to conserve the uterus for subsequent pregnancies, and conserve it in such a condition that there is no undue risk to the patient of any very grave consequences should pregnancy occur.

If we are to attain this ideal we must, at least as regards conservative Cæsarean section, greatly improve the present position of this operation. We cannot possibly be satisfied with an operation which gives somewhere between 12 and 15 per cent. of weakened scars, with 2 to 4 per cent. of ruptures in subsequent pregnancies or labours. And these are the figures not of this country alone, but of America, France, Germany, and Italy, where the ordinary incision over the active contractile portion of the uterus is employed.

Impressed as I have been for many years that this unsatisfactory condition of matters can be remedied, I invited my two University assistants, Dr. Hendry

and Dr. McIntyre to undertake with me an investigation of the healing of wounds in the uterus, and the papers we present to you to-night are the outcome of these investigations.

The wounds we propose referring to are those which have resulted from the following operations: (1) Myomectomy; (2) Perforation of the uterus by a sound; (3) Resection of uterus bicornis; (4) Cæsarean section when the incision was placed over the upper contractile portion of the uterus (ordinary conservative Cæsarean section); (5) Cæsarean section where the incision was made over the lower uterine segment.

In this evening's programme I propose to take up the subject from the clinical standpoint. Dr. McIntyre will present the results of his histological studies of various uterine scars, while Dr. Hendry will describe his personal experience of Cæsarean section where the incision was made over the lower uterine segment. Many of the cases to which he will refer were "repeat" operations on that particular area. He will also contrast the scars found in the upper and lower segments.

#### I. MYOMECTOMY.

This subject was brought prominently before your notice by Mr. Victor Bonney last session. The subject concerns gynæcologists very particularly at the present time, and for two reasons: (1) Because many surgeons do not fully appreciate the possibilities of myomectomy; they perform hysterectomy when myomectomy is in the circumstances a more desirable procedure. This applies more especially to general surgeons who essay operations in our surgical field. But even yet some gynæcological surgeons do not appreciate the extent to which this conservative operation may be employed. (2) Certain electrical and radiological experts with little knowledge of gynæcology, sometimes on their own responsibility and on other occasions supported by a physician, advocate and carry out electrical treatment when such a procedure is inadvisable.

This paper is not concerned with such matters. I merely wish to-night to range myself alongside of those who contend that very extensive myomectomies may be performed and give most gratifying results. This is the operation which we as gynæcological surgeons should develop and encourage; it is the reply to the somewhat indiscriminate employment of X-rays which one might almost characterize as one of the vogues of present-day medicine.

As illustrating the extent to which the operation may be employed, permit me briefly to refer to the following case, where I actually bisected the body of the uterus.

The patient was a young woman who was suffering from profuse menorrhagia associated with a large fibromyomatous uterus. I curetted the uterus before her marriage, but with only temporary benefit. She married, and was condemned by others to hysterectomy or X-rays, but she returned to my care under the advice of her family doctor. She was examined under an anæsthetic, when I expressed the opinion, that while I could not promise that the uterus could be conserved, I had considerable hope that it was possible. At the operation I found a large fibroid of the fundus, and the position of the tubes and round ligaments indicated that it was projecting far into the uterine cavity. I therefore removed the tumour with a portion of mucous membrane. Having repaired the edge of the wound, I approximated the two halves of the uterus and stitched them together. She made an excellent recovery, and fifteen months later became pregnant. Her pregnancy was uneventful. Her labour was protracted, as the presentation was occipito-posterior, but she was satisfactorily delivered at term by Dr. Hendry of a full-time living child weighing 8½lb.

Here was a uterus with an extensive wound including the whole thickness of both anterior and posterior walls, and extending into the cavity, and yet the wound healed so satisfactorily that it stood the strain of pregnancy and a protracted labour.

As a matter of fact, authentic cases of rupture of the uterus after myomectomy are very few indeed.

Unfortunately, the only specimen of a uterus removed by hysterectomy ten years after I had performed myomectomy was lost. I cut it up in the operating theatre, but could find no definite scars. It presented all the appearances of a chronic subinvolved uterus (chronic metritis).

*Multiple Fibromyomata removed by Myomectomy.*—A patient, aged 32, consulted me on account of very severe hæmorrhage due to multiple fibroids which could be readily palpated from above the symphysis pubis. She had seen a gynæcologist in a neighbouring city, who had pronounced the situation hopeless from the point of view of palliative treatment, and advised hysterectomy. She was extremely loath to undergo this operation, and she informed me that she had received a proposal of marriage, but she was firmly determined that she would not accept it if hysterectomy was absolutely necessary. From her history and the examination, I agreed with the other gynæcologist that operation was necessary, but I suggested that an examination under an anæsthetic should be made to determine if it were possible to remove the fibroids and leave the uterus. After this examination, which was a very thorough one, I thought that I might be able to do myomectomy, and I told her so. At the operation I removed eight fibroid tumours and left a very ragged-looking uterus. So unpromising did the uterus appear that my assistant urged me to perform hysterectomy, for which I had the patient's full permission if it were necessary. However, I had experience of some very good results from myomectomy, and so I determined to leave the uterus behind. For some days after the operation she was extremely ill, but ultimately she made a satisfactory recovery. Some little time after the operation she got married, and within a few months she became pregnant. She wrote to me from India asking if she should come home for the confinement, but I told her that I thought it was unnecessary if she placed herself in the hands of a specialist there. Her pregnancy ran an uneventful course, and so simple was her confinement that it was all over in a matter of a few hours and before the doctor arrived. Three years ago I was compelled to perform hysterectomy on account of excessive menstrual discharge. There were only a few small fibroids found in the slightly enlarged uterus.

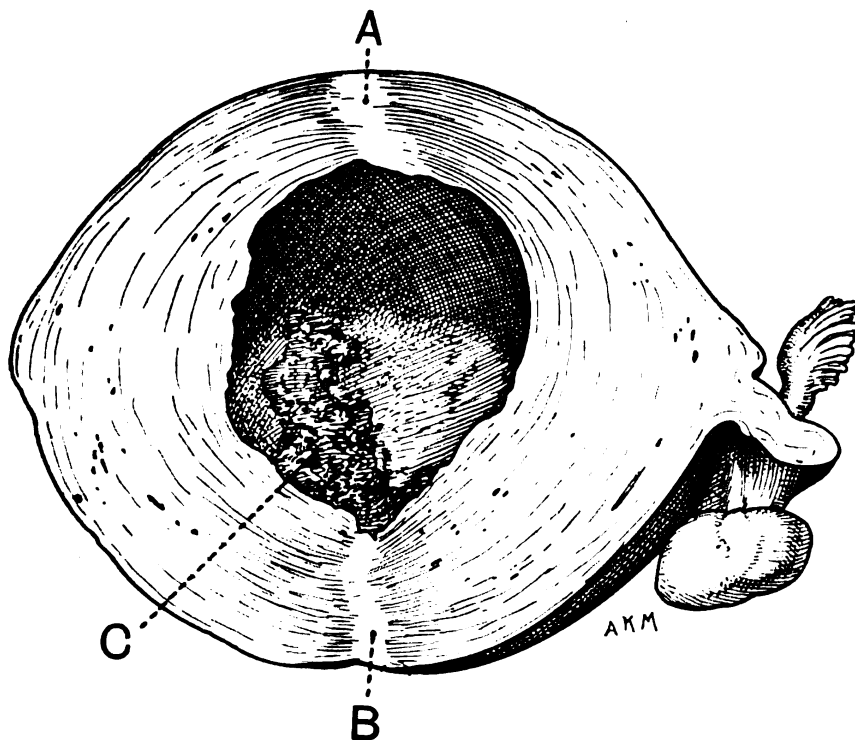
## II. RESECTION OF UTERUS FOR UTERUS BICORNIS.

(a) *Uterus Bicornis Unicollis (Symmetricus)*.—The operation of resection of a uterus bicornis unicollis (symmetricus) has not been performed many times. I know of no recorded case in this country. Further, I cannot find any record of a detailed examination of the scar where this operation had been performed previously. This I can supply, for, as stated below, the uterus was removed at the second parturition. The following is the history of the case.<sup>1</sup>

*Uterus Bicornis Unicollis (Symmetricus)—Resection of Uterus—Two Normal Pregnancies.*—This patient was seen by me with Dr. Millar, of Shettleston, when she was two and a half months pregnant. She was aged at that time twenty-five years, and had been married for nine months. Dr. Millar informed me that he had difficulty in deciding between a myoma complicating a uterine pregnancy, a pregnancy in a double uterus, and an extra-uterine pregnancy. By the time I saw her, however, the diagnosis was simpler, because the cervix was dilated and the ovum could be felt through the dilated cervix. We could therefore exclude the third possibility. I could not, however, decide between the first two. She was sent into the Maternity Hospital, and after

<sup>1</sup> Notes of this case after the first parturition were presented to this Society on October 7, 1920, and kindly read by Dr. Williamson in my absence. See *Proceedings*, 1920-21, xiv (Sect. Obst. and Gyn.), p. 190.

removal of the ovum I carefully examined the condition, and found that we had to deal with a uterus bicornis unicollis in which pregnancy had developed in the right half. She made a satisfactory recovery, but some months later Dr. Millar sent her into my wards in the Royal Infirmary suffering from a second abortion, and again in the right half. When she recovered from the second curettage I explained to her the exact condition, and told her I thought I could improve matters, and that with her permission I would resect the uterus. I thought this was quite possible, because I had examined the uterus very carefully under anæsthesia between the two abortions. She willingly consented, and I excised a portion out of each half, and carefully stitched the two halves together. She became pregnant eight months later. During the later months she resided in my wards in the Royal Infirmary, and when labour began, about full-time, was transferred to the Maternity Hospital. I watched her very carefully during the first stage. I allowed the second stage to go on for two and a half hours, and then, frightened that rupture might occur, I thought it advisable to deliver her with forceps. The pelvis was slightly flattened, and the head lay transversely just above the outlet. I experienced



much greater difficulty in the delivery than I had expected, and unfortunately did considerable injury to the child. The child was born alive, but asphyxiated. It seemed to make satisfactory progress, but ultimately died on the twelfth day of convulsions, the result of the cranial injuries. After the delivery of the placenta, I passed my gloved hand into the uterus to determine the condition of the cicatrix. I found the placenta had been situated on the posterior wall. What was of special interest and satisfaction, however, was the fact that I could feel a longitudinal ridge running vertically along the anterior and posterior uterine wall.

She was delivered of a second child in September, 1920, by Cesarean section. Both returned home in excellent health. I performed hysterectomy because she dreaded another pregnancy.

You here see the specimen and a drawing made by Mr. Maxwell. Dr. McIntyre will show, later, on the screen the macroscopic and microscopic appearances of the anterior and posterior wounds. I wish you to note the

scar on both the anterior and posterior wall. It is quite visible, and under the microscope consists largely of fibrous tissue and interspersed muscle fibres—the fibrous tissue predominating.

I was surprised when I came to examine the scars that the relative proportion of fibrous to muscular tissue was so considerable. We have, therefore, in this case an instance where exact co-adaptation of the wound in a non-gravid uterus was secured, a scar consisting largely of connective tissue—a scar, however, which stood the strain of two labours.

(b) *Uterus Bicornis Unicollis Asymmetricus (Rudimentary Horn)*.—I have had three cases of this nature in which I removed the rudimentary horn, but in only one, as far as I am aware, has pregnancy followed. This case is of particular interest, as the patient had two comparatively easy parturitions. The following is the history of the case:—

This patient, an unmarried woman, aged 22, was brought to me by her mother on account of most extreme dysmenorrhœa. The pain occurred on the first day of the period, and lasted for about twenty-four hours. She informed me that a gynæcologist had removed one ovary three years before, because it was slightly cystic, and that he had found the uterus was of the nature described above, the left half being rudimentary. He now suggested removing the other ovary. After examination under an anæsthetic I confirmed these facts. But I explained that the removal of the ovary was not the proper course, and that I would suggest removal of the rudimentary horn, as in all probability it was responsible for the dysmenorrhœa. This I did some days later. After removing it, I stitched and covered up the wall with cellular tissue and peritoneum. The dysmenorrhœa was practically removed. Some years later she married, and within a few months became pregnant. The pregnancy ran a normal course until the later weeks, when a certain amount of pelvic uneasiness was complained of, and albumin appeared in the urine. Labour came on in the eighth month, and the child presented by the breech. It was born alive. The patient's recovery was delayed by albuminuria, which persisted for several weeks. A few weeks ago she gave birth to a second child. The labour was easy; it occurred a couple of weeks before time.

### III. WOUND IN THE UTERUS FROM PERFORATION WITH A SOUND.

That very complete regeneration by muscle fibre function can take place in a small wound of the uterus is proved by the specimen to which Dr. McIntyre will refer later. But, personally, I am very doubtful whether, after an extensive wound, it is possible to secure a scar in which the tissue is predominantly muscular; the scar will always be very largely fibrous.

### IV. WOUNDS AFTER CÆSAREAN SECTION THROUGH THE ACTIVE CONTRACTILE PORTIONS OF THE UTERUS.

This group of wounds of the uterus has been very fully investigated. In these investigations the following facts have transpired: (1) A number are defective, and some give way during a subsequent pregnancy or labour. According to Eardley Holland's statistics, this occurs in about 4 per cent. of cases. (2) The process of rupture is generally a hernia of the membranes through the scar. The scar in these wounds is defective on the inside, where a gutter along the internal aspect of the wound is to be noted. Into this gutter the membranes project just as the peritoneum does in an abdominal wound which has united unsatisfactorily. (3) Infection of the wound in all wounds is the most important factor in the causation of any unsatisfactory union. (4) The method of stitching and probably the material used, as Eardley Holland has claimed, are important factors.

After examining a number of scars, I ventured some years ago to advance certain reasons as to why the uterine wound after the ordinary Cæsarean section is prone to weakness in its structure. I will now refer to them again.

(1) *The Difficulty in securing complete Asepsis.*—Now this is specially difficult to secure in the uterine wound, because of the danger of a spread upwards from the vagina. If the uterine wound in Cæsarean section becomes directly infected through the abdominal opening, the surgical team is to blame. The surgeon and his team should be able to prevent such an occurrence. But owing to the fact that Cæsarean section has often to be performed with the patient imperfectly prepared vaginally, and upon a structure so easily infected, it is not to be wondered at that even in the hands of the most careful and experienced operators infection of the uterus from below cannot always be prevented.

(2) *The Uterine Muscle-fibres during the Puerperium* are in a state of degeneration. An autolysis occurs in the muscle fibres. It stands to reason, therefore, that the healing process must be interfered with in the early days of the puerperium as a result of this degeneration.

(3) *The Sheets of Muscle which form the Uterine Wall are irregularly distributed*, and this is seen very clearly in Cæsarean section whenever the uterus begins to retract. The surface of the wound, then, instead of being smooth, becomes irregular and puckered, and no matter how carefully the surgeon stitches the wound there remain between the surfaces small pockets filled with blood. These are replaced by fibrous tissue.

(4) *The State of Unrest of the Uterus subsequent to Operation.*—Not only does the uterus "retract," but from time to time it "contracts." If the uterus contracts before the sutures are inserted it will be observed that the edges of the wound gape, and considerable traction has to be put on the sutures if the edges of the wound are to be approximated. If, on the other hand, the edges are approximated when the uterus is passive or retracted, when contractions supervene they strain the sutures. This alternate contraction and relaxation therefore disturbs exact co-aptation and favours the occurrence of small collections of blood between the co-apted surfaces. Here again, then, is a condition which favours the formation of connective tissue. A cut muscular fibre will heal by muscle tissue if the gap between the fibres is minute, but, as Dr. McIntyre will demonstrate, this will not occur if the gap is pronounced.

(5) *The Necessity imposed upon the Surgeon of using his Sutures not only as Co-aptors but as Hæmostatic Agents.*—For the ideal healing of a wound, next to asepsis comes complete hæmostasis. The general surgeon secures this by picking up bleeding vessels and if necessary applying ligatures to them. The obstetric surgeon cannot do this. He has to apply his sutures firmly if he wishes to stop bleeding and prevent the effusion of a certain amount of blood between the cut surfaces of the uterine wound.

(6) *A Placenta situated on the Anterior Wall in the line of Incision is an Unfavourable Factor.*—This occurs in 40 per cent. of cases. In such the operator will find that he has a layer of tissue peculiarly difficult to stitch and co-apt exactly. It is very spongy, very friable, and contains large vessels, and no matter how carefully he applies his sutures they have not the same hold. Again, this favours effusion of blood between the edges, and, further, it favours the formation of a gutter along the internal line of the wound already referred to.

I do not here refer to the influence exerted on a scar where the placenta becomes implanted over it at a subsequent pregnancy, an influence which

Holland claims from his investigation does not favour thinning or rupture of the scar: I refer here to the difficulty of securing good union of the edges of the wound when the placenta is encountered in the line of the scar of a previous operation.

I maintain, therefore, that there are very decided factors which militate against an absolutely sound cicatrix and which cannot be prevented if the incision is made over the upper contractile portion of the uterus.

The more exhaustive examination of scars made by Dr. McIntyre's investigations, the results of which he will describe later, only confirm the views I have expressed, viz., that after the ordinary Cæsarean section there are and will always be a quite appreciable proportion of cases in which the scar is defective.

V. WOUNDS AFTER CONSERVATIVE CÆSAREAN SECTION WITH THE  
INCISION MADE IN THE LOWER UTERINE SEGMENT.

Anyone who has employed this method and seriously studied the formation and anatomy of the lower segment must be impressed by the following advantages this area presents to the surgeon who is anxious to secure a sound uterine cicatrix. Both Eardley Holland and I have already referred to this matter in our writings on the subject.

(1) The wall of the uterus in this area is thin, especially if labour has been in progress for some time: it is often not more than one-sixth of an inch in thickness.

(2) The tissue consists of fibro-muscular tissue as in the upper segment, but here the fibrous tissue is much more abundant. Dr. McIntyre has tried to estimate the relative proportion of muscular and fibrous tissue.

(3) It is less vascular. It is surprising how slight is the bleeding when an incision is made in this area.

(4) As a result the surfaces of the wound can be more accurately approximated, and the formation of these pockets of blood-clot already referred to can be prevented.

(5) This area of the uterus, although it does not remain absolutely inactive after the uterus is emptied, is more passive, and the wound has a chance of healing better, for it is less disturbed than is a wound in the upper segment. One has only to look at the appearance of the lower segment, as shown by frozen sections, to appreciate this point and its importance.

(6) The wound is completely covered with bladder-wall and peritoneum. This keeps the wound extraperitoneal, limits slight infection, and prevents any adhesions of the uterus to surrounding tissues and structures. Dr. Hendry will give his experience of repeat operations in this segment, and will be able to inform you that in not a single case were there any adhesions of uterus to surrounding structures.

(7) Should infection unfortunately occur, it is an area that can be reached from the vagina should that be thought necessary, for the cervix can be pulled down and the bladder reflected from the cervix.

(8) The wound in the lower segment is not put on the stretch during a subsequent pregnancy: it is only after prolonged labour that this occurs. Very different are the conditions in the upper segment when during the whole time of pregnancy one is in doubt as to how the scar will stand the strain of the ever-increasing distension of the uterus and the active contractions of labour.

I leave it to Dr. Hendry to give you his impression of "repeat" operations on this area. Unfortunately, I cannot speak on this point from any extensive



experience, for at present I take no active part in the practice of the Maternity Hospital.

There is one detail in the technique which I think is of considerable importance. In stitching this area one is very apt to pull in a portion of mucous membrane, with the result that there will be a weakening in the scar. The mucous membrane should be stitched separately with very fine catgut or silk, and the edges of the fibro-muscular wall most carefully co-apted.

As regards rupture of the scar no case has occurred in Glasgow. Franz<sup>1</sup>, Wolff<sup>2</sup>, and Vogt<sup>3</sup> have reported cases, but in one certainly the incision extended into the active contractile portion of the uterus, and this to my mind is always a danger where a longitudinal rather than a transverse incision is employed.

In America there have also, undoubtedly, been a few cases in which the scar showed slight weakness, but no ruptures are reported as far as I know. Considering that the operation has been performed thousands of times in America and Germany, as will be noted from the American and German journals, and is ever increasing in favour, by this time there should have been more cases of rupture. Almost without exception, the experience of those who have done repeat operations is entirely satisfactory.

One point more, and then I have finished: it is with reference to the line of incision. In America the incision most favoured is Beck's modification of Krönig's technique, which is a transverse incision of the peritoneum just at the reflection to the bladder, and a longitudinal one through the fibro-muscular wall. In Germany the longitudinal incision is also generally employed. Personally, I prefer, and have always employed, the transverse incision for the uterine wall, and if you observe the formation of the lower uterine segment after delivery you will appreciate that a wound so placed is at greater rest than one directed longitudinally. With the longitudinal incision a tearing into the active contractile portion of the uterus during the extraction of the child is liable to occur, especially if the operation is performed before the lower uterine segment is well developed.

There have also been one or two cases recorded of injury to the bladder.

Unfortunately, there is considerable confusion in the nomenclature of the incision, e.g., I notice that in America "The Low Cervical Incision" is the term very commonly employed. This terminology would be quite permissible if it had been determined that the lower uterine segment was of cervical origin, but this matter is still in dispute. In Germany the following terms are employed: "Suprasymphysäre Transperitoneale," "Suprasymphysäre Kaiserschnitt," "Suprasymphysäre Schnittentbindung." Personally, I think it is much better to describe the incision as "The Lower Uterine Segment Incision," for we all know what that means: it is an incision in the thinned-out portion of the uterus below Bandl's ring.

Although I have been for many years a strong advocate of the lower uterine segment incision, not so much because of its advantage in suspect and septic cases, but because it yields a stronger uterine scar than an incision made in the upper contractile portion of the uterus, I am not so prejudiced as to refuse to admit certain disadvantages which this incision may possess. One of the objections to it is that it is best performed after labour has progressed for some time, and where there is a good lower uterine segment. This means waiting

<sup>1</sup> *Zeit. f. Geb. u. Gyn.*, 1915, lxxvii, p. 212.

<sup>2</sup> *Zeit. f. Geb. u. Gyn.*, 1914, lxxv, p. 740.

<sup>3</sup> "Über die Entwicklung und den Ausbau der suprasymphysären Schnittentbindung," 1921.

till labour has gone on for some time, and is less convenient than fixing a day and hour for the operation before the onset of labour, as a few are doing at present. However, the convenience to the operator is not an argument, unless in addition this early operation is of advantage to the patient. Now, it is only of advantage to the patient in such cases as eclampsia and placenta prævia when immediate operation is necessary. In fact, in certain cases it is of distinct disadvantage, as, for example, the borderline cases where with moulding the head may pass through after a time. Again, by operating before labour small and premature children are not infrequently delivered. I would emphasize this latter point for I know that there is a tendency amongst certain gynæcologists to operate before labour and largely to suit their convenience. It is not in the interests of patients.

There is one distinct disadvantage which I found on one occasion, and that is a head down in the pelvis, the operation being undertaken for contracted outlet. In such a case the lower uterine segment incision is most unsatisfactory, because neither the head nor the trunk can be brought through the opening in the uterus or only brought through with great difficulty. Fortunately, such cases are comparatively rare.

Another objection that has been urged is the tearing of the vessels at the side of the uterus. Should this unfortunately occur, though it has not happened in my experience, I think the vessels can be clamped and ligated. I think it is of distinct advantage to put, as I do, a controlling stitch at the side of the uterus before one makes the transverse incision.

The objection that it is unsuitable for placenta prævia does not hold, for, as you are aware, even in the ordinary classical Cæsarean section the placenta is encountered in 40 per cent. of cases.

The more I see of this operation, and the more reports I get regarding it, the more convinced do I become that it is the ideal operation, and I am very glad to think that in other countries it is being so extensively employed by leading gynæcologists. Our country is notoriously conservative, and, speaking generally, very slow in adopting new methods. Long may it continue so, at least in medicine. But this operation has reached the stage when we must all give it a very extensive trial in this country, for all the reports regarding it are most encouraging.

### Histological Studies of Various Uterine Scars.

By DONALD MCINTYRE, M.B., F.R.C.S.Ed. (introduced by Professor MUNRO KERR).

#### SUMMARY.

##### (A) MATERIAL.

*Case I.*—Recent wound of the non-gravid uterus produced by a dilator.

*Case II.*—Scar of a plastic operation on a uterus bicornis unicollis. Uterus obtained at a subsequent Cæsarean section.

*Cases III and IV.*—Classical Cæsarean scars as seen in the non-gravid uterus.

*Cases V to IX.*—Classical Cæsarean scars at a repeat operation.

*Case X.*—Lower uterine segment Cæsarean scar at a repeat operation.

*Case XI.*—Operation specimen of the lower uterine segment.

*Case XII.*—Post-mortem specimen of uterine wall in the second stage of labour.

## (B) DISCUSSION.

The serous layer; the mucous layer; regeneration of muscle; alteration in arrangement of fibres at the scar; involution; muscle proliferation in the scar during a subsequent pregnancy; influence of placental formation over the scar; suture material; structure of the wall in the upper and lower segments.

This contribution consists of a description of certain material having a bearing on the healing of uterine wounds. This material was collected and examined at Professor Munro Kerr's instigation. Any new facts elicited are of value from the point of view of conservative surgery of the uterus, and especially the operation of conservative Cæsarean section. The points of interest I have endeavoured to reproduce in lantern slides. In many cases, to demonstrate a special feature, a section of the whole thickness of the uterine wall must be shown. This necessitates a very low power magnification, with a resulting flat picture. In some cases I have had to resort to a diagram.

## (A)

## WOUND PRODUCED IN THE BODY OF THE NON-GRAVID UTERUS EXAMINED IN THE NON-GRAVID STATE.

*Case I.*—This specimen, a brief description of which appeared in the *Journal of Pathology and Bacteriology* [1] consists of a wound in the fundus of a non-gravid uterus produced by a dilator about  $\frac{1}{4}$  in. in diameter. There was complete penetration of the wall. Hysterectomy was performed seven days later. The specimen was fixed immediately after removal. Serial sections were prepared and examined. A diagrammatic representation of a section passing slightly obliquely through the inner part of the wound is shown in fig. 1.

There is a slight depression on the serous surface, but the serous coat of the uterine wall is healed. There is no exudate nor adhesion on the surface. The outer third of the muscle wall may also be considered as healed, but in this part of the scar the muscle fibres on either side, as they approach the scar, tend to run in a direction away from the serous coat. A few muscle fibres can be traced across the wound. In the inner two-thirds of the muscle wall the wound shows the presence of lymph exudate and red blood corpuscles enmeshed in fibrin. Surrounding this there is a zone of young connective tissue cells, amongst which a few mononuclear and polymorphonuclear leucocytes are seen. Fibroblasts invade the central zone. Where the young connective tissue cells are spindle shaped, their long axes tend to be directed in the plane of the wound. The dividing line between this and the myometrium is indistinct. Bundles of muscle fibres are seen running a short distance into the granulation tissue. In the nuclei of the muscle-bundles karyokinetic figures are found (figs. 5 and 6).

According to McGill [2] the non-striped muscle cell is not, strictly speaking, an independent cell, but is part of a syncytium. The appearance of the nuclei is of great value in differentiating muscle and fibrous tissue. In the presence of mitosis, however, the contour of the nucleus is lost. Therefore it is difficult to furnish absolute proof that these mitotic figures belong to muscle nuclei.

The following points suggest that the nuclei undergoing division are muscle nuclei. In many cases where a mitotic figure is seen, the surrounding nuclei have the characteristic appearance of muscle nuclei. After staining by Van Gieson's method, karyokinetic figures are seen in cells taking up the muscle stain. Where a mitotic figure appeared in a cell of a muscle bundle, in a series of cases the fact that it was a muscle bundle was confirmed by staining the preceding and succeeding sections by Van Gieson's method.

The endometrium shows no evidence of the wound, nor of inflammatory reaction. It encroaches slightly on the inner end of the track in the muscle.

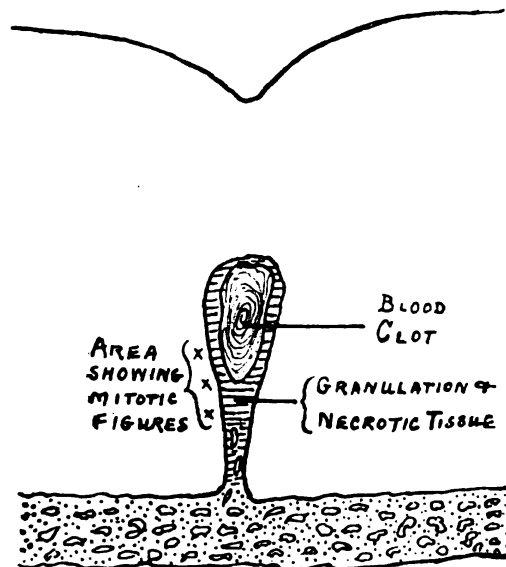


FIG. 1.—*Case I.* Diagram of a section passing somewhat obliquely through the inner half of the wound. It gives the location of the mitotic figures in muscle nuclei as seen in figs. 5 and 6.

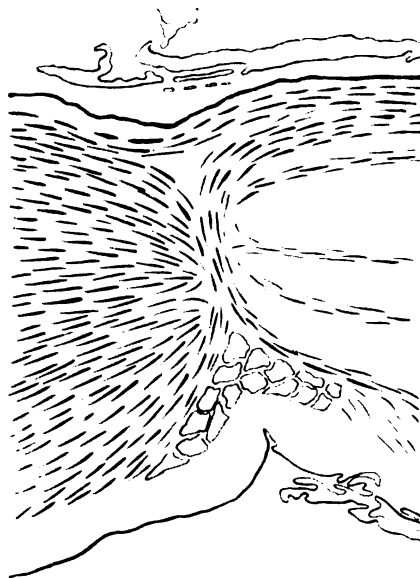


FIG. 2.—*Case II.* Scar in anterior wall. The direction of the fibres in a transverse section of the scar is represented diagrammatically.

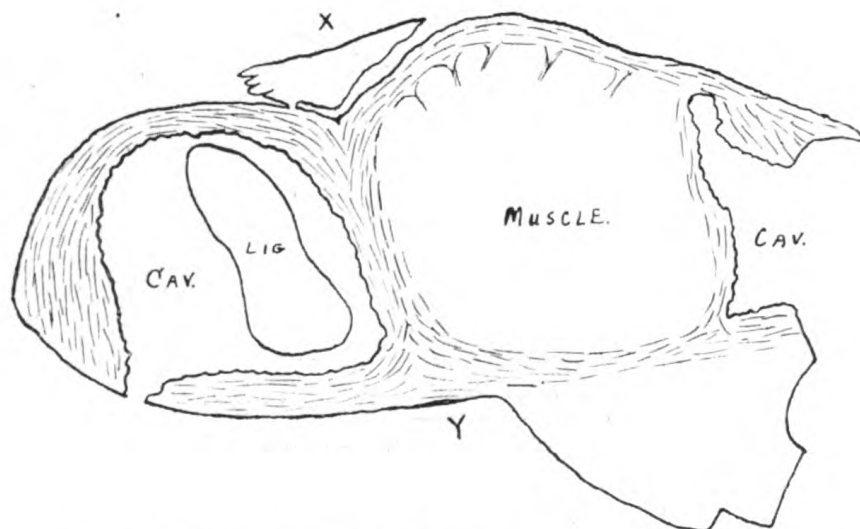


FIG. 3.—*Case VII.* Diagram of scar showing arrangement of ligature cavity.

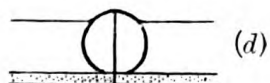
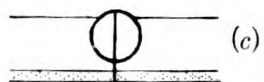
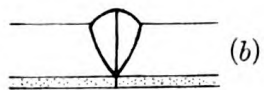
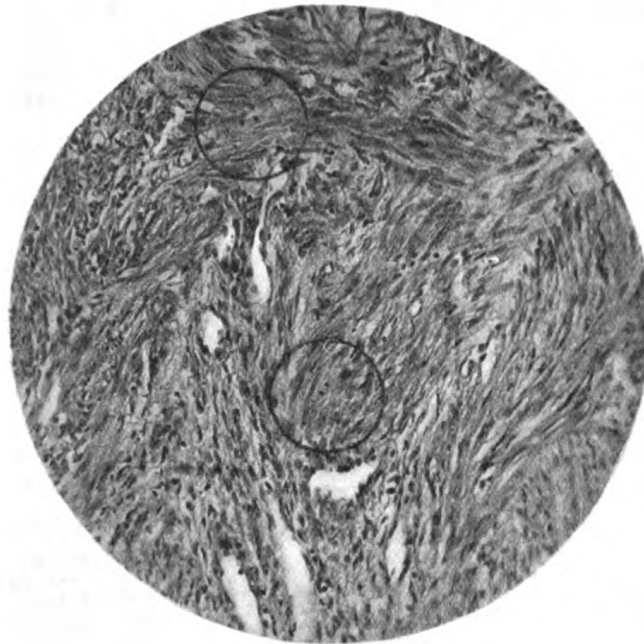
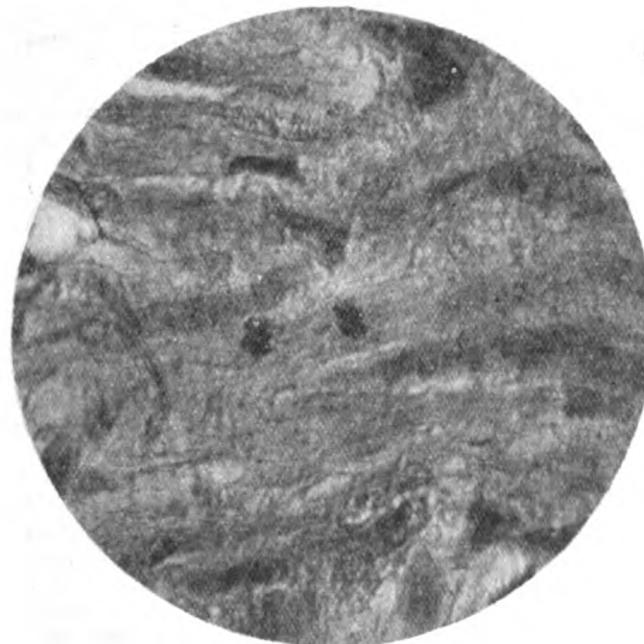


FIG. 4.



1



2

FIG. 5 (1).—*Case I.* Two mitotic figures in muscle nuclei. ( $\times 150$ .)  
(By permission of the "*Journal of Pathology and Bacteriology*.")

FIG. 6 (2).—*Case I.* The upper mitotic figure in fig. 5, showing the outline of a rod-shaped muscle nucleus in close proximity. ( $\times 800$ .)  
(By permission of the "*Journal of Pathology and Bacteriology*.")

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WOUND PRODUCED IN THE BODY OF THE NON-GRAVID UTERUS EXAMINED IN THE GRAVID STATE.

*Case II.*—The uterus was originally a uterus bicornis unicollis symmetricus.

In 1917, after two pregnancies in the right cornu which terminated in abortion, Professor Munro Kerr performed a plastic operation and reconstructed a body similar to that produced by natural fusion.

In 1919 a child was delivered alive with forceps, but with considerable difficulty on account of flat pelvis.

In 1920 a living child was delivered by Cæsarean section, which was followed by hysterectomy.

As seen in the actual specimen, and the drawing by Mr. Maxwell, figured in Professor Munro Kerr's paper (p. 126), the uterus has been divided at right angles to its long axis, about the lower limit of the fundus. The scar is obvious to the naked eye. On the surface of the uterus the remains of loose adhesions are seen. In the cavity the

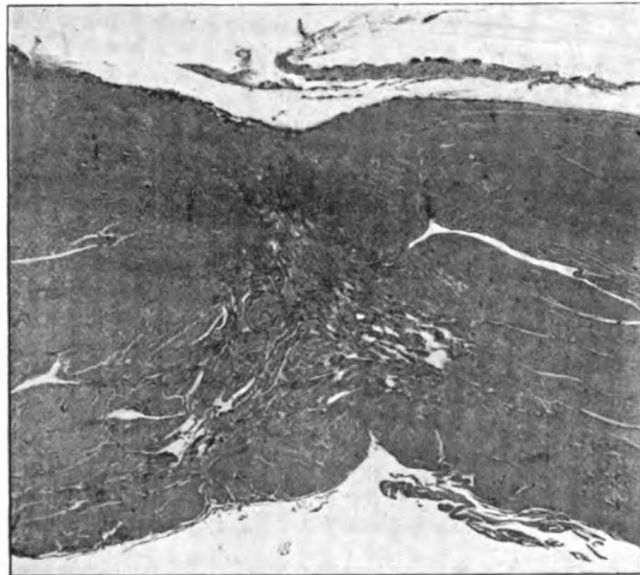


FIG. 7.—*Case II.* Scar in anterior wall. ( $\times 5$ .)

placental site is apparent. In the posterior wall the scar runs into the placental site. On the fundus several small shallow depressions are seen on either side of the scar. These probably correspond to the original sites of sutures. Before mounting, blocks for sectioning were removed from the anterior and posterior walls.

*Scar in the Anterior Wall* (fig. 7).—There is a shallow depression on the outer surface and a V-shaped depression on the inner aspect marking the site of the scar. The wall measures at the scar 7.5 mm., 1 cm. from the scar it measures 11 mm.

The serous layer is thickened and irregular. Small tags of tissue are seen on the surface. A number of multinucleated cells are present in this layer. Immediately beneath the serous coat there is a layer of white fibrous tissue. Beneath this is observed a continuous layer of muscular tissue, and this is the only evidence of continuity of muscular tissue across the scar. In the remainder of the wall, no dense cicatricial tissue is present. There is, however, an arrangement of muscle and fibrous tissue indicating the position of the scar, which is rather striking. The muscle fibres run in the direction of the scar, and the fibres on either side of the scar, as they approach it, tend to turn inwards towards the middle of the wall. An impression of the arrangement of the fibres is given in fig. 2 (p. 133).

Shreds of decidua are present on the inner aspect. Some areas are completely bare.

*Scar in the Posterior Wall.*—There is a shallow gutter-shaped depression on the outer surface, and a deep cleft on the internal aspect. The posterior wall generally is much thicker than the anterior. The wall at the scar, however, measures only 8 mm. thick.

The serous coat is thickened and œdematous. It is definitely more fibrous in the region of the scar. Immediately subserous, there is a defect in the muscle filled up by fibrous tissue, continuous with the fibrosed serous layer, but of looser texture. Slender muscle bundles run across this area. The remainder of the wall shows no definite cicatricial tissue. Blood spaces are numerous, most of them stellate in shape. This layer has lost greatly in thickness by the cleft internally.

A thin covering of decidua is present. It lines the cleft on the internal aspect. Small portions of syncytium are seen. There is no evidence of invasion of the myometrium by fetal elements.

WOUND PRODUCED IN THE BODY OF THE GRAVID UTERUS EXAMINED IN THE NON-GRAVID STATE (TWO CASES).

*Case III.*—Classical Cæsarean section was performed twice, viz., in 1903 and 1909. Between the two operations there were four pregnancies, three premature and one full-time. The last was an instrumental delivery. All four were stillbirths. The patient



FIG. 8.—*Case III.* The presence of cicatricial tissue in the outer part of the scars is apparent. ( $\times 2$ .)

was sterilized at the second operation. At the hysterectomy performed in 1922 there were no adhesions to the anterior surface of the uterus.

On the external surface the sites of the incisions are recognized by two shallow longitudinal depressions, 1 cm. apart, parallel to one another. The one on the right measures 3.5 cm. in length, the other 3.1 cm. In both depressions minute, irregularly disposed vessels are seen by the naked eye. This forms a contrast to the remainder of the anterior surface of the uterine body. No suture material nor depressions suggesting the presence of sutures are apparent. The anterior wall from the inner aspect shows nothing of note. A series of sections at intervals along the scar was examined. One of these is reproduced in fig. 8.

The serous layer can be traced across the sections. In places the cells are of normal appearance. In other situations they are cubical or columnar.

*Scar on right.*—At the middle of the scar the wall, excluding endometrium, measures 8.25 mm., and is less by 1.75 mm. than the wall between the scars. In the outer aspect of the myometrium the scar is readily recognized by the presence of scar tissue and the absence of muscle (fig. 8). This area extends 2.75 mm. inwards from the surface. The tissue present consists almost solely of white fibrous tissue. The majority of the fibres run longitudinally, while a small proportion have an oblique or transverse direction. The latter are situated near the serous coat. The tissue is loosely arranged



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and contains a few elastic-tissue fibres. Many spaces are seen and vessels of the capillary type are numerous. No muscle tissue can be traced across this area (fig. 9). In the remainder of the myometrium the scar cannot be identified, but its inner extremity probably corresponds to a point at which the endometrium encroaches slightly on the myometrium.

*Scar on left.*—At the middle of the scar the wall, excluding endometrium, measures 10 mm. Here again, in the outer part of the muscle layer scar tissue is apparent. It can be identified for 4.5 mm. from the surface. In it, however, bundles of muscle fibres are present, and some of these run obliquely and transversely, bridging across the area. There is no elastic tissue present. Vessels are less numerous. The scar tissue is not sharply defined as in the scar on the right. The scar is lost in the inner part of the myometrium, but again a depression on the mucosal side suggests the inner termination of the scar. A group of endometrial glands, one cystic, is seen in the muscle a

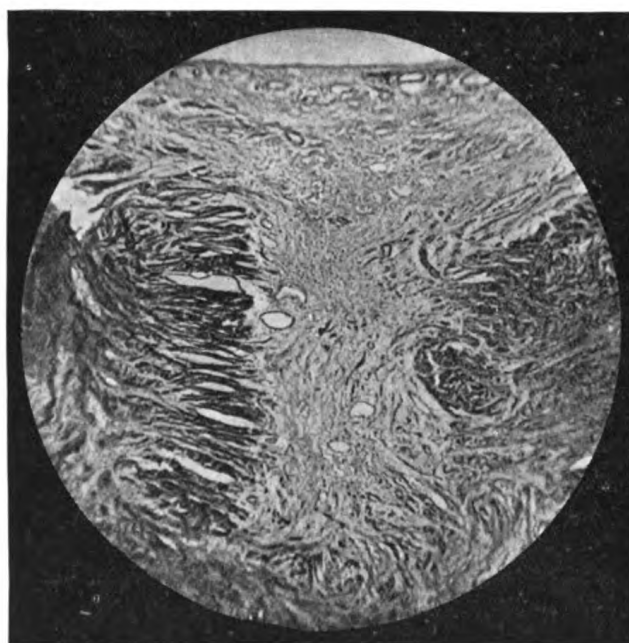


FIG. 9.—*Case III.* Outer part of scar on right. ( $\times 20$ .)

little to one side of the line corresponding to the scar and well removed from the endometrium. This is probably the result of the inclusion of mucosa. The endometrium throughout is of normal appearance.

The scar tissue present in the outer part of the uterine wall is not limited to any one part of the scars, but is present in all of the sections examined.

Several sections from the wall on the flat to include the scar were examined for the presence of suture material, but with negative result. The kind of suture material used is doubtful, but it was probably chromicized catgut. In these sections the encroachment of the endometrium on the myometrium and glands in the myometrium at a distance from the endometrium are both demonstrated.

*Case IV.*—Classical Cæsarean section was performed at full time in 1917, the pregnancy being one in the left cavity of a uterus septus. Early in the pregnancy laparotomy was performed on account of a cystic swelling palpable in the left fornix. The swelling was noted as due to sinistro-flexion of the gravid uterus. No note of any peculiarity of the uterus was made in the record of the Cæsarean section

operation. The suture material used was not recorded. The temperature rose to 100.2° F. on the second and third days, otherwise the recovery was uneventful.

Subtotal hysterectomy was performed in 1923 in Professor Munro Kerr's clinic. There was no intervening pregnancy.

The uterus, on naked-eye examination, shows little departure from the normal appearance. The fundus, however, shows a slight downward dip at its middle, and the organ is not quite symmetrical. The left lateral wall, as it passes to the angle of the uterus, slopes outwards more than the right. It was not appreciated that the uterus contained a double cavity until an attempt to open the cavity was made. An incision through the posterior wall entered the left cavity. A freshly cut surface of the cervix disclosed two channels, the left one situated almost in the centre. Macroscopically there is no clear evidence of a scar present.

The anterior wall shows the presence of numerous small tags of tissue. A block was removed from the organ at right angles to the uterine channels, so as to include them and the portion of the anterior wall which might contain the scar (fig. 10). From



FIG. 10.—Case IV. The left cavity lies on the observer's right. The faint scar is seen passing obliquely to the serous coat opposite the right cavity. ( $\times 3$ .)

the notes of the laparotomy performed, it was presumed that the scar would be connected with the left cavity. In the sections the scar can be traced from the right side of the anterior aspect of the left cavity to the serous coat. It runs a zigzag course to the surface and terminates almost opposite the right cavity.

The serous surface shows evidence of small adhesions and slight depression. In the middle coat the scar is most obvious near the surface. Here it is similar in structure to that of Case III, but the muscle fibres are not cut off so sharply. The white fibrous tissue is loose in arrangement. It contains a few elastic fibres and a few muscle fibres. The latter mostly run in a direction from serous surface to mucous membrane. At intervals muscle tissue can be traced across the scar area. For the remainder of the wall the scar is identified only with difficulty. The endometrium is of normal appearance in both cavities.

In this uterus, changes characteristic of chronic subinvolution are present. A point of interest is that these changes are most definitely present in relation to the left side.

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WOUND PRODUCED IN THE BODY OF THE GRAVID UTERUS EXAMINED AFTER TERMINATION OF A SUBSEQUENT PREGNANCY AT FULL TIME (FIVE CASES).

*Case V.*—Classical Cæsarean section was performed. Two years later the operation was repeated and was followed by hysterectomy.

The scar is apparent on the surface of the anterior wall as a shallow linear depression 6 cm. long by  $\frac{1}{2}$  to 1 cm. broad. On the cavity side it is seen as a sharp depression which becomes less distinct towards the upper limit. The anterior wall, excluding decidua, measures on an average 21 mm. thick. At the scar site it measures 17 mm. Close on either side of the scar area the wall is above average thickness—24 mm.

In sections (fig. 11) the depression on the serous surface is slight. The serous coat is thickened and fibrous in appearance, not only at the scar but also for some distance from the scar.

The muscle of the wall is normal in appearance. No actual cicatricial tissue is present. There is no special arrangement of muscle fibres according to direction, to



FIG. 11.—*Case V.* ( $\times 2\frac{1}{2}$ .)

suggest the site of the incision, except at the inner aspect where the muscle wall is encroached upon by the cleft, which measures on an average 4 mm. deep. This cleft is lined by decidua of normal appearance. The placenta was not situated over any part of the area. At one point in the scar the remains of a silk suture were found. The strands of silk lie in a cavity just under the serous layer, surrounded by a zone of white fibrous tissue. The wall of the cavity shows a mild degree of small round-celled infiltration and the presence of a few polymorphonuclear leucocytes. In it are also seen individual strands of silk and a few giant cells. In the notes of the operation it is stated that chromicized catgut was used.

Sections of the scar area were cut from blocks removed on the flat from the middle of the wall. In these no trace of a scar could be seen. No more suture material was found.

*Case VI.*—Hysterectomy was performed at a repeat Cæsarean section—classical operation.



FIG. 12.—*Case VI.* ( $\times 2$ .)

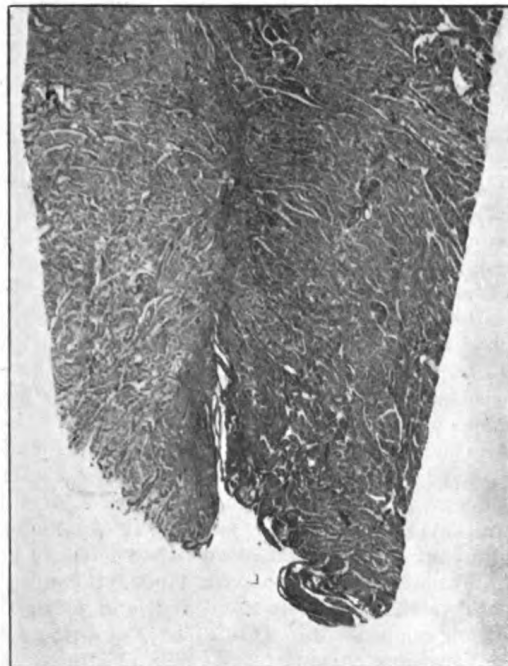


FIG. 13.—*Case VI.*—Scar as seen in inner part of wall. ( $\times 4$ .)

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The site of the old uterine incision is apparent on the surface as a gutter-shaped depression about 1 cm. broad. At the upper extremity the remains of omentum adherent to the scar are seen. On the cavity side there is a deep V-shaped depression. The placental site has included the scar area.

The myometrium measures on an average 25 mm. thick. Close to the scar area it measures 30 mm., whereas at the scar it measures only 16.5 mm. in thickness.

The serous coat is thickened and has on the surface small tags of fibrous tissue. Immediately subserous, a narrow zone of fibrous tissue crosses from side to side. It is not much reduced in thickness as it recedes from the scar.

In the outer half of the myometrium, muscle bundles can be traced across from side to side. Towards the V depression on the inner aspect (fig. 18) there is a slight relative increase of fibrous tissue, and the muscle bundles tend to run parallel to the limbs of the V, meeting in the wall some distance from the inner surface and enclosing muscle bundles having a longitudinal direction as in "Case II, Anterior Scar." In sections stained by Van Gieson's method a faint line, where white fibrous tissue is greater in amount, enables one to follow the scar. It pursues an irregular course through the wall.

The decidua is of normal appearance. No invasion of the scar by fetal elements is seen.

Sections through the scar area cut on the flat from the outer part of the myometrium show no recognizable cicatrix. No suture material is found.

*Case VII.*—Classical Cæsarean section was performed. The uterine incision was closed with interrupted silk sutures. The temperature rose slightly for the first few days after the operation, but was never above 100° F. The patient was discharged from hospital well, after the usual interval. Twenty months later, at a repeat operation, the scar area was excised. There was no intervening pregnancy between the two which were terminated by Cæsarean section.

At the second operation the omentum was found adherent to the old scar. When cleared, a series of small yellowish nodules of irregular shape were seen dotted along the scar. Later, each was found to correspond to the site of a suture.

The serous coat can be identified on the surface. Immediately subjacent to it there is a thin layer of white fibrous tissue, which broadens out as it approaches the scar. There is a definite V-shaped depression of the surface of the scar. In this, in parts of the scar, remnants of adherent tissue belonging to the omentum are seen. In some sections, reflection of the serous layer on to the adherent tissue can be made out.

In none of the sections examined could muscle union be demonstrated. The scar in the myometrium consists solely of cicatricial tissue. This cicatricial tissue is dense and nuclei are relatively few. The fibres tend to run in the plane of the scar, and as they approach the serous surface or the decidua, they pass to one or other side. There is nothing of note in the appearance of the decidua. The placental site lay clear of the scar.

In one case oblique sections of a suture *in situ* were obtained. One is reproduced diagrammatically in fig. 8 (p. 134). The cicatrix is seen at "XY" with remains of a portion of omentum adherent to its surface at "X." The cicatrix consists of dense fibrous tissue in which a few plasma cells are scattered. At either extremity this fibrous tissue splits and runs outwards from the scar; on the left to surround a cavity in which the suture is cut slightly obliquely; on the right, first to surround some muscle tissue, and then to join with the fibrous wall of the suture cavity on that side. On the left there is no muscle tissue between the cicatrix and the suture, as on the right. The suture, therefore, must have cut through the included muscle on the left side.

In the centre of the cavity, on the left, coarse strands of silk are seen (fig. 14). Surrounding the suture, and between its peripheral strands, numerous polymorph leucocytes, mononuclears, and swollen connective tissue cells are present. This, passing outwards, merges into granulation tissue showing leucocytic and plasma cell infiltration. In this area individual silk strands can be identified, and foreign body giant cells are frequently present in their vicinity (fig. 15). Here, and in the inner part of the surrounding dense fibrous tissue, curious large cells are found (fig. 16). These have relatively small nuclei, and protoplasm which stains lightly with eosin. They are probably of connective tissue origin. It is to the presence of these cells that the yellowish colour of the nodules seen by the naked eye is due. The colouring substance

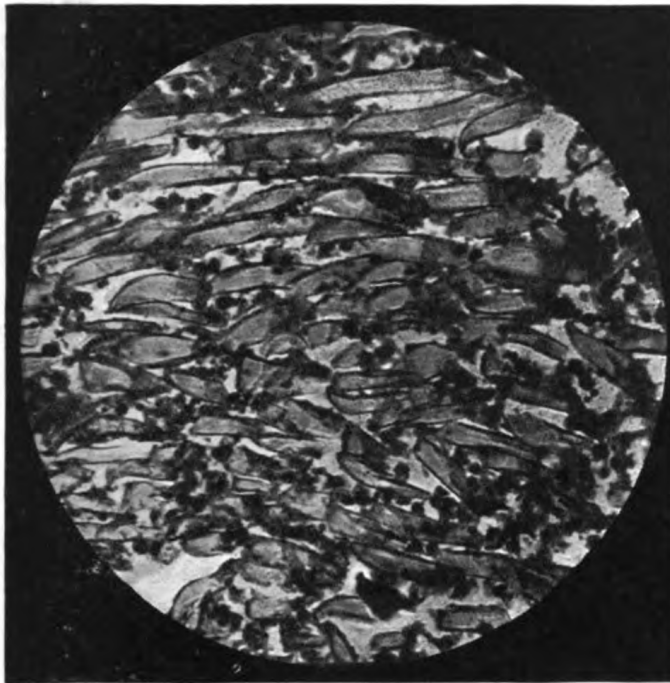


FIG. 14.—*Case VII.* Showing strands of silk in vicinity of scar. ( $\times 160$ .)

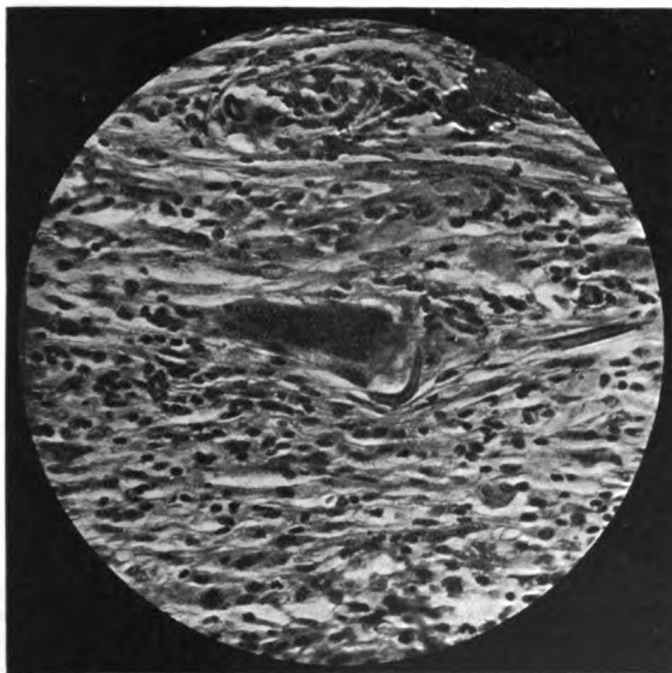


FIG. 15.—*Case VII.* The dark mass is a giant cell. Above it are seen two portions of silk. ( $\times 160$ .)



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disappears in the course of preparation of the sections. The presence of micro-organisms could not be demonstrated. The cavity on the right side, one wall only of which appears in the sections, has the same structure.

*Case VIII.*—The scar of a classical Cæsarean section was examined at a repeat operation after an interval of two years. Interrupted silk sutures were employed to close the uterine wound at the first operation, and recovery was uneventful except for a rise of temperature to 100° F. on the second and third days.

At the second operation the omentum was adherent throughout the length of the scar, but the adhesion was dentate. A small area, corresponding to each suture site, was quite free. All the sutures were easily recognized as transverse white lines about  $\frac{1}{16}$  in. in length, and both to sight and to touch appeared to lie under the serous coat. No knots were seen. There was definite thinning of a portion of the scar and this corresponded to the area of most intimate adhesion of the omentum. No histological examination was made.

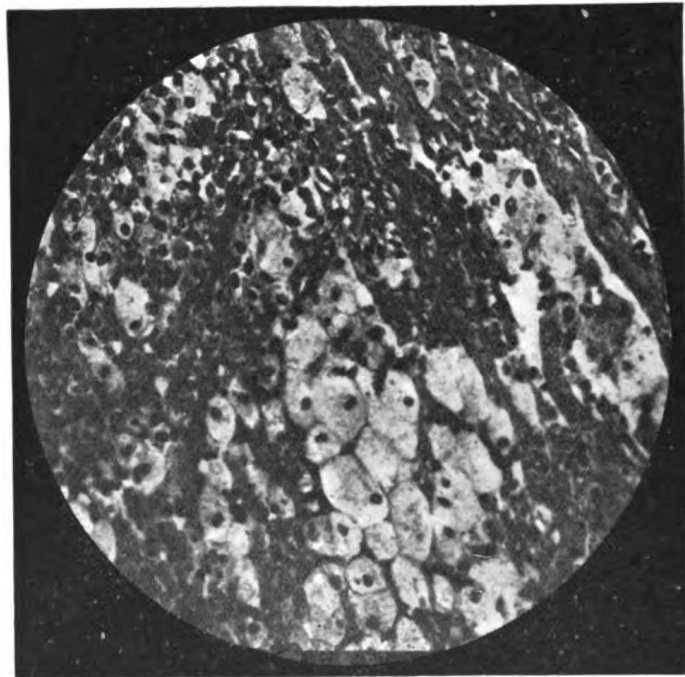


FIG. 16.—*Case VII.* The large clear cells contained a yellow colouring matter. ( $\times 160$ .)

*Case IX.*—Classical Cæsarean section was performed in 1912. The temperature rose to 100·6° F. on the second, third, fifth and sixth days. Convalescence otherwise was normal.

At the termination of the next pregnancy in 1924, a repeat operation of the same type was performed. A portion of the scar was excised. Before opening the uterus this part of the scar appeared as a pearly white, obviously cicatricial area. This area was flush with the remainder of the surface of the uterus. The placenta was not situated over the scar. After fixation of the portion of the wall removed, a block for sectioning was taken from across the middle (fig. 17). It was then seen that there existed a deep pit internally and a similar depression on the external surface. These deep depressions were situated at the cicatricial area.

The uterine wall measures 2 mm. at the scar. On either side of the scar it measures 13 mm. thick. The serous layer of the wall, somewhat thickened, lines the external cleft. Here it passes into small sharp depressions on the surface of the wall.

On one side of the scar the muscle fibres converge quite distinctly towards the cicatrix. As the narrow cicatrix is approached there is great increase of white fibrous tissue between the muscle bundles. The muscle ceases close to the narrow part of the wall.

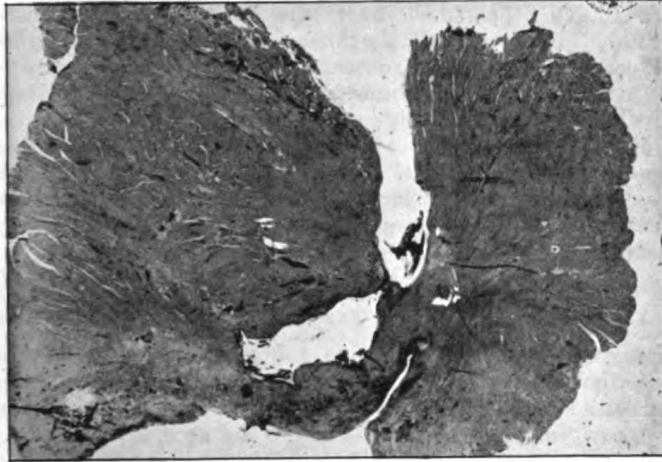


FIG. 17.—*Case IX.* ( $\times 4$ .)



FIG 18.—*Case IX.* Showing the cleft on both surfaces and the encroachment of the ligature cavity on the scar area. ( $\times 25$ .)

On the other side fibrous tissue replaces muscle at a considerable distance from the cicatrix. Only a small proportion of the muscle tissue of the wall approaches near to it. On this side the muscle fibres as they pass towards the scar converge slightly, but separate to surround a suture cavity.



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The narrowest part of the wall shows total absence of muscle. Here the middle layer is composed of white fibrous tissue, moderately well vascularized. There is no elastic tissue present. In the fibrous tissue on either side, nearer the muscle, a few elastic fibres are seen. Between the suture cavity and the external surface, the wall is composed principally of white fibrous tissue, and in this area numerous small interstitial hæmorrhages have occurred. The suture cavity contains strands of silk and a very few leucocytes. The cavity wall, the inner surface of which is slightly crenated, is composed of a layer of necrotic tissue, outside of which there is a narrow zone of mild lymphocytic infiltration. There is no muscle tissue present close to the cavity, unless the internal necrotic zone was originally muscle.

The cavity approaches very close to the cleft on the decidual aspect of the wall (fig. 18). No giant cells are present. No suture strands are seen in the cavity wall as in Case VII, nor are the curious large cells seen in that case apparent here.

The decidua is of normal appearance. It lines the cleft internally, but in this situation it is deficient in places, leaving the surface bare.

WOUND OF THE LOWER UTERINE SEGMENT OF THE UTERUS IN THE GRAVID STATE, EXAMINED AFTER TERMINATION OF A SUBSEQUENT PREGNANCY AT FULL TIME.

*Case X.*—Transperitoneal lower uterine segment Cæsarean section—transverse incision of the uterine wall—was performed. The uterine incision was closed with a double continuous silk suture. The convalescence was uneventful. The suture material was discharged *per vaginam* a few months before the second pregnancy.



FIG. 19.—*Case X.* Lower uterine segment scar. ( $\times 4$ .)

At the repeat operation of the same type after two-and-a-half years' interval, the incision in the lower uterine segment was made immediately above the old scar, and a narrow transverse strip containing the scar was removed. Transverse sections of the scar at intervals were examined. On the cut surface the scar was recognized by the narrowing of the wall.

Microscopically (fig. 19) there is narrowing at the scar level affecting the fibro-muscular wall. On the external surface the depression is filled in by areolar tissue, bringing it flush with the surface. Excluding this loose tissue on the surface, the wall measures in thickness above the scar 8 mm., at the scar 3 mm., and below the scar 5 mm.

The loose fibrous tissue on the outer surface of the scar shows the presence of numerous vessels, some of comparatively large size. Near the fibro-muscular wall

there is a mild degree of small round cell infiltration, and in the same region fairly numerous giant cells of the foreign body type. Close to the wall a few bundles of muscle fibres are seen in this loose tissue. There is no elastic tissue present.

Above the scar, the muscle fibres are arranged irregularly except in the middle of the wall where thin muscle sheets run longitudinally. Towards the outer surface the proportion of fibrous tissue present is greatest.

Below the scar, the muscle fibres are mostly cut obliquely and transversely; only a few muscle bundles are seen running longitudinally, again, about the middle of the wall. At this level the proportion of muscle to fibrous tissue is diminished.

At the scar itself, muscle fibres are cut transversely or obliquely. No muscle bundles can be traced across the scar. Fibres running longitudinally are present, but in no great quantity. The proportion of muscle to white fibrous tissue is diminished. There is no sharp differentiation into cicatricial tissue at the scar.

Above the scar the inner surface is lined by decidua which varies in thickness. The surface is irregular. At the level of the scar, decidua is still present and here contains several large vessels. The surface epithelium is lost. Glands of the cervical type can be identified. Below this level the surface is lined by columnar epithelium. This covering is complete. The cells pass in and line glands of typical cervical appearance. Below the epithelial covering, for several cells deep, the cells have an appearance resembling decidua. A short distance below the scar this picture is lost, the epithelium being set directly on the fibro-muscular wall.

#### THE STRUCTURE OF THE WALL OF THE LOWER UTERINE SEGMENT.

*Case XI.*—Transperitoneal lower uterine segment Cæsarean section—transverse uterine incision—was performed. The uterine incision was closed with a continuous silk suture. The temperature rose to 100° F. on the second day, otherwise recovery was uneventful.

A second operation of the same type was performed after an interval of seventeen months, and a strip of tissue supposed to contain the scar was removed from across the lower uterine segment. The specimen was examined by making transverse sections of the strip of tissue at intervals.

In this case the scar could not be identified.

The wall measures at its upper limit 10 mm. thick; at its lower 8 mm. Externally there is a small amount of areolar tissue. In the fibro-muscular wall externally, muscle fibres run circularly and obliquely, and here they are more sharply marked off into bundles by intervening fibrous tissue than in the remainder of the wall. Passing inwards they assume an irregular arrangement, with occasional thin longitudinal sheets throughout. Just under the decidua there is a definite narrow zone of longitudinal fibres. No large vessels are present but small vessels are numerous. Their lumen is either oval or circular. Muscle is present in greater quantity than fibrous tissue, except at the lower extremity where there is great increase of white fibrous tissue between muscle bundles.

The internal lining consists of a layer of decidua-like cells, varying from three or four deep to a layer of appreciable thickness covered by mucus containing shed cells. Deep to this decidua layer, and separating it from the fibro-muscular wall, there is a narrow fibrillary zone.

*Case XII.*—A strip of tissue embracing the wall of the body of the uterus, lower uterine segment, cervix and vagina, was obtained, after sudden death in the second stage of labour. The specimen was taken from the posterior wall, as the relationship of the anterior surface had been disturbed in carrying out the post-mortem examination. It was carefully pinned out on a board before fixing, to prevent shrinkage. A strip about 2 mm. broad was removed and cut into suitable lengths for embedding. The sections reassembled provided an opportunity of examining the structure of the wall of the uterus at different levels in section corresponding exactly to the long axis of the uterus. The observations from the examination of the sections appear in tabular form. The proportion of muscle to fibrous tissue present is a rough estimate only, and is arrived at from examination of sections stained by Van Gieson's method.

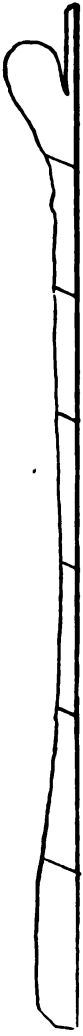
|                                                                | I                                                                                                                                                                                                                                                                                                                                                                                          |   | II                                 |   | III     |   | IV                                 |   | V                  |   | VI                        |   | VII      |   |
|----------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|---|------------------------------------|---|---------|---|------------------------------------|---|--------------------|---|---------------------------|---|----------|---|
| Mucous membrane                                                | Decidua in shreds, no glands                                                                                                                                                                                                                                                                                                                                                               |   | As in I                            |   | As in I |   | Decidua thick in places, no glands |   | Cervical           |   | Cervical, with areas bare |   | Cervical |   |
| Fibrous tissue is situated principally—                        | In outer half of wall (and around vessels)                                                                                                                                                                                                                                                                                                                                                 |   | In outer half of wall near surface |   | Diffuse |   | Inner two-thirds of wall           |   | Inner half of wall |   | Inner half of wall        |   | Diffuse  |   |
| Proportion of muscle to fibrous tissue                         | 20                                                                                                                                                                                                                                                                                                                                                                                         | 1 | 7                                  | 1 | 3       | 1 | 2                                  | 3 | 1                  | 2 | 1                         | 3 | 1        | 6 |
| Average thickness in mm. of blocks (excluding mucous membrane) | 6.                                                                                                                                                                                                                                                                                                                                                                                         |   |                                    |   |         |   |                                    |   |                    |   |                           |   |          |   |
| Length in cm.                                                  | <p> <math>\leftarrow 2.75 \rightarrow \leftarrow 2.4 \rightarrow \leftarrow 2.35 \rightarrow \leftarrow 2.6 \rightarrow \leftarrow 2.1 \rightarrow \leftarrow 2.1 \rightarrow \leftarrow 2. \rightarrow</math><br/> <math>\leftarrow \dots \dots \dots 16.3. \dots \dots \dots \rightarrow</math> </p>  |   |                                    |   |         |   |                                    |   |                    |   |                           |   |          |   |

Table for Case XII.

## (B)

In considering the structure of the uterine scar and its capacity to stand the strain of a pregnancy and labour, we are naturally most concerned with the fibro-muscular coat. It is necessary, however, to refer to the serous and mucous coats, as under certain circumstances the behaviour of these has an influence on the scar which ultimately results.

Healing of the serous layer occurs early and is generally satisfactory. In many cases it presents a smooth surface, whilst in others it passes in to line a depression. If accurate approximation is not secured and adhesions form, this layer is reflected on to the adherent tissue. If for any reason the serous coat is interposed between the muscle edges, for instance by inversion of one side of the wound, muscle proliferation will probably not effect junction with the opposite side, and a deficiency in the outer part of the scar will result.

As regards the mucous coat, it is invariably found to pass into a deep depression in the wall. This is most apparent in the cases examined immediately after delivery. No scar tissue is found in this layer. Small portions of endometrial tissue are commonly met with in the muscle wall in the vicinity of a scar at some distance from the inner surface. They result from displacement and inclusion of small fragments of mucosa between the edges of the muscle. Such displaced tissue can evidently retain its vitality. Examples of this are seen in Cases I and III, and in one of Dr. Hendry's cases, where the endometrium is present in the subcutaneous tissue of the anterior abdominal wall. If, by imperfect suturing, an edge of the wound is allowed to become everted and approximation of the mucous membrane to the inner margin of the muscle of the opposite side results, healing of this part of the muscle layer is interfered with and a deficiency of the inner part of the scar is produced.

When we come to consider the important layer—the muscle coat—of the uterine wall, the first question which arises is: Does healing occur by fibrous tissue only, or has the muscle the power to proliferate and bridge the wound with muscle tissue?

The statements regarding regeneration of smooth muscle are conflicting. The opinion generally expressed in surgical and pathological textbooks is that in the human subject no proper regeneration of unstriated muscle occurs and that healing by fibrous tissue is the rule. Whereas, examination of Cæsarean section scars brings forth the statement that muscle regeneration is perfect. Whitridge Williams [3], for instance, finds in eight out of ten cases difficulty in identifying the scar and total absence of scar tissue. In these cases muscle fibres extended across the site of the old incision as if it had never existed. In one case the wall at the incision site measured 3 mm., contrasting distinctly with the remainder of the wall which measured 25 mm. thick. The area, however, showed no scar tissue present. Gamble [4], who published a more detailed description of the same and additional material, states that "under ideal conditions the muscle unites perfectly, and its fibres cross the site of the incision as if it had never been made. When, however, there has been no muscle union, the scar is made up solely of decidua and peritoneum. The muscle bundles are not much distorted, the formation of fibrous tissue is much less than would be anticipated, and when it is increased the elastic fibres are correspondingly diminished in number." Couvelaire [5] on the other hand states that even where the macroscopic thickness of the wall is normal at the cicatrix, a film of scar tissue is interposed, and, in such, one meets muscle fibres, but these are slender and

singularly different from the hypertrophied fibres of the puerperal uterus. In five specimens examined by him (three to nine days) he found no evidence of mitosis in the muscle fibres. The source of the material is not stated, but one is unlikely to see mitotic figures except in specimens fixed when quite fresh. Ritschl [6] found mitosis in the uterine muscle in rabbits. Walsh and Loeb [7] in experiments on different animals, after cutting across one horn of the uterus and ligaturing the other, found mitosis in smooth muscle tissue between the fifth and eleventh days. This was more marked at the incision than at the site of the ligature.

I have been able to find records of only one case in which mitosis in the human uterine muscle is described, viz., by Berry [8] in 1920. In his case a curette had penetrated the uterine wall four days before the specimen was removed.

Case I furnishes an example of a recent wound of the uterine wall. The changes present and the processes involved follow the laws governing the healing of wounds in general. There is evidence of proliferation of muscle tissue and of invasion of the periphery of the granulation tissue by muscle cells. The process originates at an early stage. This case proves that regeneration of unstriated muscle may occur, but furnishes no indication of the extent to which it does occur. It is impossible to say whether or not one could ultimately have identified scar tissue in this specimen. It is reasonable to assume that if the intervening granulation tissue is of small amount, the proliferation of muscle will bridge over the gap. If, on the other hand, there is considerable amount of granulation tissue, even in the absence of infection, it is doubtful if muscle proliferation will be sufficiently active to replace the granulation tissue which will then be replaced by scar tissue with a resulting weak scar.

If we now consider the state of affairs when the wall of the body of the uterus is incised, we find that muscle fibres are cut in all directions, as in the wall there is no regular arrangement of the direction of the muscle fibres. In the healing process, to attain a perfect result, fibres cut transversely should be approximated to the like on the opposite side. Fibres cut obliquely should be replaced end to end. Those running longitudinally, however, are not so intimately concerned in bridging over the gap. Perfect apposition is never obtained; a proportion of fibres cut transversely will be approximated to fibres running longitudinally, and so on. This, in the presence of perfect muscle regeneration, will produce a slight defect in the scar. This defect, I think, can be recognized where cicatricial tissue is absent, or present in minimal amount. It is this which accounts for the deficiency in thickness of the wall at the site of the scar in Case V. As a result of the alteration in architecture of the wall at this point, abnormal retraction has occurred, producing or exaggerating the deficiency in thickness. Close on either side of the scar in this case, and in Case VI, the wall is thicker than at some distance from the scar. This might be the result of the presence of the muscle which has fallen away from the edge of the incision, or may be due partly to the misdirected retraction of this area.

In Case II, where a proportion of scar tissue is seen, it is apparent in the sections from the anterior wall that the muscle and fibrous tissue fibres which run in a direction to cross the scar area, tend to converge to the middle of the wall, and in the scar itself the majority run in the plane of the scar. This may be produced to some extent by the approximating sutures compressing the edges of the wound, or by the contraction of the scar

tissue lessening the distance between serous and cavity surfaces. It is, however, to some extent due to the newly formed muscle and fibrous tissue fibres being arranged in the line of the scar instead of across the scar as was noted in Case I.

Case III furnishes conclusive proof, if any be required, that cicatricial tissue may be found in the classical Cæsarean scar. In the scar on the right, this cicatricial tissue lies in the outer part of the myometrium. Its fibres are not arranged to stand a lateral strain. In all probability in this uterus, if another pregnancy had occurred, at full time the muscle tissue on either side of the cicatricial tissue would retract and drag out the cicatricial tissue into a narrow band under the serous coat. This type of scar, if examined immediately after the emptying of the uterus, would show a fibrous thickening under the serous layer. The wall of the scar, however, would be under average thickness and close to the scar thicker than elsewhere. Cicatricial tissue would be inconspicuous and muscle would cross from side to side. Such a description fits Case VI.

A fibrous thickening under the serous coat is noted in a scar described by Audebert and Rascol [9]. This behaviour of the scar accounts for the common description "no cicatricial tissue but deficiency in thickness." Where deficiency in the outer part of the myometrium exists in a scar, at a subsequent Cæsarean section the intra-uterine pressure will be found to bring the scar area flush with the surface. An extreme example of this was seen in Case IX. For this reason I am of the opinion that deficiency in the outer part of the wall frequently goes unrecognized, or is attributed to deficiency internally. During pregnancy and during labour, in a uterus containing such a type of scar, when the arrangement of the direction of the muscle and fibrous tissue fibres is borne in mind, it will be apparent that the intra-uterine pressure may force the membranes opposite the scar to separate the muscle fibres running longitudinally, to stretch the scar tissue present, and possibly to rupture some of the muscle fibres crossing the scar. This may occur without complete rupture. Thus, a scar, after being subjected to the strain of a pregnancy and possibly labour, if originally defective becomes more so. I therefore consider that at the repeat classical Cæsarean operation the uterus should be opened close to the scar and the portion of the wall on one side of the wound bearing the scar should be excised.

In Case III the myometrium between the two scars differs in no respect from the myometrium lateral to the scars. It has been suggested that two parallel incisions, by cutting off the trophic nerve supply, will weaken the wall between the scars. From examination of the uterus in the non-gravid state there is no indication that such is the case. It is possible, however, that the effect on this part of the wall might be seen during the hypertrophy of pregnancy.

From my observations it will be apparent that, while the possibility of muscle regeneration was recognized, the more general process involves a considerable amount of healing by fibrous tissue. Thus, my observations correspond more to the conclusions at which Couvelaire arrived than to those of Whitridge Williams and Gamble.

In Case IV only a faint line of cicatricial tissue can be traced in the scar. This line pursues a zigzag course, obliquely from the left cavity to the surface opposite the right cavity of the uterus. As the pregnancy in the left side of this uterus developed, it would draw on the wall of the right for a supply of muscle. As involution occurred, the muscle tissue would slide back to its

original position, carrying with it the scar. I am of opinion that this happens to a less extent under normal circumstances after the ordinary classical Cæsarean incision. It will occur while the granulation tissue is still soft and pliable and the sutures have become loose. In this respect the process of involution may be an advantage during repair of the wound.

Compare Case II, in which there was no involution, with Case V or Case VI.

The material I have examined does not enable me to express any opinion regarding the influence of involution on the actual proliferation of muscle as a factor in healing of the wound. Audebert [10], in describing a case of perfect regeneration of muscle in a Cæsarean scar in the body of the uterus in which the puerperium had been febrile, suggests as an explanation the possibility of muscle proliferation in the neighbourhood of the scar during a subsequent pregnancy. It is usually stated that there is hyperplasia in addition to hypertrophy of the smooth muscle during pregnancy. Taking nuclear division as evidence of proliferation of the muscle, I have been unable to confirm this in material I have examined. This material included a uterus containing an early pregnancy, in which fixation was sufficiently good to show mitosis in the tissues of the ovum.

The most recent work in this connexion has been performed by Kalenscher [11], who, after experimenting with portions of uterine muscle *in vitro*, comes to the conclusion that it appears to be almost certain that the mature uterus can form new cells and that not all the growth of the uterus during pregnancy and the later return to normal condition is due to increase and diminution in size of the muscle fibres. No proof, however, is furnished. Kalenscher has set himself the task of settling this problem. I therefore leave this at present as an open question.

The alternate contraction and relaxation of the uterus during the healing of the wound is a factor which is bound to have an adverse influence on the ultimate result. This influence, however, if we exclude its tendency to unknotting of the sutures, cannot be very marked, judging from the limited material I have examined. In Case II, after an incision of the body of the non-gravid uterus, the resulting scar is no better than that which might follow classical Cæsarean section.

In two cases, viz., Cases II and VI, the placenta was situated over the scar. In Case II it is apparent that the scar is thinner posteriorly, relative to the adjacent wall, than anteriorly, and nevertheless the scar in the posterior wall is actually thicker than the scar where it lies in the anterior wall clear of the placenta site. Again, contrary to the usual finding, the posterior wall of the organ where the placenta was situated is greater in thickness than the anterior. In this case, however, one is dealing with a uterus which was originally malformed. In Case VI there is no evidence to show that the placental formation has had any weakening effect upon the scar. In neither case could invasion of the area by cells of foetal origin be made out.

In four cases chromicized catgut is the suture material alleged to have been used. In three of these there is no structural arrangement of the tissues at the scar to indicate the points of suture, except in Case II where there are several shallow depressions on the surface to either side of the scar at the fundus. In only one of these three cases, however, was an exhaustive search made from this point of view. In the fourth case (Case V) a silk ligature was found.

In five cases unabsorbable suture material was used; three in which the scar lies in the body of the uterus and two in the lower uterine segment. In

the former, sutures are readily recognized. In Case VII, after twenty months, it is seen that there has been mild infection of the suture tracks, and each suture is surrounded by what is virtually an abscess cavity now sterile and walled off by fibrous tissue. Cutting through of the muscle by a suture is demonstrated. This is a scar in which there is no muscle union and which nevertheless has stood the strain of a pregnancy. It probably would not have stood the additional strain of a labour. In this case the slight rise of temperature during convalescence was not sufficient to indicate the probability of a weak scar. In Case VIII, after two years, the sutures are distinctly seen, and although in this case histological examination was not carried out, the appearances indicate that no infection of the suture material had occurred. In Case IX, after twelve years' interval, a suture is present and is found at the weakest point in the scar. It does not appear that there has been infection of the suture bed as in Case VII, and yet at the site of the suture the scar is much weaker than any part of the scar in Case VII. In the section of the scar in Case IX the presence of the cavity in which the suture lies in close proximity to the scar, adds to the structural weakness of the cicatrix. In the lower uterine segment cases (Cases X and XI) in the first the sutures were passed *per vaginam*. The second need not be considered, as the scar is not recognizable in the portion of tissue examined.

It would appear, therefore, that unabsorbable suture material may remain in the uterine wall for an appreciable time even where a mild infection has occurred. On the other hand, from clinical experience one knows how frequently a sinus develops, to discharge an unabsorbable suture after a straightforward convalescence and perfect healing of the abdominal wound. This must occur through an adhesion to the site of the suture. Sutures may also make their way to the exterior through the bladder, as exemplified in Case XI of Dr. Hendry's series, (p. 159), or from the cavity of the uterus. These must cut through the included muscle before becoming free, if the knot is still intact, as it frequently is. In this respect, then, to use unabsorbable material is a disadvantage.

In cases I have seen in which, following classical Cæsarean section, a silk suture has discharged or has been removed from a sinus, in the majority the knot is still intact, and I have been struck by the small size of the loop which has included the edges of the wound. A suture introduced into homogeneous soft tissues when tied tends to dispose itself in a circle. As usually introduced into the wall of the uterus the suture includes roughly a triangular area of tissue (fig. 4a, p. 134) two angles where the suture penetrates the serous layer, and the third at the junction of the myometrium and decidua (fig. 4b, p. 134). It is the result of the tendency of the suture to become circular which raises a ridge on the surface at the incision after the sutures are tied. This is done at the expense of the angles which are approximated to the centre of the triangle. The suture will cut through the tissue of the angles, with the result that it buries itself under the serous coat gradually from the point of penetration to the incision, and this finally occurs under favourable circumstances without the formation of adhesions. Meanwhile at the third angle, cutting through of the suture allows the formation of a pit into which the mucous membrane may encroach (fig. 4c, p. 134). At a post-mortem, where death occurred on the fourth day after a classical Cæsarean section, the interrupted sutures were represented from the internal aspect by deep clefts across the wound. This could account for inclusion of small portions of endometrium, such as were seen in the specimen from Case III. By now, with reduction in thickness of the wall



in the course of involution, and by the slackening of the suture, there will be a tendency to separation of the edges of the wound. If by this time the intervening granulation tissue is not sufficiently strong to maintain apposition, separation will occur at the outer extremity of the wound on account of the tendency to eversion. This tendency to eversion may produce gaping of the edges of the wound between interrupted sutures, where these are not sufficiently close together. I am satisfied that this is the cause of the dentate adhesion of omentum in Case VIII. It is advisable, therefore, to introduce sutures in such a manner that, before tying, the tissue included will form a circle approximately; that is to say as much muscle wall should be included close to the decidua as at a corresponding point in relation to the serous surface (fig. 4*d*, p. 134). The decidua or endometrium should not be included. An additional suture, to counteract the tendency to eversion, may be introduced as a continuous insertion to include serous coat and a small amount of muscle. This will also serve to ensure good approximation of the serous layer.

With regard to the suture material which should be employed, the result of Eardley Holland's investigations suggests that catgut is a dangerous suture material for classical Cæsarean section [12]. There is always the possibility of loosening of tension, or of complete unknotting and the danger of too early absorption. On the other hand, non-absorbable material such as silk, is, as I have indicated, the cause of many minor complications of the operation.

In thinking over this problem, it occurred to me that it might be possible to produce a suture with a surface which will ensure against the slipping of the knots. I consulted Messrs. G. F. Merson, Ltd., of Edinburgh, and discussed with them the possibility of producing a suture composed of a combination of catgut and silk. I was assured that this was quite a practical proposition. They have prepared for me suture material in the following manner:—

In the first instance the catgut is spun. The strand produced, before completely drying, is spun with two or more very fine strands of silk. This ensures that the silk lies on the surface although partially embedded in the catgut. These strands are spun in opposite directions,<sup>1</sup> and for practical purposes are the same length as the original ligature, as in spinning only a few turns per foot are given. There is thus provided a suture the bulk of which will absorb and leave two very fine and permanent strands of silk. This is, of course, a compromise, but the material will eliminate the danger of slipping of the knot, and of too rapid absorption. The amount of unabsorbable material left in the uterine wall will be at a minimum, and therefore less liable to lead to complications. The silk of the suture, if used uncombined with catgut, would be so fine as to cut through the muscle. The silk is sterilized before spinning and subsequently in the ordinary process for catgut, so that the sterilization of the material is equal to that of catgut. A use for this form of ligature might be found in other surgical procedures, for instance in suturing the fascia in ventral or umbilical hernia.

With regard to the scar in the lower uterine segment (Case X) the most notable feature in comparison with the scars in the upper segment is the absence of the sharp cleft on the inner aspect. The deficiency in thickness is due to a wide depression externally.

I had hoped to be able to make a comparison of scars of the upper and lower uterine segments, but as I have been able to obtain only one example of the latter I am not prepared to make any definite pronouncement.

Further experiments have shown that there is no advantage in spinning the silk in opposite directions.

In Case XII I was able to examine the wall of the lower uterine segment as it appears in the second stage of labour. Cases X and XI provide sections of the wall immediately after labour.

The following impressions were formed from examination of these specimens: The wall of the lower uterine segment, very thin towards the end of labour, becomes considerably thicker immediately the uterus is emptied. Passing from the upper segment to the lower, there is a definite increase in the amount of fibrous tissue present in the wall. This is the result of reduction in the size of the muscle bundles, with an increase of the intervening fibrous tissue. In the lower part of the upper segment the fibrous tissue is greater in amount near the outer surface of the wall, whereas in the lower segment it is in greater quantity towards the inner surface.

From the structure of the lower segment one advantage suggests itself, and that is, should fibrous healing result, the scar is more allied in structure to the neighbouring tissue than in the upper segment.

#### CONCLUSIONS.

(1) The healing of the uterine wound follows the laws governing the healing of wounds in general, but is influenced by certain factors peculiar to this special organ.

(2) Muscle regeneration occurs in wounds of the non-gravid uterus and probably also in wounds of the gravid uterus.

(3) The regenerating muscle may bridge across the wound, but where this fails its place is taken by cicatricial tissue.

(4) Cicatricial tissue is found in the classical Cæsarean scar; the amount varies.

(5) After classical Cæsarean section, in examining the scar at a subsequent pregnancy, cicatricial tissue at the serous surface is found drawn out into a thin layer and may readily escape recognition.

(6) In the one lower uterine segment scar examined, the scar tissue does not form so marked a contrast with the wall in its neighbourhood as is seen in the upper segment.

(7) In the body of the uterus an alteration in arrangement of the muscle fibres can generally be recognized. This arrangement is such as to weaken the wall.

(8) Two parallel incisions in the body of the uterus do not appear to exercise any adverse effect on the area between. Excision of the scar in repeated classical Cæsarean section is, however, considered advisable on account of the possibility of progressive weakening of the scar.

(9) The question of the possibility of muscle regeneration in a scar in the body of the uterus during a subsequent pregnancy remains unsettled.

(10) From histological examination there is no evidence of weakening of the scar as a result of placental formation over the scar.

(11) The use of thick non-absorbable suture material is the cause of many complications and may partially be responsible for the production of a weak scar.

I wish to express my indebtedness to Professor Munro Kerr, who supplied me with most of the material I have described, either from his Department in the Royal Infirmary, Glasgow, or from his private cases. To Dr. Hendry I am indebted for several specimens. My own material was obtained from patients under Dr. Martin's care at the Glasgow Royal Maternity Hospital. For the

specimen of uterine wall removed post mortem I have to thank Professor J. H. Teacher.

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**Observations made at "Repeat" Cæsarean Sections on the Uterine Scar (a) where the Previous Incision had been made through the Upper Contractile Part of the Uterus, and (b) where the Previous Incision had been made through the Lower Non-Contractile Part or Lower Uterine Segment.**

By JAMES HENDRY, M.B., F.R.F.P. & S. G. (introduced by Professor MUNRO KERR).

I SHOULD like the communication which I wish to make to the Section to-night to be regarded as merely a clinical supplement to the papers which have just been read.

Three years ago the operation of Cæsarean section was most thoroughly investigated in this country. Mr. Eardley Holland's contribution to that investigation has already taken its place among the classics of our subject [1].

The percentage of fatal cases, not only in selected, but in all cases, the percentage of morbid cases, the prospects for the child, and the frequency of rupture of the uterus during subsequent pregnancy, have been studied, and to-night Dr. McIntyre has presented a very comprehensive investigation into the histological changes which occur in the uterine wall in the region of the operation wound.

My observations in this paper are limited to the appearances of the previous uterine wound, and what is equally important to the surgeon, to the changes in the neighbouring tissues, especially the tendency to the formation of adhesions, in cases on which I have performed a second Cæsarean section. I am using the term "Cæsarean section" in the broad sense in which it is still used by British obstetricians. I notice a tendency among Continental surgeons to limit the term "Cæsarean section" to the old classical operation, and to use for those operations in which the lower uterine segment has been the point of entry, some such expression as "supra-pubic," or "supra-symphyseal" section.

In all the cases described Cæsarean section has been performed because of dystocia due to contracted pelvis. With the exception of one case, in which the operation was performed at Barshaw Hospital, Paisley, all the cases have been in the wards of Dr. Shannon and Dr. Martin in Glasgow Royal Maternity Hospital. The repeat operations have been performed between May, 1920, and May, 1924. The cases described fall into two groups: (a) Those in

which the previous uterine incision had been made through the upper contractile part of the uterus, and (b) those in which the incision had been made through the lower non-contractile part, or lower uterine segment.

(a) CASES IN WHICH THE PREVIOUS INCISION HAD BEEN MADE IN  
THE UPPER CONTRACTILE PART OF THE UTERUS.

There are nine cases in this series, and in one there had been two previous sections. In only one had I done the first operation, and therefore know the exact technique employed, but in the others the notes in the hospital journals, or the known methods of the surgeon concerned, have indicated the suture materials used.

*Case I.*—W., with a generally contracted pelvis (c.v. 3 in. minus), was operated on on June 8, 1920, at the end of her first pregnancy. A vertical incision was made in the upper segment; the placenta was posterior; four interrupted No. 3 silk sutures were passed through the wall of the uterus, avoiding the endometrium. Then the middle muscle coat was sutured with a continuous No. 3 catgut suture. The peritoneum was carefully closed with a No. 1 catgut, and the silk ligatures were then tied. The convalescence was non-febrile and uneventful. The patient went into labour for the second time on June 1, 1923. The abdominal scar was firm. When the abdomen was opened there were no post-operative adhesions. The surface of the uterus was perfectly smooth and there was no localized thinning of the wall evident on the surface, nor, as was proved later, from inside the uterus. The seat of the previous incision was marked by the presence of eight yellowish spots, arranged in two parallel rows of four. These yellowish spots I took to represent the site of the original sutures, but Dr. McIntyre has described in his paper the type of tissue which goes to make up those spots. The uterus was again incised vertically, in the middle line, and the placenta was again posterior. This appears to have been a good result.

*Case II.*—K. At the end of her first pregnancy she had a classical Cæsarean section performed in May, 1921. The closure of the uterine wound was secured by two layers of continuous catgut. The puerperium had been non-febrile. She came into hospital in labour on March 27, 1923, with the head freely movable above the brim. When I opened the abdomen I found a few adhesions of omentum to the uterus. When these had been freed I had no difficulty in identifying the previous wound. There was a uniform thin area palpable on the surface of the uterus. The wall was incised through the old scar. The placenta was situated on the posterior surface. There was a definite thinning of the uterine wall along the course of the old wound, but there was no bulging of the scar.

*Case III.*—V. Her first pregnancy ended in a craniotomy. At the end of her second pregnancy she was operated upon on March 30, 1919. The surgeon used interrupted silk sutures through the whole thickness of the uterine wall except endometrium, continuous stout catgut through the middle muscle coat, and fine catgut to close in the peritoneal coat. The puerperium had been disturbed by a slight pyrexia for the first few days. She came again into hospital, in labour, on December 18, 1923. When I opened the abdomen I found the omentum adherent to the surface of the uterus. The line of the uterine scar could just be identified, the silk suture points being quite evident. There was no bulging of the scar. The placenta was situated on the anterior wall. Apart from the dense adhesions of omentum this result appeared to be good.

*Case IV.*—C. F. Her first child was delivered by a classical Cæsarean section in March, 1921. The method of closing the uterine wound consisted in the application of multiple interrupted silk sutures through the whole thickness of the wall, with some fine catgut to close the gaps in the peritoneal coat. There had been some febrile disturbance for the first few days of the puerperium. On November 12, 1922, she came into hospital in labour, with the head freely movable above the brim. When I opened the abdomen I found the omentum densely adherent to the abdominal wall, and also firmly plastered over the surface of the uterus. When the front of the uterus was exposed, the previous incision could be easily seen. There was a series of thin areas along the

course of the scar, into which I could dip my fingers. I picked out two of the old silk ligatures. I incised through the old scar and found the placenta posterior. There was here an intermittently thin scar with dense omental adhesions. It was from this case that the scar described in Dr. McIntyre's case, No. 7 (p. 142), was obtained.

*Case V.*—M. The first section was performed in October, 1921. The wall had been stitched with interrupted silk sutures, continuous stout catgut through the muscle coat, and then some superficial fine catgut sutures. The puerperium was non-febrile. She came into hospital in labour on October 3, 1923. When I opened the abdomen I found the omentum densely adherent to the uterine surface, with the transverse colon firmly anchored. When I separated off these dense adhesions I found the old scar showing about six separate shallow depressions, with, on each side, a series of the yellow spots to which I have already referred. I opened the uterus to the right of the scar; the placenta was anterior. From the middle of the uterus I could feel a deep furrow in its anterior wall.

There was here an irregularly thin scar with yellow spots, and adhesions of omentum and transverse colon.

*Case VI.*—M. Her first section was performed twelve years ago. The suture material was interrupted silk, with some catgut to close the edges of the peritoneum. She was admitted to the ante-natal wards in April, 1924, and went into labour on April 25, 1924. When I opened the abdomen I found the uterus adherent to the anterior abdominal wall by several dense but separate adherent bands. A loop of small bowel was adherent to the wall, fixed in a very narrow U-bend. The old incision was easily identified, running very obliquely across the surface of the uterus. There were one or two very thin areas in the wall. The placenta was situated on the upper anterior surface. It is from this case that the tissue described by Dr. McIntyre in his case, No. 9 (p. 144), was obtained.

*Case VII.*—G. Her first section was performed in January, 1920. The surgeon had employed three silkworm gut sutures, then interrupted catgut sutures, and then continuous catgut for the closure of the uterine wound. The puerperium had been disturbed by a slight temperature for the first few days. She was admitted to hospital in labour on September 18, 1921. The abdominal scar appeared to be very much fixed to the front of the uterus. The previous abdominal incision went through the left side of the middle line, so I incised through the right side of the middle line. The uterus was densely adherent to the anterior abdominal wall, over an area about the size of the palm of the hand. I separated off the uterus, but found difficulty in identifying the actual line of the previous incision. Eventually I incised through the middle of the anterior uterine wall, and found the placenta on the anterior surface. There was considerable thinning of the wall in the region of the scar.

*Case VIII.*—N. Her first section was performed on June 19, 1915, and that after forceps had been unsuccessfully applied before her admission to hospital. The uterine wound had been closed by through-and-through silk ligatures. The puerperium had remained febrile for twelve days. She was admitted to the ante-natal wards on December 11, 1923, and went into labour on December 30, 1923. When I opened the abdomen I found the uterus densely adherent to the abdominal wall over a broad area. Above the level of this adhesion the omentum was firmly plastered over the fundus, and a loop of bowel was fixed to the side of the uterus. The placenta was situated on the anterior surface. On the inner surface of the uterus the line of the former incision could be identified as a deep groove.

*Case IX.*—W. Her first section was performed by one of my colleagues on May 24, 1916. I have not been able to trace the details of this operation, but she was submitted to a second section by the same surgeon in February, 1921. His first abdominal incision had been made to the right of the middle line; so, with a view to avoiding this area, he made his second abdominal incision to the left of the middle line. He found the uterus so densely adherent to the abdominal wall that he decided to discard this route. He then made a transverse incision through the abdominal wall well above the umbilicus, incised the fundus transversely and scooped out fœtus, placenta and membranes through this opening. The method of closing the uterine wound is not recorded.

The patient was admitted to the ante-natal wards on April 15, 1924, and went into labour on April 23, 1924. I decided to open the abdomen through the first incision, i.e., to the right of the middle line. I found the uterus adherent to the abdominal wall over a broad irregular area. On exploring above I found that the omentum and transverse colon were adherent to the fundus. I freed the anterior adhesions and incised the uterus along the middle line. Fortunately, the placenta was on the posterior surface. In spite of the dense adhesions there was no obvious thinning of the uterine wall. The scar in the fundus could just be traced.

#### REVIEW OF THE UPPER SEGMENT CASES.

In almost all these nine cases the previous uterine wound could be easily traced. In most the scar showed thinning, sometimes very marked. Special interest attaches to those cases in which the scar showed a series of depressions, in view of the observations on their histological structure by Dr. McIntyre. Again adhesions, often of a very dense type, were usually present, and made access to the uterus difficult. I admit, of course, that the presence of the dense anterior adhesions in Cases VI, VII, VIII and IX might have been used to make the second operation extraperitoneal, but it was evident in the second operation on Case IX that the surgeon would not risk possible damage to bowel and other adjacent organs by taking that route. I notice in *Surgery, Gynæcology and Obstetrics* May, 1924, that Foster actually stitches the parietal peritoneum to the surface of the uterus round a wide area at the first operation to allow a subsequent section to be extraperitoneal [2].

I would emphasize the fact that these nine cases are not selected cases from the practice of any one surgeon, or of the members of one surgical team. Only two of the nine have been operated upon on the first occasion by the same surgeon, and some of them are from the work of surgeons who have obtained most admirable results in their maternal mortality statistics. It can hardly be disputed, however, that the intra-abdominal results in these cases leave room for improvement in the conservative surgery of the pregnant uterus.

Before leaving the cases in which the classical operation has been performed, I will describe two unusual sequelæ which occurred in my own cases.

*Case X.*—H. I performed a classical Cæsarean section on August 13, 1921. The uterine wound was closed with interrupted silk sutures and a No. 1 catgut continuous suture along the edge of the peritoneum. The abdominal wall was closed in layers with catgut and a few silkworm gut tension sutures. Convalescence was uneventful; the abdominal wound healed by first intention; the uterus involuted well, and the patient left hospital on the twenty-third day after her operation.

She reported to me again on March 4, 1924, stating that for about six months she had felt a small "knot" in her wound, which swelled up and became very painful during her menstrual periods. I found a small nodule about the size of a green pea at the lower end of the abdominal scar, which appeared to lie in the subcutaneous fat and to be adherent to the rectus sheath. The scar appeared normal in every other respect. The uterus lay in its normal position in the pelvis. I excised the nodule under a local anæsthetic; it was quite circumscribed and superficial to the rectus sheath. Dr. McIntyre has prepared a section of the tissue; it contains endometrial tissue, encapsulated in white fibrous tissue.

*Case XI.*—M. I performed a classical section on June 20, 1922, closing the uterine wound in the same way as in Case X, the silk ligatures being No. 3 or No. 4. Convalescence was uneventful; involution was undisturbed. In January, 1924, this patient was admitted to the surgical side of Glasgow Royal Infirmary, complaining of urinary disturbance. A small calculus had been passed *per urethram*, and in it the focal material could be recognized as a silk suture. Other calculi were still in the bladder. My friend, the late Mr. A. J. Couper, opened the bladder and found hanging from the

bladder wall three large calculi, quite as large as acorns, which had formed round the ends of the silk sutures which had penetrated from the anterior surface of the uterus. The highest calculus corresponded in position to the fundus of the uterus.

(b) CASES IN WHICH THE PREVIOUS INCISION HAD BEEN MADE THROUGH THE NON-CONTRACTILE OR LOWER UTERINE SEGMENT.

The merits of the lower uterine segment as a means of approach to the uterine contents at the end of pregnancy have been much discussed in recent years. The lower segment operation has been growing steadily in favour, and I notice that, in reviewing a series of over 190 sections performed in the University Frauenklinik of Würzburg since 1908, Krause states that the classical operation (occurring in only eleven of the series) has not been performed since 1910 [3]. A similar tendency in favour of the lower uterine segment operation has been evident recently in American literature.

Since May, 1920, I have operated on forty-one cases through the lower uterine segment, and in five of these cases, in which another pregnancy occurred, I have had the opportunity of performing the second section. In addition I have operated on two cases in which a first lower uterine segment operation had been performed by Professor Munro Kerr. The technique employed was in principle that described by Professor Munro Kerr in his paper on "The Lower Uterine Segment Incision in Conservative Cæsarean Section," in 1921 [4]. Some unimportant modifications have been developed from experience.

To describe the technique briefly: The abdomen is opened through a low vertical incision, and the operation is transperitoneal, not extraperitoneal. After packing off the rest of the peritoneal cavity with gauze, and retracting the lower end of the wound with a Doyen's retractor, the operator incises transversely the loose peritoneum just above the bladder. It is not necessary to disturb the bladder very much, particularly if the patient has been allowed to remain for some time in labour, thus allowing the lower segment to become completely developed. The uterine wall is then incised transversely. While there is a risk of this incision being extended into the large vessels at the sides of the uterus, such an accident is very unlikely to occur if the patient is allowed to go well into labour. I find it convenient to pick up the head of the foetus with a pair of short forceps—not to force delivery, but to pick up the head just as one picks up tissues in the abdomen with tissue forceps. This manœuvre obviates putting one's hand inside the uterus. If the head is not conveniently situated, a little pressure on the fundus by an assistant usually causes the occiput to rotate into the wound (except where the head is impacted in the brim—that is a different problem). When the child has been removed, the cord is tied and cut, and the placental end dropped into the uterus. The uterine wall is then closed. I formerly employed a continuous fine silk suture (No. 1 or 2) through the mucous and inner muscle coat, and then a continuous No. 2 catgut suture to coapt the remainder of the muscle coat. More recently I have employed two layers of continuous catgut for the actual uterine wall. The visceral peritoneum is then carefully closed. By this time the uterus has probably regained its tone and the placenta has commenced to separate. The fundus may be brought into the wound and the placenta expressed through the open cervix. I have only in one case performed this operation before the cervix was sufficiently open to allow the placenta to escape, and there I removed it manually immediately after the child.

In seven of the forty-one cases I have found that the inner silk suture has been discharged *per vaginam* some time after the operation. I have not had

trouble with these sutures in any other direction. In two of these cases a second pregnancy has occurred, and in them I did not find any gross impairment of the uterine wound at the second operation (Cases XIV and XVII). In view of the recovery of these silk sutures I have more recently adopted a No. 1 continuous chromic gut suture for the mucous membrane and inner muscle coat, and a No. 2 or No. 3 continuous catgut for the outer part of the muscle coat.

This technique in the transperitoneal operation differs from that employed in several other schools, in that the incision in the uterus is made transversely. It is thus more easy to confine the operation to the lower uterine segment. Again, there is less risk of tearing of the bladder, which Brandt describes in 4 per cent. of his cases [5].

#### CASE RECORDS.

*Case XII.*—L. The first section was performed by Professor Munro Kerr in September, 1918. The uterine incision had been sutured with an inner layer of catgut, then a layer of silk for the muscle coat, and fine continuous catgut for the peritoneum. The puerperium was non-febrile. The patient came into hospital again in labour on May 22, 1920. The abdominal cavity was found free from adhesions. When I opened the visceral peritoneum I could just identify the first incision as a fine white line which was not overstretched even though the patient was advanced in labour. When the lower uterine segment was incised above this scar I could not make out any thinning from the inside.

*Case XIII.*—W. Her first operation was performed by Professor Munro Kerr in February, 1920. The uterine wall had been closed with interrupted and continuous catgut sutures with fine catgut for the peritoneum. There was a febrile disturbance for the first few days—the rest of the puerperium was non-febrile. The patient went into labour again on January 22, 1922. The abdomen was found free from adhesions. When I exposed the lower uterine segment through an incision in the peritoneum no trace of the old scar could be seen, and, when the uterus had been opened, examination with the fingers inside and outside revealed no depression.

*Case XIV.*—M. This patient came into hospital in labour on July 1, 1920. After delivering the child, I closed the incision with a deep and a superficial continuous silk suture, and then the peritoneum with fine catgut. Her convalescence was non-febrile, but about two months after her operation her own medical attendant removed a long silk suture from the vagina. She went into labour again on February 15, 1923. When I opened the abdomen, I found the whole cavity free from adhesions, except for a few light strands of omentum which were attached to the fundus. I think that these adhesions were due to the use of too hot a towel at the previous operation. This was one of my early cases, and at that time I delivered the uterus through the abdominal incision before opening the lower uterine segment. When the peritoneum was stripped, the whole scar could be recognized as a white wavy transverse line across the lower uterine segment. There seemed to be slight thinning of the right extremity of the scar. The tissue from this scar is described by Dr. McIntyre as Case X p. 146.

*Case XV.*—H. Her first section was performed on October 16, 1920. She was a large woman with masses of adipose tissue. The uterus was closed with two layers of fine continuous silk, and the peritoneum with fine catgut. I saw her next in January, 1924, when she was eight months pregnant; she had developed a ventral hernia. Although she was very fat the case history showed that she had been discharged after her previous operation with what appeared to be a sound abdominal scar. She went into labour again on March 18, 1924. The uterus was found to be quite free from adhesions. Careful examination of the uterus, both before and after its incision, showed no trace of the scar, and no thinning of the wall.

*Case XVI.*—G. Her first section was performed in September, 1920. The uterine wound had been closed with two layers of continuous silk and the peritoneum with fine



catgut. The puerperium was uneventful. She went into labour again on April 10, 1923. I found the abdomen free from adhesions. Careful examination of the uterus, both before and after its incision, showed no trace of the old scar and no thinning of the wall.

*Case XVII.*—S. Her first section was performed on December 22, 1921. The uterine wound was closed with a layer of continuous silk and then with a layer of continuous No. 2 catgut, and the peritoneum with a continuous fine catgut suture. The early part of the puerperium was non-febrile, but on the ninth day a stitch abscess was discovered at the lowest corner of the abdominal wound. On February 1, 1922, I removed a long silk thread from the vagina. She went into labour on May 20, 1923. There were no adhesions in the abdominal cavity. When the lower uterine segment was exposed and opened, no trace of the old scar could be found.

*Case XVIII.*—G. Her first section was performed on December 15, 1922. The uterine wound was closed with a No. 1 continuous chromic gut and an outer No. 2 continuous catgut. There was a troublesome post-operative bronchitis, but from the fourth day the remainder of the puerperium was non-febrile. She went into labour again on April 5, 1924. The uterus was quite free from adhesions, and no trace of the old scar could be found in the lower segment either by inspection or palpation.

#### REVIEW OF THE LOWER SEGMENT CASES.

In these seven cases the scar of the previous wound could be definitely identified in only two cases, in one as a fine white line, and in the other as a wavy line which at one extremity showed definite thinning. The other notable feature is that the abdominal cavity at the second operation was found practically always free from adhesions.

There are certain aspects of the lower uterine segment operation which I do not wish to discuss here. Its advocates state that the wound edges are easily sutured, that it is a relatively safe operation for infected cases, and that in a subsequent pregnancy there is no strain on the scar until the woman has gone into labour; its detractors say that it is a difficult operation and that it must always be an operation of emergency—one cannot arrange to perform such an operation at 9.30 a.m. This much, however, I can claim for the operation, that it gives a good scar and also that it gives the surgeon an opportunity of performing the second operation without struggling through a maze of adhesions.

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#### DISCUSSION.

Dr. CUTHBERT LOCKYER (President) said that the introductory remarks of Professor Munro Kerr in favour of conservatism and the non-sterilization of the patient during Cæsarean section accurately expressed the guiding principles of London obstetricians—several of whom had performed four and even six repeat-sections rather than resort to sterilization. The object of the contribution which Professor Munro Kerr and his co-workers had brought before the Section was an attempt to reduce the percentage of weakened scars and of rupture of the uterus, which latter was said to result in 2 to 4 per cent. of cases after Cæsarean section. For this purpose the wounds after section through the active contractile portions of the uterus, had been minutely examined and the causes of defective union pointed out, not the least important of these being (a) the "unrest" of this part of the uterus subsequent to operation, (b) the frequency with which the placenta was attached to the anterior wall caused the tissues to be very spongy and oozing. To obtain adequate coaptation and hæmostasis of such tissue was

difficult; blood was liable to effuse between the edges, and the internal depression and inversion of the mucosa were favoured.

Such considerations had led the authors to the conclusion that after the ordinary classical Cæsarean section there would always be a number of defective scars. Section through the lower segment was therefore advocated and the reasons stated *seriatim*—preference was given to a transverse incision which secured the greatest rest during uterine involution.

A stronger scar was here obtained; it made for safety in suspect and septic cases. It was pointed out that the time to perform the operation was during labour when the lower segment was well defined, when there was less danger of injuring the lateral vessels.

He (the President) expressed unstinted admiration for the excellent histological work of Dr. McIntyre, his lucid demonstration of which was all that could be desired. He had rendered a somewhat prosaic subject both interesting and instructive. Dr. Hendry had adopted the plan of separating adhesions between the anterior uterine wall and the parietes when such were found to be present in repeat operations. In similar cases he (the President) had preferred to deliver through a wound which had become extraperitoneal.

Dr. EDEN said he thought the work that had been brought before the Section that evening was of great value. The two important questions with regard to Cæsarean section which remained unsolved were the best position for the incision, and the best method of suture, including of course the suture material employed. It was only by such careful research as Professor Munro Kerr and his colleagues were engaged in that these questions could finally be settled; we wanted to know, so to speak, the life history of the Cæsarean cicatrix. Mr. Eardley Holland had shown the important part which uterine infection played in producing a weak scar. It was, however, very disquieting to find, from Dr. McIntyre's work, that even in aseptic cases two definite causes of weakness might be found, viz., irregular union of the endometrial surface of the incision, and inclusion of endometrial tissue in the wound. The inclusions would survive, and retain their functional activity, and participate in the menstrual changes. It seemed almost hopeless to expect a perfectly reliable cicatrix under these conditions, and we might even come to formulate the dictum "Once a Cæsarean section, always a Cæsarean section," for even if the scar withstood the strain of pregnancy it could not be relied upon during labour. He hoped the same methods of inquiry would be pursued with the lower segment cicatrix, for only in this way could clear proof be obtained that it was in fact more durable than that of the classical operation.

Dr. RUSSELL ANDREWS said that he had thought for some time that one of the chief causes of unsatisfactory Cæsarean section scars, in the absence of sepsis, was inclusion of endometrium between the muscle edges, and the truly alarming sections shown by Dr. McIntyre confirmed this view. If the uterus retracted well it was fairly easy to bring the edges of the wound together without inclusion of endometrium, but if the uterus remained flabby and the edges of the wound were obscured by bleeding it was not easy to make certain that none of the stitches would include the deeper layers of the endometrium. In such cases it was best to insert a series of sutures as deeply as possible, taking in only the deepest layers of muscle, in the hope that broad layers of muscle with no intervening decidua would be brought together superficial to the first suture. He used to allow patients who had had emergency Cæsarean section performed to manage their labours themselves subsequently, after induction of premature labour, without any anxiety, but he began to wonder now whether any Cæsarean section scars, in the upper uterine segment, were really perfect, and after seeing these sections of scars agreed with Dr. Eden that the dictum "Once Cæsarean section, always Cæsarean section" seemed the only safe advice. The danger of subsequent rupture was a very strong argument against performance of Cæsarean section in young women for such conditions as eclampsia and placenta prævia.

Dr. MCINTYRE in reply to Dr. Eden said that the importance of sepsis had frequently been emphasized. He could add nothing further from examination of his material to what was already known regarding this important factor in the healing of the wound. With regard to defects produced by inclusion of the mucous membrane between the edges of the wound, there was no evidence of such in the lower uterine scar examined.

### The Treatment of Eclampsia : An Improved Conservation Method.

By Professor B. B. STROGANOFF.

*Leningrad.*

AT the battle of Trafalgar the British navy lost 1,690 men, killed and wounded, whereas during the period of ten years from 1910 to 1920 there died in Great Britain and Ireland on the average 630 mothers per annum (22·5 per cent.) and double this number of children (47·8 per cent.), according to a very interesting article by Dr. T. W. Eden. In other words, the annual loss of human lives from eclampsia was higher than in this great battle.

It appears that in the United States of America the mortality from eclampsia is still greater. There is reason to believe that the application of the improved conservation method of treatment of this disease will diminish the mortality to a half (to 10 per cent.) or probably even considerably less (to 5 per cent. and less).

#### HISTORY OF THE METHOD.

I first applied the prophylactic method in 1897 and had no deaths from eclampsia in the first fifty-seven cases. At the International Congresses in Paris (1900) and in Rome (1902) I reported ninety-two and 126 cases of eclampsia respectively with a mortality of 5 to 6 per cent. In 1907 I visited Rosthorn's clinic in Heidelberg and there I saw a serious case of eclampsia on which Professor Rosthorn offered me to try my method. The patient had eleven fits in the course of nine and a half hours, and coma, with a pulse-rate of 150. After my treatment was commenced the patient had no more fits and rapidly recovered. Nearly the same results were obtained in 1909 in the clinics of Professor Schauta and Professor Pisceček, in Vienna, on two patients, and in 1911 in the clinics of Professor Bumm and Professor Franz (Berlin) where I treated three patients. In 1908 I was able to report on 360 cases of eclampsia with a maternal mortality of 6·6 per cent. and child-mortality of 21·6 per cent. (the latter being eclampsias *sub partu*). It was only after this that Professor Leopold, of Dresden, first applied my method in 1909, and obtained excellent results in the case of fifty patients, of whom only four died, and these were all suffering from severe complications in addition to eclampsia. In 34 per cent. of them there were no fits from the time of the beginning of the treatment, whilst 50 per cent. had only one to three fits. The mortality amongst the children was similarly favourable.

The second to adopt my method in Germany was Professor Krönig, of Freiburg. The results in his five cases of eclampsia were completely satisfactory both as regards the mothers and the children. Kapferer states in his dissertation (p. 57) that "the statements of the author [Stroganoff] were confirmed from point to point. The favourable effect of the treatment on the general course of eclampsia is evident."

Since then this method spread further in Germany (Zweifel, Lichtenstein, Zöpritz, Wertheim, Walthard, and others), in Switzerland (Herff), in Holland (Mingelen), in Sweden (Forssner), and even in the far Argentine (Professor Enrique Zarate).

In 1918 I was able to collect from the literature—which is far from complete—2,208 cases of eclampsia treated by this method and its variations, with a total maternal mortality of 9·8 per cent. Amongst 878 children born from

[July 3, 1924.

eclamptic mothers, whose history is recorded, the mortality after treatment by this method was about 12 per cent. lower than in cases treated by other methods in the same clinics. Acting on behalf of Professor Hinselmann Dr. Weisenfels discovered 1,094 more cases up to 1924, which raises the number of cases to 3,302, of which 357—i.e., 10·8 per cent.—terminated fatally.

Still better results were obtained by us during the last ten or twelve years after the method was somewhat modified. Out of 253 cases of eclampsia with fits at the State Clinical Institute of Obstetrics and Gynæcology (during nine and a half years), and the Krassovsky Lying-in Hospital (from 1910 to 1921, when it was closed) only six deaths, i.e., 2·4 per cent., were observed. As an estimate of the value of this method, these cases require at least a short description. In three cases we were dealing with hopeless patients. In one case we were unable to apply the prophylactic treatment in the absence of chloral-hydrate, and, lastly, in the two cases of relatively light eclampsia death resulted from pneumonia and septicæmia. Thus, we had not a single death from eclampsia in cases that had not been neglected, and obtained figures regarding which Dr. Fürst<sup>1</sup> quite justly remarks: "In the statistics of the whole literature these favourable results stand out unsurpassed."

#### THE SECONDARY EFFECTS OF THE TREATMENT.

The beneficent effect of this treatment is confirmed by a series of other facts.

Thus, (1) *the number of fits diminished distinctly*. In the course of the last ten years we obtained immediate cessation of the fits after the treatment was commenced in 40 per cent. of cases. In 45 per cent. the fits were repeated from one to three times, and only in 15 per cent. were they more numerous. In the last twenty-eight patients an immediate cessation of fits after commencement of treatment was observed in 53·6 per cent. of cases. From one to three fits occurred in 32·1 per cent. and only 14·3 per cent. of eclamptic patients had four or more fits. The maximum number of fits in the last group was seven and twelve; this, however, was the result of not quite accurate treatment. As soon as energetic treatment was employed the fits disappeared entirely.

(2) *There are a large number of "intercurrent" eclampsias, i.e., cases in which there was freedom from fits for twelve hours and more before delivery*. Formerly there were from 21 to 27 per cent. of such cases in prophylactic treatment. In the last group they amounted to 40·9 per cent. These figures do not, however, sufficiently characterize the power of the method in checking the fits, since many eclamptic patients are delivered in less than twelve hours after cessation of the fits. The efficacy of the method is more distinctly exhibited in cases of *eclampsia sub graviditate*; since 1916, 66 per cent. of such eclampsias manifested an intercurrent course. This is a great merit of the method, as the pregnancy progresses after cessation of the fits and the child is delivered mature.

(3) *Psychical disturbances* are very few in number and of a very mild character. Out of 253 cases of eclampsia there were only four, i.e., 1·6 per cent. In one case the patient had five fits and after delivery suffered from nephritis, when slight symptoms of psychosis were observed for some days. In another patient, who had only one fit *sub graviditate*, in the post-partum period symptoms of icterus gravis were observed, in connexion with which psychosis was observed for several days. Recovery followed.

(4) *The mortality of children is relatively favourable*. For the whole group

<sup>1</sup> Zentralbl. f. Gynäk., 1924, Nr. 16, p. 843.

it amounts to 14·3 per cent., for eclampsia sub partu et sub graviditate 20 per cent., and for post-partum 6·2 per cent. The group most interesting for us consists of the eclampsias ante-partum and intra-partum, since in these the action of the drug upon the foetus is manifested. The mortality in this group was 20·1 per cent., but if the premature children (weighing less than 2,000 grm.) who died before treatment was commenced from cold, operation, &c., are excluded, there remain only 6 per cent. attributable to eclampsia and treatment. And since 6·2 per cent. of foetuses also perished in post-partum eclampsias, very few deaths can be attributed to the action of drugs.

(5) In the whole group of 253 eclampsias there was not a single death of the mother due to treatment. As is known, in the concurrent method of treatment of eclampsia by forced delivery, some mothers die as the result of operation. In Bumm and Franz's cases about 4 per cent. of women died in consequence of operation.

(6) Diminution in the number of operative deliveries. These amounted to 56 per cent.

(7) Few cases of pneumonia. There were four of these.

(8) Rapid recovery of the patients.

On the basis of these 253 eclampsias, as well as of the analysis of the preceding 570 cases treated by the less perfect variety of the prophylactic method, *I have arrived at the conclusion that an absolutely favourable prognosis can be given for the mother in cases of eclampsia that have not been neglected.*

An extremely interesting confirmation of this is presented in the very large monograph on eclampsia produced by Professor Hinselmann, of Bonn, in collaboration with twelve other specialists. On p. 833 he writes:—

"The reason for choosing Stroganoff's method here is due to the fact that experience has shown its great efficiency. A glance at the latest figures of Stroganoff, in *Archiv für Gynäkologie* 1922, Bd. 116 and in the appendix, shows them to be so perfect that *any further investigation seems to be superfluous*. Further experience will show whether these figures of Stroganoff's are realized in the hands of others. If they are it will only remain to agree with Stroganoff that the problem of treatment of eclampsia has been solved."

Further, on p. 834, he says:

"I cannot refrain from noting the remarkable circumstance that our analysis, which started from quite different premises, entered into the same channels as Stroganoff's therapy."

On pp. 849 the following appears:—

"The rules which Stroganoff set forth for the 'improved prophylactic method'—we should like to name it Stroganoff II—include all the essential useful measures, which have been so well weighed and tested in practice that at present they represent the fundamental principles. I see no way of treating eclampsia against Stroganoff's fundamental principles, as they represent the essence of the entire obstetrical experience."

And lastly, on p. 854:—

"Thus the following is of significance in regard to prognosis. On account of the great efficiency of the modern treatment of eclampsia, the fate of the patient depends not upon the disease itself, but on the condition of the brain, lungs and heart on admission. The idea of severe and light cases should disappear (auszumerzen) as being erroneous."

I hope that the British and American cases of eclampsia will not be more severe than the Russian and German. I may add that even at present in some of the Russian institutions, in which the prophylactic method is not applied, the mortality is 39 per cent. (Professor Krivsky), rising to 41 per cent. (Professor Novikov) in certain years, and in the institutions and departments directed by me the mortality formerly reached 21-22 per cent.

## PROPOSALS FOR TRIAL OF THE METHOD IN ENGLAND.

I should like to verify this method here and, may be, even improve it; at the same time it is my desire to collect the forty-seven cases to make up the total of 300, as required by mathematicians according to the theory of probability, in order to arrive at exact conclusions.

(1) The first condition for an adequate verification of my method would be to give me the opportunity of taking part in the treatment of as many cases of eclampsia occurring during the following one and a half to two months of my sojourn in London as possible. The authorities in London could perhaps select four or five lying-in institutions in London and apply to their directors with the request to provide me with the necessary material. It is important to provide for this purpose a separate hygienic room.

(2) The exact application of the method is only possible with the help of an experienced assistant possessing a knowledge of the method. It is also necessary to have a midwife capable of looking after the eclamptic patient according to the requirements of the method. If a nurse is available, she should also be instructed accordingly. If it were possible to choose these persons beforehand, I would speak to them and instruct them in the technical part of the process. Of course it is important to have the necessary medicaments and instruments always ready (morphia, chloral, chloroform, oxygen, digitalis, instruments for venesection and for delivery).

(3) I consider the demand of Dr. T. W. Eden that eclampsia should be classified as mild, severe and moribund, to be very important. To the seven features by which he characterizes the severe cases I would add the following two: nephritis of long duration and fits recurring four or more times during two hours. It would seem to me desirable to establish another category of severe cases: those manifesting two-thirds of the symptoms of severity should be referred to as *morituræ*, or very severe cases, with an additional symptom—*presence of cerebral hæmorrhage*.

(4) Considering the importance of this experiment, not only for London and England, but for all civilized countries as well, it would be desirable to establish a special committee for the exact estimation and observation of the cases. Such a committee would naturally be composed of the directors of the lying-in departments of the corresponding institutions and presided over by a person elected by the Section of Obstetrics and Gynæcology of this Society.

I am prepared to present myself to take part in the treatment both by day and by night.

## THE SCHEME OF THE METHOD.

In conclusion, I will give a brief exposition of the treatment which was more fully described in the *Journal of Obstetrics and Gynæcology of the British Empire*, spring, 1923.

Eclamptic patients in most cases die from the destructive action of the fits upon the heart, respiratory centre, &c. Therefore the fits must and can be repressed. Eclampsia *sub graviditate* and eclampsia *sub partu* are treated with equal energy. Post-partum eclampsia runs a milder course, and therefore in most cases requires the administration of smaller doses of narcotics. Sometimes it is extremely severe, and then it requires maximum doses. During the fits the patient should be protected against injuries, the tongue protected against bites, and oxygen should be given after the fit to remove asphyxia. Chloroform is harmful in typical fits, but if the patient is breathing (clonic convulsions) it is useful.

(1) *Removal of all sources of irritation*: (a) If possible elimination of all noise; (b) room darkened; (c) examinations reduced to a minimum; (d) if possible, separate room and constant observation. Transfer to room under chloroform.

(2) *Administration of narcotics*: Morphia and chloral hydrate according to scheme below.

The beginning of treatment: 0.015 grm. (0.01 to 0.02 grm.), i.e.,  $\frac{1}{4}$  gr. ( $\frac{1}{3}$  to  $\frac{1}{2}$  gr.) of morphia.

One hour, 2 grm. (2.5 to 1.5 grm.), i.e., 30 gr. (38 to 22 gr.) of chloral hydrate.

Three hours later from the beginning of the treatment: from 0.015 grm. (0.02 to 0.01 grm.), i.e.,  $\frac{1}{4}$  gr. ( $\frac{1}{3}$  to  $\frac{1}{2}$  gr.) morph. mur. hypodermically.

Seven hours as at one hour—when conscious, *per os*, when unconscious, *per rectum*.

Thirteen hours: 1.5 grm. (2 to 1 grm.), i.e., 22 gr. (30 to 15 gr.) chl. hydr.

Twenty-one hours: 1.5 grm. (2 to 1 grm.), i.e., 22 gr. (30 to 15 gr.) chl. hydr.

Dose increased to 0.02 for morphia and to 2.5 for chloral hydrate in severe eclampsia in the case of strong individuals, and diminished to 0.01 grm. morph. mur. (0 hour and 3 hours) and 1.5 grm. chl. hydr. (1 hour and 7 hours) and 1.0 grm. chl. hydr. (13 hours and 21 hours) in mild forms. When the patient is conscious chloral hydrate is introduced *per os*, and in the unconscious condition *per rectum*, with milk and physiological salt solution  $\bar{a}\bar{a}$  100.

During the first two intervals we frequently administer chloroform 10 to 20 min. in the presence of prodromata of a fit. On the second day undelivered patients receive 1 to 1.5 grm. (i.e., 15 to 25 gr.) chl. hydr. t.i.d. In the absence of fits during fourteen hours, and when the patients are in good condition, the doses may be diminished.

(3) If the fit recurs two or three times or even once in a severe form, in spite of administration of morphia and chloral hydr., it is necessary to perform venesection, drawing off 400 c.c. of blood. This is not resorted to if delivery is expected within the next one to two hours.

(4) As soon as the eclamptic patient can be delivered without harm to herself and child, delivery is undertaken either with forceps, by extraction, or—rarely—by version. In the absence of contra-indications we rupture the bag of membranes if the os has dilated to two fingers in a multipara, and about three fingers in a primigravida.

(5) *Maintenance of the regular functions of chief organs*:—

(a) Kidneys and skin: Patient kept warm with hot water bottles at the feet and in the region of the kidneys; diluted hot tea with milk should be given; unconscious patient must have milk and physiological salt solution  $\bar{a}\bar{a}$  (about 1000 c.c. *per diem*), usually with chloral hydrate, *per rectum*.

(b) Lungs: Oxygen after a fit, pure warm air, removal of all hindrances to respiratory movements; unconscious eclamptic patients should be kept chiefly on the right side; careful cleansing of the mouth and nose from mucus, blood and vomited matter.

(c) Heart: Administration of fluids (see *a*) when weakened after numerous fits—digitalis (if the pulse-rate is 110 or higher)—and, when the heart is still weaker, camphor, caffein. Patients admitted from outside after six or more fits immediately undergo narcotic treatment and venesection. Continuous observation of patient and child during the first twenty-four hours after delivery.

In very severe cases 9 grm. (=135 gr.) chl. hydr. and 0.04 grm. (=  $\frac{2}{3}$  gr.) morph. mur. are introduced in the course of fourteen hours (the administration of narcotics is much more frequent than is shown in the scheme, until the fits cease) + chloroform narcosis several times. The forerunners of a fit are: increase of headache (which is recognizable in unconscious patients by the face, restlessness in bed), convulsive contractions in the extremities or in the face, dimming of vision, and especially increase of blood-pressure.

I may express my confidence that all the slight cases of eclampsia will recover if the method is applied in the proper manner; the severe cases also should not prove fatal provided they do not contract pneumonia or sepsis.

If this experiment results in the saving of even a few lives I should consider myself perfectly satisfied. It will, however, be of still greater benefit in the future.

In conclusion, I would like to express my deep gratitude to the Society for its willingness to render assistance in the elucidation of a problem which has hitherto produced so few results, in spite of the work of numerous scientists during two centuries.

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SECTION OF ODONTOLOGY



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## Section of Odontology.

President — Mr. DOUGLAS GABELL, L.R.C.P.Lond., M.R.C.S.,  
L.D.S.Eng.

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### Modern Improvements in Mechanical Dentistry.

#### PRESIDENT'S ADDRESS.

By DOUGLAS GABELL, L.R.C.P.Lond., M.R.C.S.,  
L.D.S. Eng.

AN inaugural address is one of our ancient customs and now that its character has evolved on scientific rather than purely historical lines, I propose to continue it with the hope that a few words on the modern changes in mechanical dentistry, now known as dental prosthetics, will be found useful.

I propose to survey only the last thirty-five years or so and those from the view point of the ordinary practitioner rather than of the super-specialist.

The method of preliminary examination and preparation of the mouth for dentures shows changes during this period, for the normal anatomy of the edentulous jaw is better known and more intelligently examined. May I call your attention to practical points clearly taught to our students to-day, which in olden times were merely described as "surrounding soft tissues."

[A series of lantern slides illustrating the anatomy of the mouth were then shown.]

A much greater change has occurred in our recognition of pathological conditions. I was taught how to grind down a "stump" and bevel its edges suitably for retention under a denture, later I listened to discussions on when to extract and when to retain roots; now of course they are never left under a denture. Bridges were then regarded as high-class work, now we know they are fundamentally bad. Even to-day the bridge error taints much of our partial denture work, and in this country as well as in America small plates are designed with too much regard to smallness of bulk and rigid fixation and too little attention to the prevention of stagnation about the natural teeth and especially the gingival margins.

In my early days tartar was sometimes left on the lower incisors to "support the teeth" and conveniently fill troublesome undercuts.

The danger of fitting a denture around a tooth with a pyorrhœa pocket was not appreciated and naturally enough cases of "mercurial poisoning" from red vulcanite and also stomatitis due to heat retention under non-conducting vulcanite plates were common. I am forced to admit that not every practitioner knows the real cause of these troubles to-day. Perhaps the same may be said as to allowing, even encouraging, patients to wear partial dentures at night, in fact continuously, "for fear the mouth should change shape." I can remember

## 2 Gabell: *Modern Improvements in Mechanical Dentistry*

gold plates that had to be cut out, the gum having grown over and neighbouring flaps become united. I have not seen this now for many years but I very frequently see mouths very badly injured from wearing plates at night and it is often very difficult to get all the swelling reduced before making the new denture. When the patient will not submit to leaving out the plate, a drastic cutting back of the base plate and one or two relinings will sometimes secure a firm gum foundation which does not "alter in the night" if the plate is left out.

There was very little recognition of the cause of marked absorption or great thickening of the alveolar bone in the edentulous jaw or of the soft patches on the palate or alveolar border. They were regarded as natural peculiarities of the patient and the thought of treatment, curative or preventive, seems to have been limited to the totally ineffectual alum mouthwash. Even to-day, when I read that the shape of the edentulous jaw may be used as a guide to the pattern of tooth to be selected for the case I wonder whether pathology is studied by prosthetic-dentists. Our advance in the treatment of pyorrhœa should not only make artificial dentures less often needed but also prevent many of the evil conditions that hamper the prosthetist. But, as is usual with new discoveries, some men are apt to run to extremes, so I think that the men who attempt to anticipate bone changes by dissecting out the teeth and cutting away the alveolar borders are going too far in nine cases out of ten. That in a fortnight the mouth will have attained to a permanent form I find hard to believe. That the shape surgically obtained is as good from the point of view of mechanics appears to me very doubtful, especially in the upper jaw. The risk involved by laying bare large areas of cancellous bone in a septic wound should be obvious. The very cases in which the removal of infected bone is most desirable are the very cases where the operation is most fraught with danger. I believe there are cases in which bone should be removed, but I believe that it should be done after the extraction wounds have healed, the mouth becomes cleanable and the need shown to be urgent.

Surgical treatment of the soft tissues is less dangerous surgically, and in thirty years the technique has greatly advanced, largely owing to experience obtained in the late war. And although the operation still remains tedious and painful in some cases, yet obstructive scars can be removed to improve the denture foundation.

But after all timely treatment to prevent these troublesome soft patches, obstructing scars and tender spots is more effectual than curative surgical treatment. It has been shown that the extraction of a tooth is not normally followed by the total removal of its socket, more often about one-third of the socket becomes filled with bone and an excellent foundation for dentures is thus provided. It is a smaller circle in both jaws than formerly, except in the lower molar region, but certainly a wider and better foundation than that produced by surgical operation or acute inflammation. The very wide upper edentulous jaws produced by "exostosis" are seldom favourable for dentures on account of the tenderness to pressure of these scars and the frequent persistence of infection within them. It is in such cases that I should like to know how far electro-therapeutics can help, and during the session I hope a contribution will be made on the subject.

Another great advance in the preparation of the mouth is the recognition of the permanent damage that is done by allowing the patient to go edentulous or even destitute of proper back teeth for a long period. The loss of the normal habit of mastication, the development of asymmetrical action, and a very wide range of movement are fatal to good articulation work in the artificial teeth,

and the destruction and deformity of the temporo-mandibular joint prevents even a willing patient from regaining the lost ground. I think our forefathers knew that a closed bite was incurable, but they did not recognize the rapid loss of normal masticatory movements and its anatomical sequelæ.

With regard to impression-taking I have recently recorded my view that the great advance is that we are now much more accurate and thoughtful than formerly. Both plaster and composition workers aim at securing even pressure on the foundation and an accurate depth at the margins, whereas formerly a "full" depth was aimed at and the plate-edge subsequently adjusted.

A very great measure of success has been attained in this matter, and pain at the edges of the denture is now more often due to other causes than inaccurate impression work. I fear that in general practice delicate work at the periphery of the impression is not yet universal, and that stock trays and viscous impression materials are still used, but I believe it is now very generally accepted that applying pressure to the outer alveolar plate is only a temporary expedient and of no lasting value.

With the improvement in impression-taking there has arisen a demand for better casts, and although the plain plaster cast and zinc dies are still in general use their imperfections are better known and many workers have forsaken them for more modern, harder and less variable materials. But I must record that some workers in the past took the trouble to get iron dies for their gold work, and it surprises me that with our modern small furnaces this practice has not become more prevalent.

Perhaps the casting process under sufficient pressure to overcome surface tension has something to do with the lack of advance in wrought plate work. Except for small work such as crown diaphragms, lugs to prevent sinking, and a few cases where a large and complex tooth surface needs to be accurately fitted and where strength can be provided by a wrought backing, I regard the use of cast plates as a retrogressive step. The strength and durability are admittedly very inferior. The cleanableness is inferior and the much vaunted better fit is very questionable. Four per cent. shrinkage in a flat mouth where a tyro could fit a struck plate may not matter, but with a high palate or well-marked tuberosities the defect is very obvious. Casting is, like vulcanite, a temptation to avoid good work.

What I have said with regard to the denture base is much more applicable to cast clasps. They are most difficult to cast so as to fit accurately, they are very inferior in resilience, and they encase far too much of the tooth with a none too cleanable gold surface. These cast clasps are usually an inheritance from rigid bridge work technique and have no place in clean plate work.

Bite-taking stands much as it did thirty years ago except for (1) a little more neatness and accuracy; (2) the use of composition blocks in place of wax, and more care being taken in carving the blocks to a correct contour and bulk; (3) a few new devices to prevent our making mistakes in the antero-posterior direction, and to get even pressure on both sides. But we are still without definite means of measuring the correct height of bite. The appearance of the face and the patient's sense of comfort have to be estimated. I think it is quite possible to devise a means of actually measuring the correct height of bite, but it has not been done yet.

Great accuracy in these matters does not yet appeal much to the "practical" man because the subsequent work is very apt with him to distort the positions of the teeth and render it futile.

#### 4 Gabell : *Modern Improvements in Mechanical Dentistry*

The articulation of the teeth and adjustment to accommodate the movements of the jaw have received immense attention, new articulators appear in rapid succession, but still we are far from possessing a perfect practical instrument. Practical advance has been made, much has been learnt; it is often possible now to make more efficient masticating dentures than we could thirty years ago; but still much is needed in simplicity of technique and accuracy of record and reproduction of the patients' movements before the general practitioner takes an anatomical articulator into his routine practice.

In many cases anatomical or "super-anatomical" articulation is beside the point altogether, because the patient has lost the habit and even the power of masticating naturally. In such cases the "flat" bite of thirty years ago is still the best that can be done. But all are not like that and fewer ought to be so crippled, and for these cusped teeth are an advantage. Mr. Wilson Charles's principle of fitting the artificial teeth to the natural teeth is very sound, indeed I think it is the best method where circumstances permit. The use of the various "average" approximation articulators followed by a thoughtful cutting-in of the teeth at the chair-side to the patient's own movements is the next best and more widely applicable method. There are many other kinds of technique that have not yet proved their worth.

The subject of retention has advanced. In edentulous jaws "springs" have not totally disappeared, but resort to them is felt to be very old-fashioned, and has a taint of inefficiency about it; good practitioners do not resort to the use of springs nearly so frequently as of old.

The "suction disc" has arrived in the guise of a multiplicity of ingenious devices, and has already been exposed as a "septic fraud"; it is still extensively used by the uneducated.

Air-tight marginal fit without pressure, though not always obtainable, is the goal that modern impression technique has placed within our reach in a very large majority of cases. The air lock is not necessarily at the actual edge of the denture, it may be a little on the gum side of it, or far more often on the buccal and labial surface, and a study of the anatomy of the buccinator and orbicularis oris when in action will enable us to so shape the outer face of the denture so as to secure retention at all times.

For partial work, I feel that progress is slow and fluctuating. Mr. Badcock many years ago showed hygienic plates, cut well away from the teeth, yet this type of denture is still scarce. Its construction demands more knowledge, care and accuracy than that required in the manufacture of the old wide-bearing many-clasped denture. The patient is nervous of it at first, and its immense value as a healthy and non-destructive apparatus is unknown to him. Hence the demand is small and the supply very inadequate, but it is the duty of dentists to push the use of the hygienic denture. Many dentists mistake smallness for healthiness; this is quite an error, for a hygienic denture may cover a large amount of gum, away from the teeth, and a small denture may form many undesirable food traps about the teeth. Elaborate clasp and clean devices are far too common. If you follow sound principles and work accurately, simple means will ensure efficient mastication and good retention. The general rule in partial work is to keep away from gum margins.

Even the despised springs are better mounted now than formerly, and attention is paid to the base upon which the plates rest as well as to the relative positions of the swivels and stops.

In partial work the clasps have been lifted away from the necks of the teeth, or at least they ought to be, wherever possible. The very deep band is now also discredited.

The lower edentulous jaw is now the principal *bête noire*, whereas I think that distinction used to be attached to a flat upper jaw. I cannot remember having been taught much about the action of the lips, cheek and tongue in the displacement of lower teeth, except that the frænum of the tongue must be avoided; now we have very much more instruction as to the "safety zone."

I regard as unreal the pretentious claims made as to the efficiency of a small backward extension of the plate on the lingual side of the ramus.

I have witnessed the adoption of celluloid dentures, and later the adoption of celluloid gum facings, and I have seen both go out of use. It was very pretty and light material, but it quickly deteriorated in the mouth.

Continuous gum can now be soundly and safely constructed with the clean and perfectly controllable electric furnace and pyrometer which have taken the place of the old gas or coke furnace, and the heat can be determined by colour only. Just now the expense of the necessary solid platinum base is prohibitive to all but the very few. And this class of work is seldom carried out frequently enough to make the dentist sufficiently experienced in the performance of truly artistic work, for the beauty of this work depends on a delicate technique in colour blending, contouring and correct fusing in order to obtain appearances which do not show during the building up, but only when the work has cooled. And all such work is heavy, fragile, and difficult to adapt or repair.

Low fusing porcelains have been introduced and have been found wanting in permanency, but porcelains of English make are procurable that are slightly more fusible than the early bodies and yet possess excellent working qualities and durability.

Several of our younger men have been devoting time and thought to the production of a practical instrument that will render gum section work more easy to join perfectly, and such an instrument would be valuable for other work if not too elaborate. I think there is an immediate prospect of the adoption of better methods here, and the Girdwood block is another return to sound artistic handcraft after a temporary lapse to ready-made misfits and "sloppy" plastic work.

The introduction of carborundum and the great improvement in the cutting of these stones by a judicious selection of grit and bond—work done mainly for the engineers, but of great benefit to us—has made the carving of porcelain a practical process.

Vulcanite is more uniform in strength, but unimproved in appearance. Its faults are becoming better recognized and hence its defects can often be largely avoided by the adoption of appropriate methods, though, unfortunately, they cannot yet be entirely overcome in this way. Much research has been spent on this material by a wide class of artisans and improvements in working qualities have been attained by some manufacturers, but, on the whole, dentistry has not much benefited. I have seen several new materials that are proposed as substitutes for vulcanite and their translucency is beautiful, but the colours are very wrong. Pink is apparently one of the most expensive and difficult colours to obtain in a permanent condition. There is no such thing as a strong light-pink vulcanite, yet hollow vulcanite work can now be produced with the confidence that it will come out truer to shape than solid work. But there is great difficulty in bulky cases in preventing binding on the outer alveolar face, undesired relief in the back part of the palate and distortion of accurate articulation due to tiny tilts in the teeth.

The appearance of dentures, with regard to the stock artificial teeth as



## 6 Gabell: *Mechanical Dentistry*; Dowsett: *Periodontitis*

supplied by the manufacturer are much improved to what they were thirty years ago, but they are still only stock teeth and practically always need cutting to make them appear natural in the mouth. The dentist with artistic feeling and technical skill in porcelain cutting is still far in advance of the man depending on good selection only.

The introduction of some system, be it true or untrue, in the classification of artificial teeth is a great help to students in learning what materials are at their disposal, and the number of good patterns even of all four of the principal makers is not too great to learn.

The introduction of "removable" facing in place of the soldered pins in plate work is an important technical advance. But the use of dowel crowns in plate work, though more perfect in finish, is so very much more unhandy in its manipulation that I still prefer the old tube tooth. Diatoric teeth constitute an example of the fact that cheapness does not mean nastiness.

The non-platinum tube tooth is inferior to its tube predecessor on account of its non-standardized tube. If these were ground to an accurate standard it would be a very great improvement.

The final "articulation" of a denture set up to a correct, evenly balanced, over-the-ridge, average normal "occlusion" with an approximate occlusal plane and machine-made articulation, far from spoiling its appearance or masticating efficiency, should improve both if it be done with our modern knowledge of the anatomy and the functions of the teeth cusps and sulci. In such work one always has the option of blunting the cusp or deepening the opposing sulci; of flattening out the occlusal plane or curving it more sharply according as we grind the upper or lower set and the result ought to be a more efficient and natural-looking denture.

Fewer springs, fewer clasps, longer cusps behind and better looking teeth in front, greater efficiency and cleanliness might well sum up our progress.

### Interesting Case of Rarefying Periodontitis.

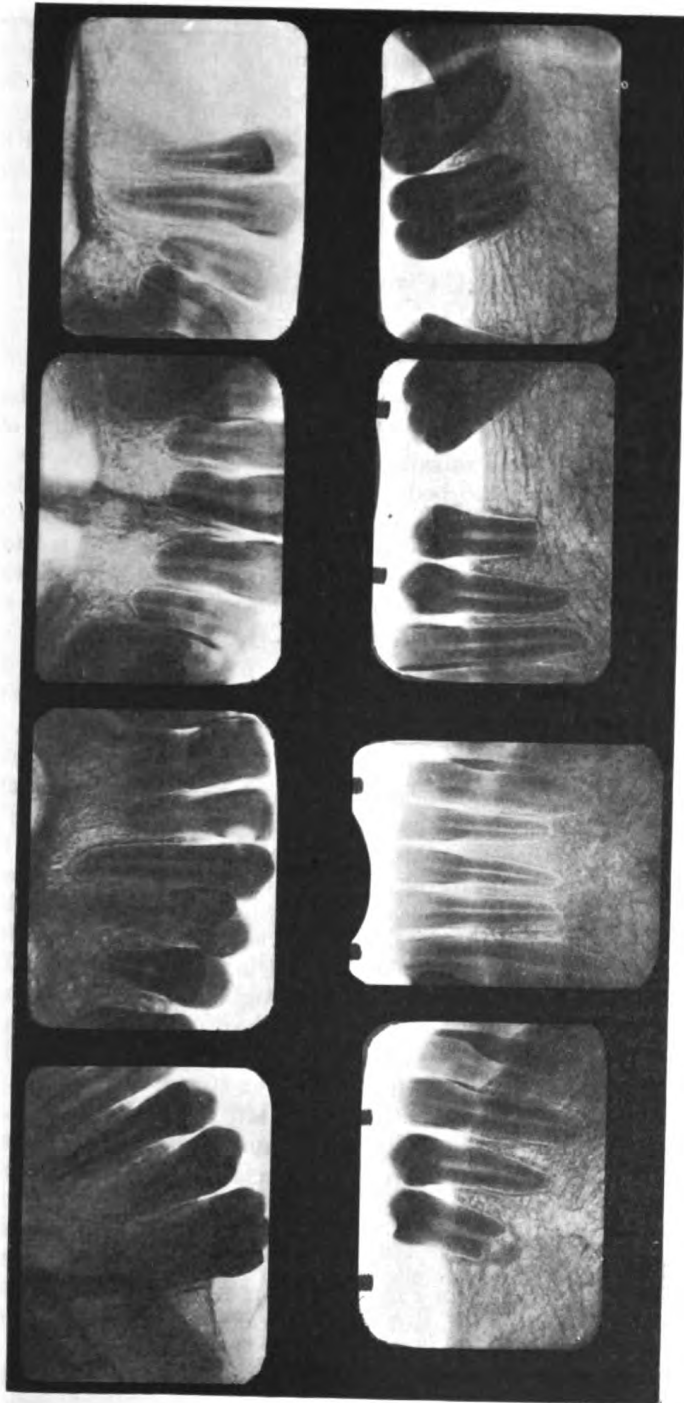
By E. B. DOWSETT, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

MISS B., aged 22, had been suffering from pains in her hip for about two years. Tubercle was first diagnosed and the patient spent some months in bed. A few months ago arthritis of the sacro-iliac joint and hip joint was diagnosed and she presented herself at Guy's Hospital. Her teeth were skiagraphed as a routine and the following interesting conditions were found. There was marked absorption at the apices of the following teeth :

|    |    |    |   |
|----|----|----|---|
| 54 | 21 | 12 | 4 |
| 5  |    |    | 5 |

The condition is interesting from the fact that the process seems to have been very slow; and in all the absorbed teeth, except the lower right second premolar, a reparative process has taken place in the bone around the apices, so that there are no cavities in the bone in the neighbourhood of the rarefaction, as usually found. The lamina dura is seen to exist in each case surrounding the fore-shortened roots, but the periodontal membrane is thickened in each case. In the lower right second premolar the rarefaction is more irregular and a small piece of the root of the tooth has become absorbed off and is free from the remainder.

There was no history of pain in any of the teeth, nor were any of them loose. There was only a very slight gingivitis around the whole mouth, but



Interesting case of rarefying periodontitis.

## 8 Dowsett: *Periodontitis*; Pitts: (?) *Dermoid Cyst of Mandible*

no pockets nor pyorrhœa were present. Did the infection, which it must have been, come from the gingival margin or from the blood stream?

The above mentioned teeth have been extracted and cultures made from the apices of each separately. In two cases, a mixed culture of *Bacillus lactis aërogenes* and *Staphylococcus albus* was found, but in the remainder there was found a pure culture of *Streptococcus longus*. It would appear, therefore, that the first mentioned organisms were probably contaminations, and that the condition was one of streptococcal infection. A vaccine has been prepared, and the patient is improving.

### A (?) *Dermoid Cyst of the Mandible.*

By A. T. PITTS, D.S.O., M.R.C.S., L.D.S.

PATIENT, a woman of 43, was seen by me at the Royal Dental Hospital, in December, 1921. She had a swelling in the lower jaw in the region of the left canine. The first premolar was missing, having been extracted earlier. A considerable area of bone was absorbed and fluctuation was obtainable. There was a sinus present and on pressure on the tumour some yellowish, granular débris could be squeezed out. A probe passed into the sinus led to a large cavity extending both towards the incisor and the second premolar. The patient stated that the swelling had been present for about eight years. A skiagram showed the presence of a large cavity extending from | 2 to | 5. A diagnosis of a suppurating dental cyst was made, probably in connexion with the extracted first premolar, since the canine and second premolar were not carious. The patient refused operation at the time, but in July, 1922, the cyst was opened under a general anæsthetic. | 2 3 5 were extracted. The mucous membrane was incised horizontally and reflected downwards. The bone was removed until the whole area of the cavity was exposed, and the septa between the extracted teeth were removed. No attempt was made to curette the cavity or to dissect out the lining membrane. There was a considerable amount of granular débris in the cavity. Healing was uneventful and was practically complete by October, 1922. So far the history is that of a typical suppurating dental cyst. I saw the patient next in January, 1923. The cavity had diminished considerably and was lined throughout with epithelium. The lip sulcus in the region of the cyst was deeper than normal, and, opposite, the canine terminated in a narrow chink which was quite clean. The patient complained that something irritated her tongue at times, "tickled it" was her phrase, which seemed to come from the cavity left by the operation. At first I could not see anything, but presently I discovered a long, coarse, white hair, about 1½ in. in length, which grew from the base of the cavity at the site of operation. It grew from the anterior aspect, that is, from the inner surface of the lip, but at a lower level than the normal sulcus, and opposite to the extracted canine. I cut off the hair and told the patient that when it grew again I would dissect it out. She was seen again in October and then informed me that recently she had pulled it out herself and that it had not recurred. I do not think it possible that the hair became implanted at the time of operation. I think it most probable that it was growing in the original cyst cavity but that it escaped notice when the cyst was removed as might easily happen. If this view be correct then the cyst could hardly have been a dental cyst as I originally assumed. The only type of cyst known to me which contains hair

[November 26, 1923.]

is a dermoid cyst. Is it possible that this cyst is a sequestration dermoid? Dermoids usually occur in situations where coalescence takes place between cutaneous surfaces. Dermoids of the face are not uncommon, but are most frequently found in connexion with the nose, orbit or cheek. Bland-Sutton describes various imperfections associated with the failure of union of the two halves of the mandibular process and it seems to me that this cyst may possibly belong to this category and represent a dermal sequestration. I put forward the suggestion tentatively, however, for I cannot prove it by histological evidence and I rely only on the presence of a hair discovered after the operation, but, by inference, present originally in the cyst itself. Whether this diagnosis be accepted or not the tumour is still one of considerable interest and rarity.

### A Dentigerous Cyst Apparently Associated with a Supernumerary Tooth.

By A. T. PITTS, D.S.O., M.R.C.S., L.D.S.

THE patient was a man, aged about 45, whom I saw in January, 1923, at the request of Mr. Marston, of Clapham. There was a swelling in the palate on the right side opposite the first molar tooth, though this had been extracted previously. Fluctuation was present and a ring of bone around the fluctuating area could be felt. I judged that an area of bone about the size of a shilling had been absorbed. The molars in the upper jaw on that side had been extracted. The patient gave me the following history: the swelling in the palate was noticed for the first time in the beginning of December, 1922. He then began to suffer pain in the first upper molar, which was tender to bite upon. A swelling developed in connexion with the tooth and burst apparently through the cervical margin. The patient described the discharge as a thick, viscid fluid. The 6<sup>1</sup> was extracted but without effect on the swelling in the palate. Later the 7<sup>1</sup> was extracted; there was a discharge through the socket, but this healed up and the swelling in the palate had reaccumulated. Except for the tenderness in the first molar the patient had had no pain. The sockets were completely healed when I saw the patient, and the external alveolar plate was not expanded or tender. I thought that probably the swelling was a dental cyst which had suppurated. He was anxious to have the swelling opened, so under local anæsthesia I made an opening through the palate in the centre of the fluctuating area. There was a gush of yellow, glairy fluid. There was no odour and nothing to suggest that there had been suppuration. The opening was enlarged to correspond with the area of bone absorbed. I found that the cavity was a very large one and seemed to occupy a considerable part of the maxilla. It was evident from its size that the antrum had been displaced and must have been reduced to a slit, though still existent, for there had never been any discharge through the nose. On examining the cavity with a strong illumination I saw a whitish body projecting from the outer wall of the cyst opposite to the first or second molar region. I seized it with a pair of forceps and it came away easily. It proved to be a small conical supernumerary tooth of the type not uncommon in the maxillary molar region. The root was embedded in the wall of the cyst but the crown was free and not covered by tissue. I did not make any attempt to curette the cyst or to remove the lining membrane as I feared that I might open into the nasal cavity in so

doing. Mr. Marston made a plate to cover the palate with a plug of vulcanite just projecting into the opening to keep it patent. I saw the patient three weeks later. He was syringing out the cavity several times a day and had been quite comfortable. I syringed the cavity myself and found that the fluid was returned quite clean and that it did not enter the nose. With the plate out the patient's voice had a hollow timbre, but with the plate in it was normal. I have seen the patient at intervals since and he has been quite free from pain or discomfort. He syringes out the cavity himself and states that the fluid is returned clean. There is no doubt that the cavity is gradually getting smaller. The cyst was so large that I wondered after the operation if I had done rightly in tackling it in so limited a way, so I took the patient to see Mr. Cleminson, Assistant Laryngologist to the Middlesex Hospital. His opinion was that the result was a good one and that it was inadvisable to do anything further so long as the patient remained comfortable. The possibility of a dentigerous cyst arising in connexion with a supernumerary tooth has been recognized but is rare. Gabell, James and Payne in their "Catalogue of Odontomes" state that four such cases have been described in connexion with supernumerary teeth. Whether we regard the dentigerous cyst as a tumour arising *de novo* in connexion with an unerupted tooth or a tooth supernumerary to the normal series, or whether we accept the view put forward by Mr. Sprawson that a dentigerous cyst is only a dental cyst which has secondarily invaded an unerupted tooth by extension, is still unsettled. In either case this cyst is, I think, sufficiently unusual to be worth placing on record.

## Section of Odontology.

President—Mr. DOUGLAS GABELL, L.R.C.P., M.R.C.S., L.D.S.E.

### Caries of the Teeth in Old-world Monkeys.

By Sir FRANK COLYER, K.B.E., F.R.C.S.

THE following notes on caries of the teeth of old-world monkeys have been obtained from an examination of the skulls in the British Museum, the Zoological Society's Collection, the Museum of the Royal College of Surgeons of England and a private collection. The number of skulls examined was 1,212; of these 795 were classed as from animals in the wild state, 417 from animals in the captive state: in all cases where a doubt existed the specimens have been included in the captive group.

The old-world monkeys belong to the family Cercopithecidae, which includes twelve genera distributed into two sub-families:—

(1) The Cercopithecinae—the diet is mixed, there are cheek pouches, the stomach is simple; (2) the Semnopithecinae—the diet is mainly leaves and shoots, there are no cheek pouches, the stomach is sacculated.

The following table shows the number of specimens examined of each genus and the incidence of caries:—

|                                     |     |     | WILD SPECIMENS     |                  |                        | CAPTIVE SPECIMENS  |                  |                        |     |     |    |     |     |
|-------------------------------------|-----|-----|--------------------|------------------|------------------------|--------------------|------------------|------------------------|-----|-----|----|-----|-----|
|                                     |     |     | Number<br>examined | Number<br>carius | Number<br>carius teeth | Number<br>examined | Number<br>carius | Number<br>carius teeth |     |     |    |     |     |
| Sub-Family <i>Cercopithecinae</i> . |     |     |                    |                  |                        |                    |                  |                        |     |     |    |     |     |
| Genus, <i>Macacus</i>               | ... | ... | 118                | ...              | 3                      | ...                | 8                | ...                    | 136 | ... | 14 | ... | 38  |
| „ <i>Cercocebus</i>                 | ... | ... | 11                 | ...              | 1                      | ...                | 5                | ...                    | 18  | ... | 1  | ... | 5   |
| „ <i>Cercopithecus</i>              | ... | ... | 216                | ...              | 12                     | ...                | 47               | ...                    | 150 | ... | 14 | ... | 37  |
| „ <i>Miopithecus</i>                | ... | ... | 7                  | ...              | —                      | ...                | —                | ...                    | 4   | ... | —  | ... | —   |
| „ <i>Erythrocebus</i>               | ... | ... | 8                  | ...              | —                      | ...                | —                | ...                    | 24  | ... | 6  | ... | 15  |
| „ <i>Cynocephalus</i>               | ... | ... | 66                 | ...              | —                      | ...                | —                | ...                    | 45  | ... | 7  | ... | 23  |
| „ <i>Theropithecus</i>              | ... | ... | —                  | ...              | —                      | ...                | —                | ...                    | 4   | ... | —  | ... | —   |
| „ <i>Cynopithecus</i>               | ... | ... | 3                  | ...              | —                      | ...                | —                | ...                    | 6   | ... | —  | ... | —   |
| Sub-Family <i>Semnopithecinae</i> . |     |     |                    |                  |                        |                    |                  |                        |     |     |    |     |     |
| Genus, <i>Colobus</i>               | ... | ... | 89                 | ...              | 2                      | ...                | 2                | ...                    | —   | ... | —  | ... | —   |
| „ <i>Semnopithecus</i>              | ... | ... | 259                | ...              | —                      | ...                | —                | ...                    | 30  | ... | —  | ... | —   |
| „ <i>Nasalis</i>                    | ... | ... | 14                 | ...              | —                      | ...                | —                | ...                    | —   | ... | —  | ... | —   |
| „ <i>Rhinopithecus</i>              | ... | ... | 4                  | ...              | —                      | ...                | —                | ...                    | —   | ... | —  | ... | —   |
|                                     |     |     | 795                |                  | 18                     |                    | 62               |                        | 417 |     | 42 |     | 118 |

The percentage of specimens with caries in the wild specimens was 2'2, in the captive specimens, 10'0.

Mr. J. G. Turner<sup>1</sup> found caries in 5'31 per cent. of the 433 skulls he

<sup>1</sup> "Dental Caries from a Clinical Standpoint." Annual Reports, Royal Dental Hospital, 1913.

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examined in the British Museum and in 16'66 per cent. of those examined at the Zoological Society's Gardens. The difference between Mr. Turner's results and my own is due to—

(1) The division of the British Museum specimens into wild and captive groups.

(2) The inclusion in the captive group of specimens other than those in the Zoological Society's Collection.

In the following table is shown the number of individual teeth affected and the position of the carious cavities.

|                     | WILD           |                     |                                   | CAPTIVE        |                     |                                   |
|---------------------|----------------|---------------------|-----------------------------------|----------------|---------------------|-----------------------------------|
|                     | Crown cavities | Approximal cavities | Complete destruction of the crown | Crown cavities | Approximal cavities | Complete destruction of the crown |
| Maxillary teeth ... |                |                     |                                   |                |                     |                                   |
| I <sub>1</sub>      | —              | 19                  | —                                 | 2              | —                   | —                                 |
| I <sub>2</sub>      | —              | —                   | —                                 | —              | —                   | —                                 |
| C                   | —              | —                   | —                                 | —              | —                   | —                                 |
| Pm <sub>1</sub>     | —              | 1                   | 1                                 | —              | 2                   | —                                 |
| Pm <sub>2</sub>     | 1              | 2                   | —                                 | 1              | 2                   | —                                 |
| M <sub>1</sub>      | 2              | 2                   | 1                                 | 21             | 9                   | 3                                 |
| M <sub>2</sub>      | 2              | —                   | —                                 | 14             | 1                   | —                                 |
| M <sub>3</sub>      | —              | 1                   | —                                 | 12             | —                   | 1                                 |
| Mandibular teeth    |                |                     |                                   |                |                     |                                   |
| I <sub>1</sub>      | —              | 9                   | —                                 | —              | —                   | —                                 |
| I <sub>2</sub>      | —              | 2                   | —                                 | —              | —                   | —                                 |
| C                   | —              | —                   | —                                 | —              | —                   | —                                 |
| Pm <sub>1</sub>     | —              | —                   | —                                 | —              | —                   | —                                 |
| Pm <sub>2</sub>     | 1              | 2                   | 2                                 | —              | 1                   | —                                 |
| M <sub>1</sub>      | 2              | 4                   | 2                                 | 9              | 5                   | 2                                 |
| M <sub>2</sub>      | 2              | —                   | 2                                 | 6              | 5                   | 2                                 |
| M <sub>3</sub>      | 2              | —                   | —                                 | 16             | 2                   | 1                                 |
|                     | 12             | 42                  | 8                                 | 81             | 27                  | 9                                 |

These figures show a marked difference in the position of the carious cavities in the wild and captive specimens.

The details of the eighteen wild specimens showing caries are the following:—

(1) *Macacus fascicularis*. ♀ B.M. 9.4.1.26. From Pangansaran.

| M<sub>1</sub> complete destruction of crown. It is doubtful if the destruction is solely due to attrition or in part to caries.

M<sub>1</sub> | approximal caries anterior and posterior aspects. The caries appears to have started at the cervical region. On either side of the tooth there are definite food pockets.

The molar teeth of this specimen show considerable attrition.

(2) *Macacus bintangensis*. B.M. 9.4.1.23. From Bintang Island.

| M<sub>1</sub> mesial aspect. The dentine of the crown is exposed, the attrition being more marked towards the anterior aspect. There are food pockets on either side of the tooth.

| M<sub>2</sub> The anterior portion of the crown surface shows deep attrition with commencing caries.

(3) *Macacus sinicus*. ♀ B.M. 19.6.2.2. Kolagiri, Nilgiris.

Approximal cavities on distal aspects M<sub>1</sub> | M<sub>1</sub> and distal aspects of Pm<sub>2</sub> | Pm<sub>2</sub>.

In this specimen there is considerable destruction of the alveolar

process and much attrition of the teeth. The caries started at the cervical margin.

- (4) *Cercocebus collaris*. ♂ B. M. 5.5.23.1. Efulen, Cameroons.  
The teeth show advanced attrition.

$M_1$  |  $M_3$   $\overline{M_3}$  | caries, occlusal surfaces.

$M_1$  | anterior root only remains.

$M_2$  | lost ante mortem.

- (5) *Colobus caudatus*. B.M. 11.4.7.172. Solai, British East Africa.

|  $M_1$  Tooth considerably destroyed. Caries started cervical margin. Big food pockets in mandible between the second and third molars.

- (6) *Colobus* (not classified). B.M. 4.6.2.3. Monrovia (Liberia).

| 1 Caries palatine aspect, cervical region.

Definite periodontal disease.

- (7) *Cercopithecus neglectus*. B.M. 6.11.1.1. Charada Forest, Kaffa.

|  $M_1$  Internal-palatine aspect destroyed following attrition.

|  $M_3$  occlusal surface.

$M_2$  | commencing caries occlusal surface.

|  $M_1$  two roots alone remain.

|  $M_2$  two roots alone remain.

|  $M_3$  cavity on lingual aspect.

|  $Pm_2$  deep cavity anterior portion crown surface. The dentine in this region exposed by attrition.

|  $Pm_2$  deep cavity crown surface following on attrition.

- (8) *Ceropithecus centralis*. ♂ B.M. 13.10.18.4. East of Amala River, British East Africa.

$M_1$  |  $M_1$  cavity on lingual aspect following attrition.

- (9) *Cercopithecus pygerythrus*. ♂ B.M. 6.11.8.1. Coguno, Inhambane.

$M_1$  |  $M_1$  anterior half of each tooth destroyed by caries.

$Pm_2$  |  $Pm_2$  roots alone remain of each tooth.

$I_1$  |  $I_1$  caries mesial aspects. The caries appears to have started in the cervical region.

- (10) *Cercopithecus pygerythrus*. B.M. 6.11.8.4. Coguno, Inhambane.

|  $Pm_1$  root only.

|  $Pm_2$  caries cervical margin.

$Pm_2$   $Pm_1$  | caries mesial aspects commencing at cervical margin. Alveolar process around the maxillary second premolars and first molars destroyed.

- (11) *Cercopithecus denti*. B.M. 7.1.2.1. Ituri Forest.

$I_1$  |  $I_1$  Caries mesial aspects at cervical margin in |  $I_1$  distinctly more

$I_2$  |  $I_1$  marked towards palatine aspect;  $I_1$  | root only.

- (12) *Cercopithecus denti*. B.M. 14.6.2.5. Duye River, 10 miles east of Penghe, Congo Forest.

Caries mesial aspects all four first incisors.



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(13) *Cercopithecus denti*. B.M. 20.10.17.4. Mt. Beni, Semliki Valley, Congo.

Caries mesial aspects of maxillary first and mandibular right first incisors.

(14) *Cercopithecus whitesedii*. ♂ B.M. 11.10.19.2. Nsoli, Bompana, Congo. Caries mesial aspects of the four first incisors and the distal aspects of the mandibular first incisors. The mandibular left second incisor has been lost post mortem. The caries seems to be a definite "eating away" of the enamel due to food lodgment. The teeth in this, as in many specimens, are covered with a dark "resinous" material.

(15) *Cercopithecus whitesedii*. ♂ B.M. 11.10.19.3. Nsoli, Bompana, Congo. Caries mesial aspects maxillary first incisors.

(16) *Cercopithecus mona*. ♂ B.M. 71.7.8.1. Mt. Cutter, Cameroons. Caries mesial aspects of the first incisors; distal aspect mandibular left first incisor, and mesial surface second incisor.

(17) *Cercopithecus petaurista*. ♂ B.M. 11.6.2.1. Bibianaka, Gold Coast. Caries mesial aspect maxillary first incisors. The cement between the mandibular incisors shows signs of softening.

(18) *Cercopithecus* (not classified). B.M. 21.3.2.1. Caries mesial aspects maxillary first incisors.

It is of interest to note that three out of four *C. denti* and two out of four *C. whitesedii* showed caries in the incisor region.

The specimens afford evidence that the caries in these wild animals does not commence in the enamel, but either in the dentine after it is laid bare by attrition, or by portions of enamel flaking off the approximal surface, or in the cement following injury of the periodontal membrane.

The caries in the incisor region is of great interest. Monkeys have a habit of pulling at food with their front teeth and it may be that certain foods are liable to injure the gum margin. The fourteen cases of captive cercopithecues showed thirty-seven carious teeth, the positions of the cavities being as follows: labial surface, two cases; occlusal surface, twenty-nine cases; approximal surface, six cases. In the teeth showing occlusal cavities the caries had started in the enamel. The captive macaques and baboons show a similar preponderance of occlusal cavities in the carious teeth, the figures being: (1) macaques, twenty-four cases in thirty-eight teeth; (2) baboons, sixteen cases in twenty-three teeth.

The evidence afforded by the specimens shows: (1) That the Cercopithecinae are more liable to caries of the teeth than the Semnopithecinae. (2) That caries of the teeth is more common in animals kept in captivity than in those living in a free state. (3) That the type of caries usually seen in captive animals differs from that usually seen in those from the wild state.

**Dermoid and Dentigerous Cysts in Animals.<sup>1</sup>**

By FREDERICK HOBDAY, C.M.G., F.R.C.V.S.

It will be of interest to Members of this Section present to have their attention drawn to the fact that dentigerous cysts exist in animals. In veterinary practice they are most commonly met with in the horse and are of considerable importance in the question of examining a horse for soundness. In dentigerous cysts the teeth found are always supernumerary and the commonest situations are the petrous temporal region—immediately behind and under the base of the ear—and the testicle. It is a peculiar anomaly that molar

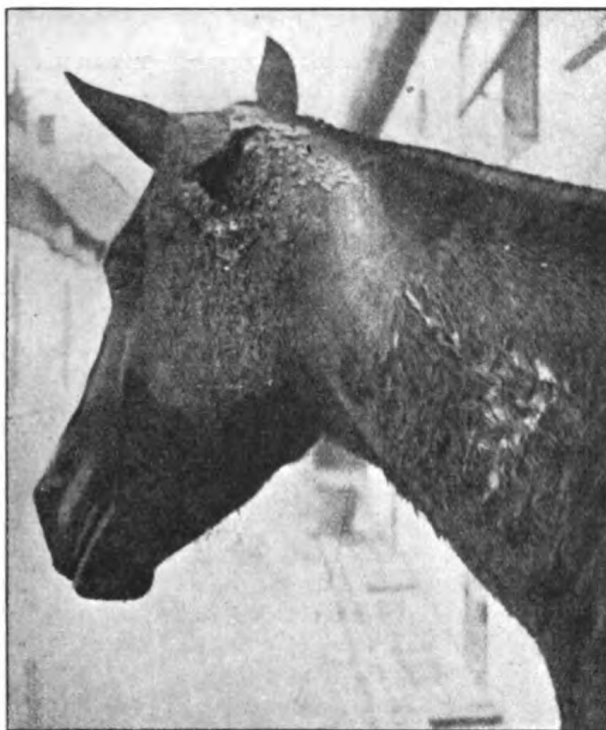


FIG. 1.—The usual site (under the ear) of a dentigerous cyst in the horse. After operation.

teeth are well known to exist in dentigerous cysts of the testicle of the horse, but in man and in domesticated animals other than the horse this condition is rarely found. On the contrary, dentigerous cysts of the ovary are not uncommon in women, whereas in the domesticated animals they are practically never met with. I have with me, this evening, a number of actual specimens taken from dentigerous cysts in the petrous temporal region of the horse, and

<sup>1</sup> This communication is the substance of remarks made in discussion on Mr. A. T. Pitt's case of dentigerous cyst. See *Proceedings*, 1924, xvii (Sect. Odonto.), p. 9.

[November 26, 1923.]

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I can show on the epidiascope representations of actual patients, demonstrating the situation before and after operation. The operation consists in cutting down upon the affected part under anæsthesia and in the removal of the teeth or tooth by the aid of forceps or other means. As a rule the operation wound heals up well and the sequel is a perfect success.



FIG. 2.—The two mal-formed molar teeth removed from the dentigerous cyst under the ear of the horse in fig. 1.

[Mr. Hobday showed a number of photographs on the screen by the aid of the epidiascope, two of which are here reproduced. (See figs. 1, 2.)]

## Section of Odontology.

President—Mr. DOUGLAS GABELL, L.R.C.P., M.R.C.S., L.D.S.E.

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### DISCUSSION ON PYORRHŒA; ITS PREVENTION AND TREATMENT.

Mr. J. G. TURNER.

MY object to-night is to provoke a discussion, and I propose to do this partly by leaving blanks to be filled in by other speakers, and partly by throwing out challenges which I hope will be taken up.

The measures of prevention and cure a man adopts should depend on his conception of the natural history of the disease in question. I regard pyorrhœa as the result of an attack made by various germs, chiefly of low virulence, on the alveolar and dental tissues—an attack always originating at the necks of the teeth. The germs harbour on the necks of the teeth and in the periodontal sulcus.

To the surfaces of the teeth the germs cling so tenaciously that the tooth may be decalcified and sectioned without disturbing them. This I regard as a fact of prime importance. Resistance to the attack is measured by the vital force of the tissues and is affected by local peculiarities. The vital forces may be lowered by any condition of illness, from overwork to intestinal stasis, and the local disease may be expected to improve, and does improve with the amelioration of the general disease. The local peculiarities which affect resistance lie in the natural contours of the teeth and in the arrangement of the teeth in the mouth. Both these peculiarities are such as to ensure the stagnation of germs, and fine-ground sticky food, such as our present-day flour, in close contiguity to the gum edges.

I regard pyorrhœa as a "dirt" disease, the dirt being germs and sticky food débris, and I am of opinion that as long as we persist in eating fine-ground sticky flour the normal mouth will suffer from pyorrhœa.

In the absence of "dirt" no general disease will produce pyorrhœa; in its presence the healthiest being sooner or later shows clinical signs of the failure of resistance. Such well known facts as the absence of gum-disease in clean mouths during mercurial treatment or during attacks of scurvy, and the ready response to local cleansing treatment in absence of all general treatment, prove the first point, and I assert the second as the result of thirty years' close clinical observation.

It will seem, then, that my idea of prevention begins with a healthy body, a body fed on fresh food and having its intestinal tract in good working order.

These desiderata are not unobtainable. Next I want food that is not sticky; and here, I fear, I am asking far too much. With our present flour, the best I can suggest is to eat the bread stale—three or four days old. It will be found then to eat far cleaner than when new.

Next, I want the dental arches as large and as regular as possible. This means avoidance of all forms of dental overcrowding, and involves especially the early removal of adenoids, since their presence invariably leads to subnormal growth of the maxilla and very generally to lateral compression by mouth breathing.

Then I want the teeth arranged in these large arches in such a way that the neck of each tooth is cleansible in its entire circumference. That is I want each tooth spaced from its neighbours to such an extent that 3-ply worsted will pass easily between them. The best way to obtain this spacing is to extract the first permanent molars at the age of about 14 to 16. With a quickly developing child it will be necessary to anticipate my age of 14. I select the age of 14 to 16 because at that age while the movements of teeth which follow naturally on extraction and are dependent on growth and the soft state of the bone, are not yet in abeyance, yet the bone is sufficiently set to withstand, to a large extent, the compressing action of the lips and cheeks. In cases of unilateral early extraction of the first permanent molar I have found that later in life—at 40 years of age—the extracted side is free from pyorrhœa while the other side is badly diseased. This means of prevention is founded first on preservation of the first permanent molar till the alveolar arch is of full size (excepting the wisdom teeth) and secondly on extraction to obtain the spacing allowed by the large arch. This method is equally effective as a prevention of interstitial decay. The possibility of mal-occlusion and traumatic occlusion do not alarm me in the least. The inevitable sepsis of a normal dental arch in presence of our sticky food is far more to be dreaded.

The other thing I want is an antiseptic which will eat its way through the overlying mucus, dissolve the sticky food débris, find its way into the periodontal sulcus without being put there, and kill the ultimate germ adhering to the tooth neck. This I have not yet found.

I will now discuss the treatment of the established condition. I have already indicated the scope of general treatment in considering prevention, but I am sure that general treatment alone will never cure a case of pyorrhœa; it may, and should in some cases ameliorate pyorrhœa, but that is as far as it will go. Local treatment is by far the more important factor in producing a cure. My objects in local treatment are to clean, render cleansible, and teach the patient to clean. If the last condition be fulfilled we may hope for what is practically a cure, though years of experience have shown me that even in "cured" cases there is only too often a slow destructive process at work in the gums and bone.

In order to clean the teeth all tartar must be removed, all rough fillings and overlapping edges smoothed away—and it is often easier to make a new filling than to polish away overlapping subgingival edges—and all ill-fitting caps and crowns should also be removed. The pain of scaling is chiefly due to the tenderness of inflamed gums, the cementum of the tooth is singularly insensitive, and true tooth pain only occurs when the dentine is exposed. If the greater part of the tartar be removed and the necks of the teeth carefully sponged in their entire circumference with small rolls of cotton wool soaked in bicarbonate of soda dissolved in weak carbolic solution, followed by

a similar application of tr. iodi on two or three days in succession, the tenderness of the gums will rapidly subside and overlapping fillings can be more easily dealt with.

The next step is to decide whether any teeth are to be extracted. Some stand condemned by their looseness, some by the extent and inaccessibility of their pockets, some by the extent of caries, and after this comes extraction for drainage, that is, to render cleansible. Alternate extraction gives fullest expression to this form of treatment. If 8-6-4-2 be extracted, the remaining teeth, excepting the centrals, are isolated and are accessible to cleansing. Some modification of this alternate extraction will often be found very valuable in practice.

After the condemned teeth have been extracted and the remaining teeth have been rendered as near aseptic as we can hope, pockets should be dealt with by excision of the gum flap, leaving the denuded root accessible to cleaning. If the cutting be done too early, infection of the wound is sure to follow and the tooth may be lost through an acute infective periodontitis.

After excision of the gum flap, it is well to see the patient daily for five or six days and to clean the teeth and exposed roots. The gums will heal if the tooth surfaces be free from infection. The ideal in excising gums is to leave the gum attached to the tooth at the same horizontal level all round, an ideal by no means always attainable. Carious cavities will obviously be easier dealt with after extractions and gum excisions have been done, and it is important to remember that well-finished fillings are essential to cleansibility.

The next thing is to teach the patient to clean his teeth. Unless he can clean every exposed tooth surface down to its last millimetre, recurrence of the pyorrhœa is only a matter of time, generally a short time. The means we have at disposal are the toothbrush, waxed silk and worsted thread. The brush covers but a small part of the ground. The neck of the tooth is the important area to clean, and though by proper tilting of the brush the labial or buccal part of the neck may be reached, even here the last millimetre is liable to be missed. Waxed silk can be used only in one direction, vertically: if drawn to and fro in a horizontal direction it cuts the soft tissues. Worsted thread is the best means I have discovered of cleansing the necks of the teeth. Three-ply worsted is strong enough to take a fair strain, it is woolly enough to soak up fluid readily, thin enough to pass between slightly spaced teeth, and soft enough to be harmless in the way of cutting into the soft tissues when drawn to and fro. Soaked in a cleansing solution it can be ringed entirely round a tooth, or looped partly round it, worked right to the bottom of the periodontal sulcus and drawn to and fro in the horizontal plane so as to act as a mechanical cleanser. The important point is always to work in a loop or a ring closely applied to the tooth. To obtain access for the worsted it is good practice to make just enough separations between the teeth to allow of its passing up. This may be done with steel strip saws, followed by steel polishing strips. Where there are abutment fillings, no hesitation need be felt in making this separation, but even where it is necessary to polish away enamel I have no hesitation in doing so. The separation need not be of great extent and never need be enough to allow of meat lodging. If 3 in. of one end of a length of worsted be waxed, leaving the rest to soak up the cleansing fluid, the waxed end will pass through an appreciably narrower space than the soaked part. The fluid to be used aims at being cleansing and not at being germ-killing. A weak acid, or a weak alkali, will disintegrate or dissolve

mucus and will supplement the mechanical action of the worsted far better than antiseptics, indeed by their coagulating action antiseptics will often frustrate the use of the worsted. The object is to wash away the germs, not to kill them *in situ*, which often means merely killing the superficial layer and leaving the deeper layers protected by a coagulum.

Unfortunately the shapes of the teeth will not allow of perfect cleansing. A transverse section at the neck nearly always shows two slight depressions interstitially, and over these worsted will bridge and so the adherent germs will escape. To meet this I use, and instruct the patient to use, the weak tincture of iodine (*tr. iodi mitis*) after using the worsted, painting it on the necks of all the teeth. The iodine is used every third day. It will stain, but what it stains is not the tooth, but the still adherent germs, and the stained layers can be scraped off at each periodical visit to the dentist. There will also often be larger areas inaccessible to the worsted, especially where gum-excision has been practised. An intelligent patient can be taught to rub these areas with small cotton-wool swabs wrapped on the end of a blunt dental probe, first with a cleansing solution and every third day with iodine.

I have made no attempt at a general review of all the known methods of treating pyorrhœa. I have put forward my ideas and methods and I leave it to others to refute or to supplement them.

#### MR. E. STURRIDGE

said that he considered the prevention of pyorrhœa more important than its treatment.

He did not agree with Mr. Turner in some things he had said, and would therefore only refer to those points on which he differed.

He was surprised to hear Mr. Turner advocate the extraction of four first molars at the age of 14-16 years to procure spacing of the teeth and prevention of pyorrhœa in later life. He was convinced that such procedure was more calculated to cause pyorrhœa in later life than to prevent it. His experience was that the most troublesome cases to treat were those which, from the loss of molars or pre-molars, had developed mal-occlusion and abnormal spacing of the teeth.

Undue stress was often the sole cause of starting periodontal disease at points in the denture where the periodontal attachments and alveolar bone were submitted to traumatic occlusion. The endless irritation from this resulted in chronic inflammation, and consequent formation of subgingival calculus on the roots of teeth so affected. He went to the greatest trouble in treatment to discover faulty occlusion, and to relieve undue stress by judicious grinding of teeth, and also by replacing missing teeth to restore balance of articulation.

He felt sure that no cure of pyorrhœa could be expected when teeth moved badly at their attachments on every closure of the jaws. He considered this ætiological factor one of the most difficult to diagnose and to overcome, and the deliberate establishment of it by extracting the four first molars in early life was a pernicious practice in his estimation.

With regard to alternate extraction of teeth he understood Mr. Turner only resorted to this practice in extreme cases. He had been shown one of these cases in progress of treatment, which at the time seemed to be doing well, the gums and attachment of the teeth being healthy, but he (Mr. Sturridge) would like to know what the effect would be in years to come.

The advantage claimed of producing cleansible spaces appeared doubtful, because the brush could not cleanse the interspaces; and whereas the use of tape between these teeth (which was better than three-ply worsted) was effective, nevertheless patients who would not cleanse ordinary spaces, where all the teeth approximated, were not likely to change their habits and be careful in regard to large spaces created for the purpose.

He did not think that iodine, or any antiseptic applied to the pockets and gum surface, would be sufficient to neutralize the bacteriological factor of pyorrhœa. It had been shown by the research work of Mr. J. G. Turner and Dr. A. H. Drew that bacteria infected the tissues and alveolar bone, as antiseptics applied to the tissue surface had no effect on deep-seated organisms, and reaction of the tissues must be slower (if they reacted at all) under the influence of infection.

This was why he (Mr. Sturridge) advocated ionic medication in treating periodontal disease; he knew of no other method of reaching organisms in the tissue, and considered it essential that the tissues should be sterilized as effectively as possible. He had good reason to be satisfied with this method of applying antiseptics.

#### Sir HARRY BALDWIN

said he did not agree with Mr. Turner as to the inception of pyorrhœa. In his opinion the first thing to become inflamed and infected was the free edge of the gum, which became infected by the ordinary micro-organisms present in every mouth. This infection took place when the gum was low in tone owing to absence of friction. Friction of the gum, best carried out with a toothbrush, would absolutely prevent the onset of pyorrhœa. The gums must be frictionized all over—behind and before—every square inch of them, at least once and preferably twice a day. The only antiseptic he used for this purpose was saline water. The friction acted by vitalizing the tissue. It enabled the tissue to resist infection. It also enabled it to eliminate the germs when the tissue was not too heavily infected. Friction of the gum was essential to prevention and formed an integral part of treatment. He (Sir Harry Baldwin) would never think of extracting six-year molars and cutting spaces between the teeth to prevent pyorrhœa; and if Mr. Turner really intended young practitioners to follow him in this respect he thought his teaching dangerous. In the treatment of pyorrhœa ionization with sulphate of zinc or sulphate of copper was a valuable method; if a little cocaine were added to the solution the pockets were soon rendered quite insensitive and advantage could be taken of their distension after withdrawal of the cotton wool to remove much unsuspected tartar. Though tartar was a most important accessory in the causation and exacerbation of pyorrhœa it could not be said to be the actual cause. He had seen many cases of severe pyorrhœa where there was no trace of tartar. He had also seen many cases of severe pyorrhœa where the jaws were well formed and the bite free from irregularities; where the patient was not a mouth-breather and where the patient had taken constant pains to clean the teeth but had taken equal pains to *avoid* any brushing of the gums.



## Mr. COLIN KEAY

said his view was that mal-occlusion might be regarded as one of the chief primary causes (other conditions being favourable) which led up to pyorrhœa. He contended, further, that it (mal-occlusion or so-called traumatic-occlusion) was present much more frequently than was generally recognized, but that the secondary symptoms (pockets, bacterial invasion, tartar) being more visible the condition was attributed to them, whereas in reality they were, in very many instances, the result and not the primary cause; the primary cause could only act when the vital resistance was lowered, want of resistance being more frequently met with in middle age than in youth.

He (Mr. Keay) asked members what was their experience of the cautery in opening up pyorrhœa pockets. Did it give a better result than that obtained by the use of the lancet?

He spoke in favour of ionic medication (ZnChl) in clearing up the discharge from pockets.

## Mr. F. W. BRODERICK

said he thought that they would not get very far in the discussion of the prevention of a disease in which the ætiology was so uncertain as was that of pyorrhœa. Mr. Turner seemed to be just as certain that they had in pyorrhœa a local disease brought about by local conditions, as he (Mr. Broderick) was certain that they had not.

Nevertheless, he would remind them that even those who advocated the theory of the local causation of pyorrhœa appeared to believe that certain general diseases predisposed to it; amongst such he instanced diabetes, certain types of nephritis, general wasting diseases and acute fevers. But these advocates of the local origin of pyorrhœa maintained that, although in the presence of these diseases there was some change in the vitality of the tissues which tended to bring about pyorrhœa, the diseases themselves were by no means essential as a causative factor, and that pyorrhœa might arise without them.

It was hardly the time or the place to discuss the ætiology of pyorrhœa, essential though this was to the matter under discussion; he (Mr. Broderick) had lately published an account of his ideas on the subject, which might be familiar to some of those present. He would point out, however, that if they took those diseases that predisposed to pyorrhœa and searched for the common factor among them, they would be going a long way towards arriving at a decision as to first causes.

In this search, however, it would be necessary to go very deeply into the pathology of these diseases, as otherwise they might miss the essential factors; for example, diabetes indicated an upset in the metabolism of carbohydrate, but nephritis did not indicate that. This did not mean, however, that the question of carbohydrate metabolism could therefore be dismissed, because other factors had to be considered; for instance, an upset in carbohydrate metabolism definitely interfered with the blood reaction. This was known to be a factor in certain forms of nephritis as well.

He (Mr. Broderick) believed that if they made a study of all the predisposing diseases they would find that this upset in the acid base equilibrium of the blood was common to them all. Further, he believed that in those cases of pyorrhœa that might seem to arise without any definite predisposing disease they would find a condition present that also tended to upset this equilibrium

in a milder way, and that this upset in all these cases was in one direction, viz., towards an excess of base.

From the point of view of prevention, he contended that this was a fact of the utmost importance, if they could remove or prevent this excess of alkali then pyorrhœa could not take place; if, on the other hand, it was present, then the utmost precautions would not prevent it, nor the utmost care in treatment cure it.

It was most difficult for him to understand why, if pyorrhœa was simply a dirt disease due to carbohydrate stagnation, it should occur so very much more frequently at certain periods of life and in certain types of people; and also why carbohydrate stagnation could produce pyorrhœa in one case and dental caries in another—conditions so very different, even if not, as he believed, antagonistic to one another.

He (Mr. Broderick) said that, if he was right in his suggestions as to the ætiology of pyorrhœa, then both in prevention and treatment they should aim at diminishing the quantity of alkali in the body; this was just as important, if not more so, than local treatment, and it would be brought about by either neutralization or elimination.

The simplest method of effecting this neutralization would consist in taking increased exercise, which, by increasing CO<sub>2</sub> formation, together with the acid products of fatigue, could be easily accomplished. Secondly, he suggested that a diet should be chosen that would give acid metabolites in excess, and for this purpose the cutting down of the carbohydrates would be essential, their place being taken as far as possible by fat.

On the question of elimination Mr. Broderick drew attention to the very small quantity of liquid that was imbibed in twenty-four hours by the majority of women; he mentioned that he had made a point of ascertaining this in all cases of pyorrhœa that came under his notice, and that it was rare to find that the liquid intake during this period exceeded 1½ pints. This was entirely inadequate, especially should there be an excess of salts to be eliminated.

Lastly, as to the question of vaccine treatment. He believed that this had generally been found to be most unsatisfactory, but he had recently been invited to try a new method of vaccine treatment authorized by Professor Besredka, of the Pasteur Institute. It was too early at present to go into details as to results, but he had been much struck with the rapidity with which suppuration had ceased on its administration, and with which pain had disappeared in those cases in which pain was a prominent symptom. How long this immunity would last remained to be seen, but he had purposely refrained from scaling and the surgical treatment of the pockets, so as to give every opportunity for suppuration to recommence. If the result of Besredka's cuti-vaccination really was an immunity lasting for any considerable time, then they had a method of treatment which, combined with scaling and excision of pockets, held out a considerable chance of success.

#### Mr. A. T. PITTS

said that he wished to comment on two points in Mr. Turner's address. The first was the advice to extract the first permanent molars at the age of 14 or thereabout as part of the preventive treatment of pyorrhœa. If it was certain that this would result in spacing, then something might be said for it. In his (the speaker's) experience it more commonly happened that

there was tilting of the teeth, which not only impaired the occlusion but might also lead to an awkward food pack. Orthodontists had been preaching for years the evil results following premature loss of the first permanent molars, for they recognized how often it led to mal-occlusion. He could only regard Mr. Turner's advice as being not only unsound but also unfortunate.

The second point was the suggestion made by Mr. Turner that if there was difficulty in passing worsted between the teeth, a small space should be made between the teeth with a disc. He could not accept the view that it was possible to make a space sufficient to allow worsted to pass, but too small to allow food being jammed within the space. It was one of the commonest clinical experiences to find that small spaces led to food impaction, and were thus a cause of pyorrhœa. It did not seem to him (the speaker) that it was a logical or rational proceeding to make a space between the teeth to facilitate their cleansing them, which was itself a predisposing factor in the production of the disease.

#### Mr. F. N. DOUBLEDAY

observed that much of the discussion had turned upon the treatment of certain well recognized predisposing causes, about which there was general agreement, but unless they were prepared to go further than this, and to make a careful bacteriological examination of each individual case their local treatment could not be rightly directed.

Some marginal infections were due to such organisms as *Micrococcus catarrhalis* and staphylococcus, and others were due to more virulent infections. In this connexion he thought it had not appeared from the discussion that the range of drugs used in local treatment had greatly increased in the last twenty years. Was there nothing to be gained by the use of arsenical preparations in the spirochætal types of infection? Would not the colloidal preparations, such as colossal iodine and colossal mercury, give helpful results in staphylococcal infections when used in local treatment of the gum. Trimethenal allylic carbide (yadil) was another preparation which he (the speaker) had found of use in some cases.

Mr. Turner's recommendation of the alternate extraction of teeth did not appear to rest upon any sound basis. Anatomically, with the exception of the upper third molar, and lower central incisor, each tooth of one jaw articulated with two teeth in the opposite jaw; if alternate teeth were extracted the effect of the inclined planes must be to cause the teeth to slide between each other, and not to articulate at all. This was a point which could be easily proved if Mr. Turner would bring forward models showing the mouths of his patients before he commenced this treatment of alternate extraction, and again models of the mouths of the same patients five and ten years after it had been carried out. He (Mr. Turner) did not appear to have done this, although he had been advocating this treatment for some years, and it was to be hoped that he would eventually put some evidence of this sort before them.

#### Mr. FREDERICK HOBDAY

said that he was much interested to hear of the prophylactic benefit attached to the effect of friction as a preventative of pyorrhœa because the experience in veterinary practice was that it was a disease which especially followed in

instances where there was a want of work for the teeth. Pyorrhœa was especially common in the pet dog, which was artificially house fed ; but it was comparatively infrequently met with in sporting dogs whose teeth had work to do in the way of eating a quantity of bones. From the comparative aspect the various shapes of mouths found in dogs constituted a subject of interest, especially those in which the teeth were very crowded together ; and he was only repeating a well-known fact when he said that pyorrhœa was much more prevalent in the domesticated animals than in those in the wild state. In conclusion he would ask whether it was possible for pyorrhœa to be transmitted from the dog to man. Dogs suffering from this complaint were frequently fondled by their fair owners, who would even allow them to kiss their lips and hands. He had often wondered whether it was possible for a dog to transmit the trouble to the owner by contact with the lips direct or through the medium of food contaminated by the hand or fingers, which had been licked by a dog the subject of pyorrhœa. He merely threw this out as a hint and again emphasized the great prevalence of this trouble in canine patients.

MR. ARTHUR BULLEID

remarked that he had always been rather impressed with the fuso-spirilla infection present in pyorrhœa and that he had sections showing this type of infection in the substance of the alveolus. At present he was unable to say of how great importance this infection was, though Kritchevsky and Séguin laid great stress upon it. He (Mr. Bulleid) was of opinion that local treatment only was quite insufficient in itself, as it could not combat the deep bone infection, and that it must be combined with efficient general treatment as well. The establishment of adequate drainage by radical excision of the pockets, really efficient scaling and polishing of the teeth, and treatment with novarsenobenzol locally had given good results. The general treatment he advocated consisted in the intravenous injection of novarsenobenzol, commencing with a dose of 0.15 grm. and working up to 0.6 grm. at weekly or ten-day intervals. Coupled with this, he advocated the administration of a lubricant, such as liquid paraffin, internally, and finally the administration of an autogenous vaccine prepared, if possible, from the organisms at the apex of an extracted tooth or else from one of the deepest pockets previous to excision. This autogenous vaccine should be prepared and administered at weekly intervals by a competent bacteriologist. He (Mr. Bulleid) had tried Besredka's vaccines, but was not able to say much about them. One trouble experienced was the difficulty of effecting the local inoculation satisfactorily ; contrary to Besredka's opinion, a marked general reaction was not uncommon.

MR. GERALD JACK

said that the best means of dealing with pyorrhœa was by prevention, and in order to prevent it, it was necessary to know the cause. Man was a very old institution ; the bulk of his time on earth had been spent in a primitive state, during which period his food had been hard to acquire, and, when procured, was eaten in a raw state, or, if cooked, was of a tough nature : he existed on flesh, roots, berries, fruit, and food of a similar nature, and he was bound by the very toughness of his food to keep his gums and teeth in a hard condition. Then towards the very end of this long primitive existence, he

commenced to become civilized, and his food conditions changed. Instead of eating his food hard, he cooked it until it was soft ; he became lazy in the matter of chewing, and of course his gums lost their tone and condition, and many individuals were bound to get their gums infected. Civilization had been too quick for Nature, which had not had time to adapt herself to the new conditions. Therefore, to prevent pyorrhœa, patients must be taught to keep their gums in condition by light friction, and the spaces between their teeth free from this soft clinging food by the proper use of a mouth-wash, as described by a previous speaker. When this was done properly, we should find pyorrhœa becoming a thing of the past.

[The discussion was adjourned till February 25, 1924.]

## Section of Odontology.

President—Mr. DOUGLAS GABELL, L.R.C.P., M.R.C.S., L.D.S.E.

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### ADJOURNED DISCUSSION ON PYORRHŒA: ITS PREVENTION AND TREATMENT.

Mr. C. H. HOUSDEN

said that it was evident the treatment of any oral disease must begin outside the mouth, i.e., by not wilfully introducing organisms into the oral cavity to infect the teeth and gums. This being so, the mechanical means we possessed for thoroughly cleaning the mouth must be introduced into it in a hygienic condition. The toothbrush was the ordinary means of cleaning the teeth and gums, at least, it was used by the majority; this being so, a clean brush must always be used—also one which was easy to keep clean. The bulk of bristles on the brush should not be more than 1 in. in length and  $\frac{1}{2}$  in. wide and  $\frac{1}{2}$  in. deep, the bristles being arranged in tufts, having quite a neck where these tufts were fixed into the handle, and widening out at the contact points of the tufts, then tapering to points, so that the cleaning edge of the bristles had, *in toto*, a serrated appearance. A brush of this kind could be mechanically kept clean, but it was advisable to keep it in some antiseptic, such as lysol. He (Mr. Housden) placed very much more faith in correct gum massage with the brush, than he did in mouth-washes, although these were useful in removing loose debris from around the teeth. The friction of the bristles was, in his opinion, a sufficient stimulus to the gums, if applied from above downwards in the maxilla, and below upwards in the mandible. Personally he thought this means of stimulus of far greater importance than any so-called germicide, passing through the soft tissues into the alveolus; surely a germicide of sufficient strength would upset the living tissues, if strong enough to destroy their attacking enemies. A much more important factor in treatment consisted in making sure that the infected teeth were functioning. If not, this should be corrected, so that the periodontal membrane had a chance of reacting to a masticating stimulus. It was no good treating so-called pyorrhœa with medicaments only; Nature must be given a chance of playing her part, which was the most important and in the performance of which dental surgeons were only her assistants. This point of function rather detracted from the soundness of Mr. Turner's suggestion of alternate tooth extraction. One should not entirely lose sight of the stimulating effect on the jaws of the "bite." This was a most important factor in the treatment of these cases in the way of clearing up congested areas by force. With regard to the contact point of the teeth, he failed to see how, if the teeth were spaced as Mr. Turner suggested,

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absolute freedom from stagnation areas was ever to be obtained. The inclined planes of the premolars would cause them to move in the direction of least resistance and this in time would promote the formation of "food traps." Surely, if each tooth was approximating to its neighbour with a force which would drive the contact points of the respective teeth into close proximity with one another, the food-packing idea was ridiculous, their aim was achieved, and débris could only enter by way of loose gum tags, which were carefully removed as low down as the infected alveolus shown in radiograms. He (Mr. Housden) maintained that the contact point was vitally important in the treatment of these cases, and where it was impossible to obtain such a result it was better that the tooth should be extracted. If all the teeth were loose enough readily to allow fibrous foods to wedge, then it was impossible to effect anything approaching a cure, unless the very greatest care were taken thoroughly to remove all débris from between the teeth after every meal. Their aim and ideal should be the practice of a healthy frictioning of the gums, and the formation of a sound contact point and the formation of a triangular space below the contact point, the base of the triangle being on a level with the necks of the adjacent teeth, and the sides of the triangle being of such smoothness that it would be difficult for food particles to adhere. In this way free drainage was maintained and when débris did collect in these triangular spaces the tongue would be able to play the part of Nature's vacuum cleaner.

#### Mr. F. ST. J. STEADMAN

said that one of the chief points of interest brought out in this discussion so far was the difference of opinion which still appeared to exist as to the cause of periodontal disease. It seemed to him that Mr. J. G. Turner, in describing the condition as a local "dirt" disease due to germ carbohydrate stagnation, had ruthlessly swept away many of the cobwebs which had been allowed to form around their conceptions of its ætiology, and on the other hand Mr. Broderick had been equally ruthless in endeavouring to replace these cobwebs.

That periodontal disease was in many cases due to sticky carbohydrate food which was not cleansed away either by the toothbrush or by the process of mastication, could be clearly demonstrated in the mouths of children. It was usual to see a young child with a perfectly clean mouth on one side, but on the other spongy, bleeding gums, the teeth foul, covered with masses of tartar, and surrounded with pockets from which food débris and pus could be squeezed. Even more frequent were the cases in which these conditions were present on both sides of the mouth. The cause in these cases was quite clear. An exposed pulp in one or more of the teeth or badly done fillings, with irritated pulps beneath them, accompanied by sinuses discharging pus, were almost invariably found. Drastic and thorough removal of the cause, thus restoring the remaining teeth to normal function, led in the majority of cases to a rapid disappearance of the gingivitis. Unfortunately, instead of removing the offending teeth many dentists persisted in filling them, consequently they remained tender, and function was not restored. As Sir Frank Colyer had pointed out, the removal of septic and tender deciduous teeth often resulted in an increased area of mastication as the six-year-old molars were brought into action (their use having been hitherto avoided during mastication on account of the tender teeth in front).

He had often watched a first permanent molar around which an area of

gingivitis and a pocket had been forming become normal again as the child was taught to use it. Again there was seen the gingivitis which occurred around the upper and lower incisor teeth due to mouth-breathing; this rapidly cleared up when the adenoids were removed, provided that the child was taught to breathe through the nose—a necessary sequel to the removal of the adenoids, which was sometimes neglected.

These cases were very common. Several were seen almost daily in a busy dental practice. The more he studied pyorrhœa the more was he convinced that it was due to neglect of the mouth in childhood. The gingivitis passing on to true pyorrhœa on one or both sides of the mouth in cases such as he had described was often left untreated even when the young patients were under constant dental supervision, and the precious years when so much could be done to cure the disease were allowed to pass; consequently the child lost the habit of using the affected side of the mouth entirely or, when both sides were affected, he learnt to bolt his food, so that when the permanent teeth replaced the deciduous, they, too, were not properly used, and soon became dirty and septic also.

He (Mr. Steadman) was becoming more and more impressed with the fact that periodontal disease was primarily a disease of childhood; in the majority of cases it began under 10 years of age. He believed that if a man reached adult life with clean and healthy gums he would probably keep them so for the remainder of his life. For he had learned to eat food and to masticate in a manner which left the mouth clean at the end of a meal, and the chances were that he would retain these habits unless he changed his environment and went where the food to which he had become accustomed was not easy or even possible to obtain. These cases such as he (Mr. Steadman) mentioned were very common, and, in his opinion, the conclusions to be drawn from a thoughtful and careful examination of them was that the ætiology of pyorrhœa was after all simple, namely, the lodgment of sticky carbohydrate food. It was in the main so simple that it was, in fact, apparently too simple for many minds to grasp. Faced, as they were, with such clear and definite clinical facts, it did not appear to be either reasonable or necessary to follow Mr. Broderick in his study of the acid base equilibrium of the blood, or in his inquiry into the amount of water drunk or the amount of urine passed by women per diem in order to find the cause of periodontal disease. Nor did he follow Mr. Colin Keay in his fear of so-called traumatic occlusion, as he (Mr. Steadman) thought the more work within reason that healthy teeth had to do, the better. If traumatic occlusion were an important factor, he would expect to find pyorrhœa common in bone-eating animals, whereas the reverse was found to occur.

With regard to treatment, he disagreed with Mr. Turner's teaching with respect to the removal of the six-year-old molars. He agreed with Mr. Pitts here that adequate spacing did not always follow the extraction of these teeth, and he also agreed with him that small spaces led to food impaction and thus predisposed to the disease. He had hesitated to recommend his patients to pass waxed silk between the teeth as he had feared that by damaging the interdental papillæ they would do more harm than good. Would Mr. Turner state whether he had seen damage done in this way? If Mr. Turner was right in recommending that narrow spaces between the teeth should be cut, then it appeared that they should give up making contour fillings. At present he (Mr. Steadman) took great pains in so restoring the lost tooth substance that it was in close contact with the adjacent tooth. He did that with the



idea of preventing food from being forced down between the teeth and injuring the interdental papillæ. He was glad to hear Mr. Turner draw attention to the fundamental importance of well-finished fillings.

When once the disease was established he agreed with Mr. Turner as to the value of isolation of the less affected teeth by the extraction of their neighbours and the free excision of pockets. He had for some years past been using ionic medication and lately the high frequency current in sterilizing the gum margins after excision. He had found this of very great value. He did not quite understand what Mr. Turner meant when he said that this cutting open of the pockets should not be done too early on account of subsequent infection. A further explanation of this point was needed. He believed that he prevented infection of the wounds by ionization. He had found ionic medication of great value in the early treatment of the disease in young adults, and for this purpose he used zinc chloride and copper sulphate alternately.

#### Mr. W. HERN

said that in any discussion on pyorrhœa it was important to remember (1) that the disease occurred in a mucous chamber of the body where the main factors for the growth and multiplication of organisms were present—namely, *heat*, *moisture*, and a *varied and plentiful supply of pabulum*; (2) that it was a chamber which it was difficult to cleanse, possessing as it did two arches of barrel-shaped teeth with interstitial spaces and a surrounding sulcus of gum to each tooth providing colonization foci for these organisms. Taking into consideration such physical conditions and supported by certain observations of cause and effect, as he would mention later, he had long held the view that pyorrhœa was essentially a *local disease* caused by the growth and colonization of organisms. He recognized that general conditions of health might play a secondary part by lowering the vitality of the tissues.

Pyorrhœa commenced as a gingivitis of the gum margins, most commonly on those portions of the mouth where friction, natural or artificial, was absent or was in abeyance and from thence spread to contiguous teeth and gums. This view of local origin was confirmed by the fact that pyorrhœa was found to develop in those positions most free from the disturbing action of the lips, tongue, and food, e.g. (a) under plates, bridges, ill-fitting crowns, projecting edges of stoppings, and under leaning natural teeth; (b) around teeth, especially in the molar regions, which were not antagonized by teeth in the opposite jaw; (c) the more frequent occurrence of the disease on the lingual than on the labial aspects of teeth in patients who used the toothbrush on the buccal surfaces only; (d) the distinctly increased tendency to pyorrhœa and gingivitis found in mouths where, for any cause, the movements of the tongue were inhibited or prevented, e.g. (a) under plates and fixed apparatus of any kind in the mouth; (b) in cancer of the tongue when its brushing effect was abrogated.

Other facts in support of the local origin were the following: (a) Pyorrhœa developed gradually and spread by continuity; (b) there was no clear line of demarcation between gingivitis and pyorrhœa, the one condition merging into the other; (c) pyorrhœa was cured by removal of the cause, viz., the extraction of pocketed teeth and removal of foci of infection; (d) both gingivitis and pyorrhœa were alleviated or cured by treatment directed against the colonization of organisms; (e) in the absence of these organisms, or what Mr. Turner called "dirt," he agreed with him that no general disease would produce

pyorrhœa; (f) he also concurred in his statement that general treatment alone would not cure a case of pyorrhœa, and, it should be added, would not prevent pyorrhœa. He entirely disagreed with some of Mr. Turner's methods of prevention as stated in his opening address: notably, in his advocacy of the extraction of first molars as a routine practice to gain space. He (Mr. Hern) regarded a complete arch with full masticating ability as a great asset in the prevention of pyorrhœa by the marked assistance which the natural friction of the food had on the gums in mastication. For the same reasons he was equally opposed to his method of combating pyorrhœa by the extraction of alternate teeth. Neither could he support Mr. Turner in his advocacy of cutting spaces between teeth to allow of the worsted filament to pass. Such spaces would spoil accurate contours and encourage the catching and resting of food and thus favour the production of gingivitis and subsequent pyorrhœa. He preferred the floss silk, if used intelligently.

If pyorrhœa was a local disease, as affirmed, it followed that it was a preventable disease, and he was convinced that one of our most important duties to patients was to *teach them how to prevent it*. In prevention, physical measures played a far more important part than therapeutic measures. He fully agreed with Sir Harry Baldwin that systematic friction of the toothbrush on both labial and lingual surfaces of the gum margins twice daily was the most effective measure, and would absolutely prevent the onset of pyorrhœa. Massage of the gum was also of much help in hardening gums when gingivitis was present. It was an advantage, too, as focussing the patient's attention in brushing specially on the gums, to suggest some antiseptic to be brushed on to them; for this he preferred a weak solution of peroxide of hydrogen 5 to 10 vols., or common salt.

He (Mr. Hern) had directed his remarks chiefly to the prevention of pyorrhœa instead of to its treatment, because he thought that the most effective attack on the disease was by that method.

An important point in the practice of prevention was to gain the *co-operation of the patient*, and in order to get this it was well to explain the rationale of the treatment to the patient, including as Mr. Turner rightly remarked, "teaching the patient how to clean his teeth." To this end it was a good plan to show the patient, in his own mouth, what was *healthy* gum and what *unhealthy*, also where and how to brush to rectify his defects—the patient should also be instructed as to how to use the floss silk most effectively to cleanse the interstitial contacts and spaces of his teeth.

Although pyorrhœa was one of the most prevalent diseases they were called upon to treat it was in his opinion one of the easiest to prevent in this way with the intelligent co-operation of patients.

#### Mr. D. R. CURNOCK

emphasized the necessity, in his opinion, of radiographing all cases of pyorrhœa and a careful examination and interpretation of the films to see the extent of the disease, and to judge the resistance being put up by the patient's tissues. He considered pyorrhœa an infection, not only of the gingival tissues and the periodontal membranes, but frequently, what was more serious, an infection of the supporting bone. In many cases the infection spread a great distance into the jaw-bones. Frequently the whole of the alveolus and the deeper tissues were involved. On account of the deep infection of the bone Dr. Stanley Colyer suggested the term "Chronic Infection of the Jaws," or "Chronic Osteo-myelitis," which gave a very much better idea of the extent

of the disease than the term "Chronic General Periodontitis." The deep infection of the bones of the jaws was shown on the radiograms as either a porosis of the deeper bone or else a sclerosis. He thought that the indifferent results obtained in many cases of extractions for pyorrhœa in general disease were frequently due to clinically sound teeth being left, in which, however, radiograms showed marked infection. There was too great a risk in leaving these unless the radiograms showed that recuperation was taking place. He thought that with deep bone infection taking place—and the more radiograms taken the more one was struck with the frequency of this condition even in so-called mild early cases of pyorrhœa—in the use of such methods as gum curettage, mouth-washes, frequent scalings, ionic treatment, &c., the further limits of the trouble were not being reached, and that extractions should be undertaken more frequently than they were. He had been struck with the large number of cases which clinically showed little evidence of trouble, in which, however, the radiograms showed a loss of the lamina dura, thickened and irregular periodontal membranes, translucency of the apices and frequently a massive infection of the deeper bone. Unless these cases were radiographed they would be missed. In apparently mild cases it was frequently seen that the infection from the upper incisors was spreading right up to the floor of the nose, and from the posterior teeth along the floor of the antrum which often was a considerable distance from the gingival margins and the roots of the teeth. Patients with such infection present did not always show symptoms, but as they got older their resistance eventually broke down and general medical symptoms supervened. This frequently occurred between the ages of forty-five and fifty-five. Radiograms of jaw-bones taken two years after extractions sometimes showed residual infection present in the bone, which, possibly, might keep up medical symptoms. This being so it would seem wiser, in treating cases of pyorrhœa in which the deep bone was involved, to extract early, while the patient's resistance was good, so as to give the jaw-bones a chance of complete recovery.

Mr. J. G. TURNER (in reply).

To answer my critics, I think the best plan will be to make first a general statement which will answer many of the criticisms, and then to deal individually with the remainder.

Clinical observation shows that "pyorrhœa" is a disease of young life, beginning often in childhood, attacking all sorts and conditions of men provided (with a slight reservation as to mucus) they are fed on sufficiently sticky food. It attacks the vigorous and healthy equally with the debilitated; the perfect denture equally with the imperfect, and is as often absent in the one condition as in the other. It is widely prevalent in some countries among peoples in whom caries of the teeth is comparatively infrequent, but here in England it is the commonplace of practice to find "pyorrhœa" and dental caries in all its forms in the same mouth. In the lower animals it is readily produced by sticky food. In man and the lower animals eating food that is not sticky both "pyorrhœa" and dental caries are absent, as can be seen from Eskimo skulls of 100 years ago and in any museum of wild animal skulls. Sticky food, with us, means the "carbohydrate" foods, but these contain nitrogenous, as well as non-nitrogenous ingredients, and both may serve as nutrient media for germs of pyorrhœa.

Unless these clinical findings can be controverted they are a sufficient

answer to those who advocate the claims of mal-occlusion (or traumatic occlusion) as a cause, and to those who think that some general constitutional defect is the prime factor in the production of "pyorrhœa."

The non-cleansability of the perfect denture is a point deserving careful consideration. The toothbrush touches the least important areas. Mouth-washes are entirely unable to penetrate between the teeth, even under the most vigorous cheek pressure, until destructive pathological processes have opened a way through atrophy of the interdental papillæ. If, before this stage of atrophy, you test a mouth-wash-user's teeth by passing a wisp of rolled cotton wool between the two molars and smelling it as you warm and dry it, you will soon assure yourself that no mouth-wash penetrated. Waxed silk is but a poor cleanser, and where the teeth are so close together that nothing else will pass between them, then the perfect denture is uncleanable.

Only where, as not infrequently happens, the teeth of a perfect denture are spaced enough to allow such a thing as 3-ply worsted to pass between them is there any hope for the perfect denture. A mouth-wash is of little use even in these cases. The infecting germs cling so closely to the tooth that even the rough handling of section-cutting fails to dislodge them, and the universal coating of mucus seals the entrance to the periodontal sulcus. Food-packing appears to be looked on by many as the starting point of "pyorrhœa," and for fear of it they condemn all spacing. But food-packing is not the beginning: it is a result of pyorrhœa. It occurs both in the perfect denture with closely opposed teeth and in the imperfect denture. It occurs in the first case only after destruction of the interdental papillæ by the early processes of pyorrhœa has made gross lodgment possible. In the second case, that of the denture injured by extraction, impacted food is easily removed and so is far less dangerous.

I have advocated extraction of the first permanent molars at from 14-16 years of age as a means of prevention both of pyorrhœa and caries. Mr. Sturridge finds it a pernicious practice, Sir Harry Baldwin says it is a dangerous teaching, Mr. Pitts thinks it unsound and unfortunate. Only Mr. Dowsett has noted that a one-sided extraction of the "6" has left that side sounder in later life than the uninjured side. Tilting, especially of the lower second molar, frightened me for a long time, but when I realized the value of mechanical cleaning and began the use of worsted thread soaked, not in an antiseptic, but in a cleansing fluid, my fears vanished and I have no hesitation in recommending this unsound, dangerous and pernicious practice!

Sir Harry Baldwin and Mr. Pitts object equally to my cutting spaces between the teeth and Mr. Pitts does not believe I ever cut spaces between the teeth so small that while 3-ply worsted will pass, food will not jam. Perhaps Mr. Pitts has not done so, but if he uses discs on the engine I do not wonder; if he will content himself with steel strips and polishing tape he will succeed; and I presume Sir Harry Baldwin will be satisfied when he realizes that food-jamming is not a necessary sequence. Mr. Pitts doubts the movements of the teeth upon which I have relied to produce separation. I can only reply that these movements are observable clinical facts of such frequency as to warrant the formulation of a law of cause and effect.

Mr. Doubleday thinks alternate extraction rests on no sound basis. But his objection seems also to rest on none. Does Mr. Doubleday suggest that opposing 7's will slide on each other's inclined planes till they fail to occlude? As to the clinical application of the idea, it is not often that a case suitable for its full application presents itself; partial application of the idea is

more often possible, and the isolation of one molar in all four quarters of the mouth gives very good results. The most serious trouble is that pyorrhœa is often further advanced than is realized at the time of extraction. Last week, I saw a patient from whom the models here shown were taken. The extractions were done in February, 1922, and her mouth is to-day just the same as the models. She came to me to ask whether it was necessary to have all her teeth out to cure her pyorrhœa. Modified alternate extraction and gum-cutting have had excellent results.

Mr. Doubleday also thinks that no good can be done till we know what germs are at work. This is unfortunate, for if so none of us can be doing any good! And yet a big broom has no knowledge of what kind of dirt it removes.

Mr. Bulleid refers to fuso-spirillary infection, but he seems to me to be dealing with a class of case that is generally referred to as acute ulcerative stomatitis and is not just now in question. I have not yet found such an infection in ordinary long-standing pyorrhœa except in one case examined in the research laboratory of the Royal Dental Hospital of London, in which it was found in the pulp. It would be very interesting if Mr. Bulleid would bring forward his evidence.

Mr. Hobday refers to the dog and the bone. It seems to me quite obvious that no dog, nor man, could clean his teeth by biting a bone—bone and tooth only meet at a tangent. It is what is not eaten that keeps the dog's mouth clean. I do not think pyorrhœa is communicated from dog to man or from man to man as pyorrhœa; but I think that infection takes place immediately or mediately, and that the result depends on the infecting germ and the susceptibility of the recipient. In reply to Mr. Keay, I find the knife gives better results than the cautery, probably for the same reason as elsewhere, that a sharp knife leaves a far smaller area of injured tissue.

With reference to other methods of treatment, I gave up ionization because I found I could get quicker results by "mechanical" tooth-cleaning and leaving the tissues to deal with their own infection. Moreover, I am still unconvinced that the current does not take the surface path of least resistance. Of vaccines for the actual pyorrhœa I have had a surfeit and I shall be quite content to wait and see what others can do with Besredka's method. This, however is certain, it cannot prevent re-accumulation, and the advent of "seven other devils" is only too likely.

Friction of the gums I have not used, because by rubbing the teeth you can prevent pyorrhœa, caries and tartar; whereas gum friction cuts only at pyorrhœa. I have no doubt an extra supply of healthy blood will be beneficial, but I may point out that if Mr. Broderick is right it will only be an extra supply of vitiated fluid, and perhaps more injurious. The line of treatment I advocate is simple in design and rapid in effect, and an intelligent patient can ensure its durability. The same cannot be said for ionic treatment, for vaccine treatment, or for treatment by rectification of that elusive quantity—traumatic occlusion.

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#### ODONTOLOGICAL MUSEUM.

The Curator of the Odontological Museum (Sir Frank Colyer) will always be pleased to receive specimens for the Museum, especially old dental instruments and specimens of human anatomy illustrating age changes.

## Section of Odontology.

President—Mr. DOUGLAS GABELL, L.R.C.P., M.R.C.S., L.D.S.E.

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### The Nerve Supply of the Dentine.

By J. HOWARD MUMMERY, C.B.E., F.R.C.S., L.D.S.Eng.,  
D.Sc.Penn.

WE have recently been told a good many things about the histology of the teeth, which strangely disagree with what we have always considered to be facts.

We have been told that enamel and cement are dead substances; that enamel is in fact a dead, solid, inert, calcified secretion and quite out of the pale of nutrition. Recent investigations have, however, led us to the conclusion that, apart from the great improbability of a dead substance being held in vital connexion with the living body, there are many indications that enamel is a living tissue and probably under the influence of physiological changes. With regard to the nerve supply of the teeth, Professor Hopewell-Smith has publicly made some very emphatic statements which, as they have been so widely circulated, must I think receive a little attention before I proceed to my demonstration of the innervation of the dentine.

In the paper which he read before this Section last year, he said, "this tissue (referring to dentine) is destitute of nerves" [1]. He also said, "dentine is insensitive," although in 1915 he said "its sensitivity is well known."

He has "come to the conclusion that this tissue, being nerveless, cannot, *per se*, be sensitive," but he brings forward no evidence to show it is nerveless, although he states it is capable of "irrefutable proof." He proceeds to found his argument on the premiss that there are no nerves in the dentine.

He says that the nerves of the pulp terminate somewhat similarly to the ordinary sensory nerves in minute endings around the odontoblasts, which, in support of his theory with regard to these cells, he calls fibrilloblasts.

He disposes of my later work in the following somewhat obscurely worded sentence:—

"The myelinic (non-medullated) nerve-fibres in man are cellulipetal. Constituting the peripheral axones of receptive afferent neurones, they are essentially the distal teleodendrites of the peripheral sensory neurones, entering the pulp at the apical foramen of the teeth in company with the blood-vessels and terminating somewhat similarly to the ordinary sensory nerves, not in special anatomical formations as described by Mummery, but in minute endings around the fibrilloblasts on the anatomical threshold of the pulp."

(We fail to understand the term "myelinic" as applied to the neurofibrils, as myelinic is synonymous with medullated.)

It certainly does not appear that we are justified in accepting, or even considering assertions of this kind that are not sustained by any proof. As was

said by a well-known physiologist: "Medicine owes no debt of gratitude to those who teach her theories without proof." And the same statement applies with equal force to physiology and histology.

As I have in several previous communications given a résumé of the literature on the nerve supply of the dentine, I need not again refer to it here except with regard to the paper by Professor Huber, published in America twenty-four years ago, to which I may briefly refer [2], as the views of this author appear to have been almost universally adopted in America and to a considerable extent in our own country.

Professor Huber made use of pulps separated from the dentine which could not necessarily give the relations of the pulp-nerves to the hard tissue. He says:—

"In all the preparations examined by me I was never able to trace any nerve-fibril beyond the odontoblast layer, as one might expect if the nerve-fibres extended into the dentine."

Again, he says:—

"We might expect, I think, that on removing the pulp, some nerve-fibre would be pulled out of the dentine and might be observed extending beyond the odontoblast layer."

He did not consider that in his preparations such fibrils would be retracted into the pulp. However, in many specimens prepared with the dentine and pulp in connexion this is exactly what is observed, as I will now demonstrate (fig. 1).

It is seen that not only do nerve-fibres extend beyond the odontoblast layer but where they have been torn across they are seen to be projecting from the pulp and also retracted to the dentine to which they are attached.

I cannot, therefore, but think that while Professor Huber's work was of interest in the demonstration of the distribution of the nerves which surround the odontoblast cells, it proved nothing with regard to their distribution to the dentine; the evidence was purely negative. In his preparations he could not see any nerve-fibre passing to the dentine and therefore concluded it did not do so. It seems extraordinary that these observations should have been looked upon as the last word to be said on the distribution of the nerves of the pulp. Dr. Noyes, in his "Dental Histology" [3], after referring to my work says: "Support for almost any idea can be found in the literature, but many of the conditions described have been shown to be errors in microscopic interpretation." He then refers to the work of Huber of twenty-four years ago as being the most recent in America, and despite evidence to the contrary in other countries says that "*the nerve-fibres have never been followed into the dentinal tubes,*" and adopts the theory of conductivity of sensation by the dentinal fibril and the odontoblast. Professor Kölliker was more cautious; he said he had traced nerves to the dentine but did not think this represented their ultimate termination.

*The Medullated Nerves of the Pulp.*—In studying the medullated nerves of the pulp I have recently adopted a method suggested by Azoulay for the demonstration of medullated fibres in other tissues.

It is particularly well adapted to the study of the pulp as the nerves and blood-vessels only are stained, appearing black on a clear ground. It is a very easy process to carry out and the sections can be mounted in about half an hour from the commencement of the process.

Longitudinal sections of a tooth pulp showing parallel nerve trunks are placed in a 1 in 500 solution of osmic acid in water for from five to fifteen minutes; they are then rinsed and placed in a 10 per cent. aqueous solution of tannin and heated gently over a spirit lamp until vapour begins to be given off. They are then placed in water for a quarter of an hour—dehydrated and taken from absolute alcohol to camsal balsam or euparal, as with these mountants no clearing oils are required and shrinkage is avoided. By this method of staining it is seen that not all the nerve-fibres of the pulp are medullated, as bundles of unstained fibres may be seen running parallel with the black stained fibres.

The great value of this method of staining lies in the demonstration of the distribution of the medullated nerves to the blood-vessels.

It is seen that the nerve-fibres which separate from the main trunks in the

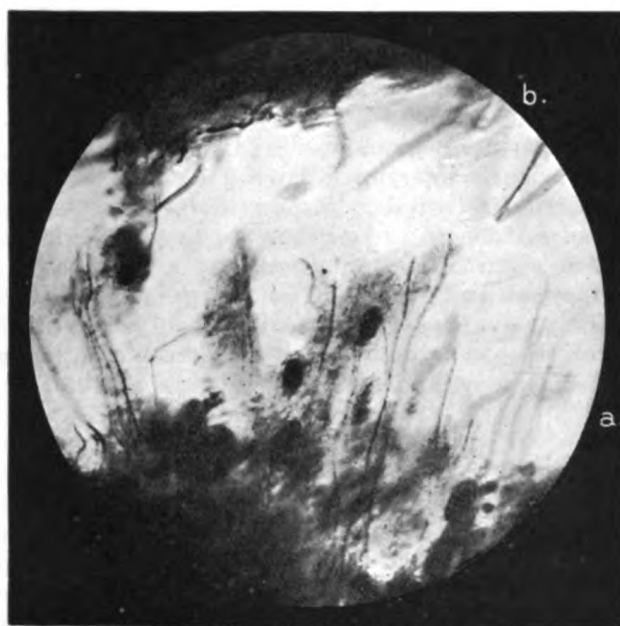


FIG. 1.—Pulp torn away from the dentine showing projection of beaded nerve-fibres from the pulp at *a* and from the dentine at *b* (here out of focus).  $\times 700$ .

centre of the pulp are looped around the blood-vessels and many appear to be distributed to their coats.

As I have described elsewhere [4] very fine divisions of these fibres can be seen in some instances forming a network around the arterioles with nodes at their junctions. These would appear to be the vaso-motor nerves which control the calibre of the small arteries.

That a sensory nerve should contribute motor branches to the arteries appears at first sight to be extraordinary, but Professor Bayliss showed that branches of a sensory nerve contain what he called antidromic fibres acting as vaso-constrictor nerves to the vessels [5].

A true sensory nerve can thus supply both sensory and motor fibres. These fibres are called antidromic, meaning contrary to the normal course of the nerve impulse.



The principal nerve-fibres of the pulp, after giving off numerous branches in their course, are directed to the circumference of the pulp but in greatest abundance to its coronal portion where the area of the dentine to be supplied is greatest.

At their terminations the medullated fibres lose their medullary sheath and later their neurolemma and spread out into innumerable fine fibres, the finer ones forming a delicate plexus beneath the odontoblasts and the larger divisions of the axis cylinders of the medullated nerves passing across this plexus to the dentine. This plexus is named the plexus of Raschkow and is an interlacement of fine nerve-fibres beneath the odontoblasts. It was first described by Raschkow, a surgeon of Silesia, in an inaugural dissertation entitled, "*Meletemata circa Mammalium Dentium Evolutionem*" in 1835.

In this paper he says: "Perhaps no part of the body exists where the extremities of the nerves present so evident and beautiful an appearance as in the dental pulp" . . . "the nerves, after they have severally or in bundles of several branches, entered the pulp of the tooth, recede more or less from each other, and so in simple filaments partly separated and partly combined, proceed to the extreme apex of the crown, where again they divide by degrees into their simplest filaments, which, swelling like rows of beads, constitute the primitive series of variously interrupted nervous articuli, being accompanied in their course by a delicate cellular tissue in order that they may not deviate from a straight line. At length, under the apex they again form a plexus from which filaments are given off, terminating like 'penicilli' at the extremity of the apex, where they are surrounded by a network of vessels. In the incisors of a man about 30 we found a great abundance of filaments in comparison with other parts of the body, and also with the teeth of other animals which we examined. Hence may be deduced the great sensibility of the teeth, especially in man."

Doubt has been thrown by some recent observers upon the existence of this plexus. This has probably arisen from noticing the fact that comparatively large nerve-fibres pass through this area, which do not break up into a plexus, but it is evident in most preparations that they pass across a space occupied by very fine nerve-fibres which are those that surround the odontoblast cells.

These larger fibres form the expansion of the axis cylinders of the medullated fibres of the pulp when they have lost their medullary sheath and neurolemma, and pass to be distributed to the dentine.

On using a connective tissue stain this plexus is not brought into view, but a nerve stain renders it quite evident.

In an experiment which I have previously described, a tooth which had been stained with carmine showed a clear area in this position, but on re-staining with gold chloride it was shown to be occupied by a fine network of fibres.

Under a low power of the microscope I am showing on the table a nerve preparation double stained with Azoulay's nerve stain and iron and tannin. The plexus is very well brought out, and is seen as a dark misty area beneath the odontoblasts. The medullated fibres can also be seen entering and breaking up in the plexus.

Of course this area is also occupied by the fine connective tissue fibres of the pulp, and these can be demonstrated by a special connective tissue stain, as Van Gieson or orcein, but they are not so delicate as these fine fibres of the nerve plexus.

Before describing the preparations further I wish to demonstrate that the

fibres which I shall presently show passing into the dentinal tubes are indisputable nerve-fibres.

First, with regard to their size, it was said the other day that it is extremely difficult to differentiate between nerve-fibres actually entering the dentine and the dentinal fibril. But this is the least difficult part of the problem. In a properly fixed specimen in which the dentinal fibril is not shrunk where the finest nerve-fibres enter the dentine and are seen with the dentinal fibril, they are at least six to eight times smaller.

It could only be with badly fixed and shrunken preparations that any suggestion of confusion could arise even if in such a preparation the nerve-fibre could be demonstrated at all, which is extremely improbable.

Professor Hopewell-Smith in his paper in June last said that "appearances which may be interpreted as nerves are easily produced by following certain methods of microscopic technique." This is certainly not my experience nor do I think it is that of others. I found the preparations I am showing this evening so very difficult to obtain that I do not think there is any other part of the body in which the fine nerve-fibres are so difficult to demonstrate as in the dentine, and this fact was long ago pointed out by Sir Charles Tomes.

In an investigation of this kind many attempts must be made and many failures recorded before definite results can be arrived at.

My first attempts simply led me to the conclusion that if they could be better demonstrated the nerves would be seen distinctly passing into the dentine, but I could only trace them in a few instances, and it was not until after very numerous attempts extending over some years that, finally, I was able to show them as definite continuous fibres passing to the dentine.

*Connective Tissue Fibres.*—It has been emphatically stated by the author I have quoted that the fibres I have shown are not nerve-fibres at all but connective tissue.<sup>1</sup>

It is quite evident that we have at the dentine margin of the pulp not only nerve-fibres but connective tissue in abundance.

So long ago as 1892 I pointed out, in common with Professor von Ebner, that coarse and fine connective tissue fibres from the pulp become incorporated with the forming dentine and that as long as the formation of the dentine proceeds these fine fibres, at the periphery of the pulp, enter into and become incorporated in the matrix [4a]. While von Ebner considered that these fine connective tissue fibres were derived from the odontoblast cells themselves I held that they were the fine connective tissue fibres of the pulp which formed the basis substance of the calcified matrix.

Von Korff, of Kiel, thirteen years later, published a paper in which he claimed that he was the first to show that there is a connective tissue foundation to the dentine [6]. The corkscrew-like fibres of von Korff with the fan-shaped expansion at the dentine margin he only described in a very early stage of development of the dentine when the first evidences of calcific deposit appeared, but sometimes there are found scattered around the margin of the pulp, in young calcified teeth, coarse fibres projecting into the pulp. If a section is cut obliquely at the margin of the pulp chamber the odontoblast layer is not seen but large coarse fibres forming a close interlacement within the dentine and crossing one another for the most part at right angles or nearly so to the surface. These are very large fibres and take a deep red stain with the Van Gieson connective tissue stain.

<sup>1</sup> Although, in a letter to the *British Dental Journal* for May 1, 1924, he appears to have abandoned this view, and now says that the fibres I have shown are not fibres at all, but mere "artefact" deposits of gold or silver.

These different connective tissue fibres form the foundation or matrix fibres of the dentine and are derived from the pulp and according to von Ebner also from the periphery of the odontoblast cell. In addition to forming the organic framework of the dentine their function is to support the blood-vessels and nerves of the pulp.

One of the chief histological characters of connective tissue is that it is "almost devoid of extensibility." "By its inextensibility it maintains in apposition the parts which it connects"—[7] and the necessity of this quality we can quite understand when we consider that it forms the supporting tissue to the various organs of the body.

The nerve-fibres, on the other hand, are capable of very great extension, as is well shown in their connexion with the dentine, as the neurofibrils can be pulled out from the dentine by the retraction of the separated pulp to a very great extent before they give way. Sir William Bayliss, speaking of the extensibility of nerve-fibres, says that the nerve can be stretched, and not only uncoiled, and it was shown by Carlson that the time involved in the conduction of the nerve impulse, when the fibre is stretched, is greater than when it is unstretched.

The finest nerve-fibres also show a beading throughout their course. This beaded appearance is due, as Sir Sharpey-Schafer says, to the fact "that the fibrils are not solid but of a semi-fluid nature."

He says: "That the fibrils are not solid but of a semi-fluid nature is probable from the fact that they easily become varicose, with little beadlets or droplets upon their course; this is what one would expect with a viscous fluid, but not with a solid" [7a].

It is interesting to note the observations of Sir Frederick Mott, who finds no indication of neurofibrils in the living cell [8]. After treatment with fixing reagents these fibres can be demonstrated by a suitable stain. There is some invisible material, probably in the nature of a fluid, which under the influence of reagents appears as a visible fibre.

Hopewell-Smith gives a different explanation of the cause of beading of the neurofibril. He says: "The demonstration of beading is the best way in which one could histologically discriminate between a strand of connective tissue and a fibre of the non-medullated nervous system, by the presence of little gemmules which were merely the nuclei of the neurolemma sheaths of the medullated fibres."

When, however, shown beaded nerve-fibres entering the dentine in the photographs, he would not acknowledge this distinction which he had previously claimed. This beading has certainly no resemblance to a chain of nuclei and could not be so produced, as long before the neurofibril reaches the dentine it has lost its neurolemma sheath.

It is scarcely necessary, therefore, to discuss the statement that the beaded nerve-fibres seen in my nerve-cell preparations are connective tissue fibres either in their physical properties or appearance, and they support this author's first statement that true ultimate nerve fibres are distinguished by their beading and consequently, by his own definition, must be nerve-fibres and nothing else.

It does not follow, however, that nerves are always seen as beaded fibres. The fine nerve-fibres are distinctly beaded, but the larger bundles of neurofibrils, particularly when they are not stretched, in their wavy course to the dentine, do not show any beading, so that this mode of identification of nerve-fibres only applies to their finest divisions.

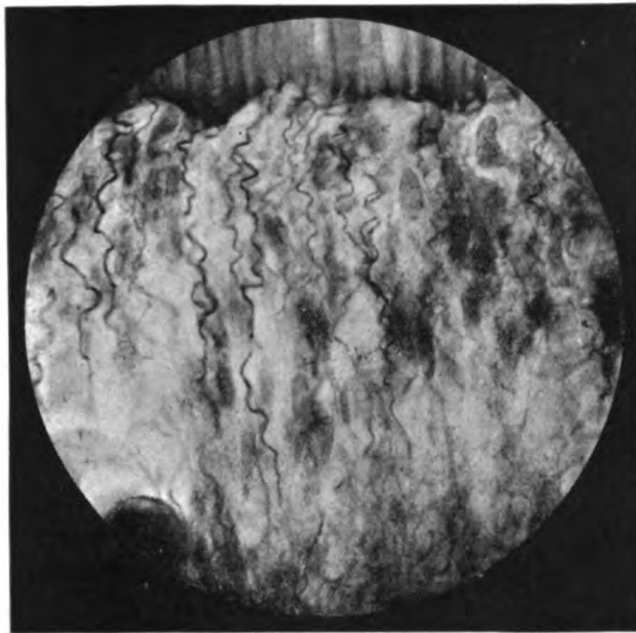


FIG. 2.—Nerve-fibres at a cornu of the pulp which can be traced from the axis cylinders of medullated nerve-fibres in the pulp.  $\times 800$ .

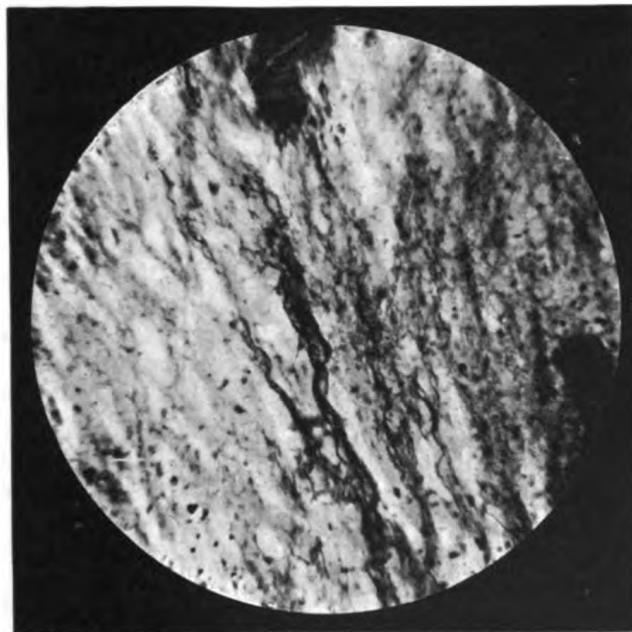


FIG. 3.—Adjoining part of the same preparation from which fig. 2 was taken, showing part of the bundle of medullated nerves from which the fibres in fig. 2 arise.  $\times 400$ .

To sum up the distinctions between connective tissue and nerve-fibres :—

- (1) Connective tissue fibres are practically inextensible.
- (2) Nerve-fibres are highly extensible.
- (3) The ultimate nerve-fibres are beaded.
- (4) The finest connective tissue fibres are not.
- (5) Nerve-fibres do not take selective connective tissue stains.
- (6) *The nerve-fibres in these preparations are continuous with the axis cylinders of medullated nerves.*

We will now proceed to describe the preparations and to show the continuity of the fibres passing to the dentine with the axis cylinders of the medullated nerves. In this slide, photographed from the preparation under the microscope on the table, one cornu of the pulp is shown of a premolar tooth with unfinished root. Several medullated fibres of the pulp are seen at the lower part of the preparation, which can be seen to be breaking up into their constituent fibres and passing to the dentine in a radiating manner. These medullated fibres can be traced far back into the pulp and form a portion of the main nerve-trunks which pass to the crown portion of the pulp without dividing (figs. 2 and 3, p. 41).

This specimen should I think entirely dispose of the statement that these fibres are not nerve-fibres but connective tissue. This special slide was chosen by Sir Sharpey-Schafer for illustration in his "Essentials of Histology," to demonstrate the innervation of the dentine. These larger fibres do not appear to be seen at the lateral margins of the pulp but only where the main medullated trunks are being distributed at the apex. It is a curious fact that these relatively very large nerve-fibres are not seen in preparations treated with ordinary stains, they are transparent and invisible, but when treated with a suitable nerve stain such as chloride of gold well reduced in their substance they are very conspicuous. I have succeeded in staining them both with the Beckwith process and by the formic acid process of Viallane.

It is seen that the nerve-fibres when not pulled upon by any separation of the pulp from the dentine have an undulating course but in other preparations where traction has been exerted upon them by the partially separated pulp they are drawn out into straight lines and many are apparently under strong tension.

This is an important preparation, as it so clearly proves that these fibres passing to the dentine are the elements of the axis cylinders of the medullated nerves of the pulp. Individual fibres can be distinctly traced under the microscope from medullated nerve-fibre to the dentine.

In the following slides I will show that these spreading fibres can be seen not only to enter the dentine but to pass up the tubules. Identical fibres are here shown in the coronal part of the tooth, but as they are shown in a transverse section their connexion with the medullated fibres cannot be here seen (fig. 4) as the main nerve trunks from which they arise have been cut across; but it can be seen that they come from a considerable depth in the pulp where the medullated fibres break up before entering the plexus of Raschkow. The fibres can be seen entering the tubes and their course within them can be distinctly followed. It is only now and then that a preparation is obtained which shows the nerve-fibres within the tubes at the margin of the pulp, as in most instances when the gold stain is sufficiently reduced to show the nerve-fibres effectually in the dentine, the latter has taken on such a deep stain that they are hidden, but in the preparations from which these photographs were taken the dentine remains clear and shows them very perfectly.

They can also be seen more deeply in the dentine but owing to the curvatures of the tubes it is difficult to keep these fine fibres in focus for photographic purposes. By making use of a long extension of the microscope with a  $\frac{1}{2}$  oil immersion lens and a high eye-piece I was able to make a camera lucida drawing of the fine divisions of the nerves in the tubes in a single field of the microscope, magnified 2,000 diameters. The appearance of branching of the nerve-fibres is probably due to the separation of strands of neurofibrils in the main tubes giving off divisions to the finer branches, and shows us that the nerves are distributed to all parts of the dentine.

Other specimens were treated with an anilin stain in order to ascertain if nerve-fibres could be demonstrated by other than metallic impregnations.

The method adopted was the anilin blue and safranin method of Stroebe. Sections treated with a concentrated aqueous solution of anilin blue are

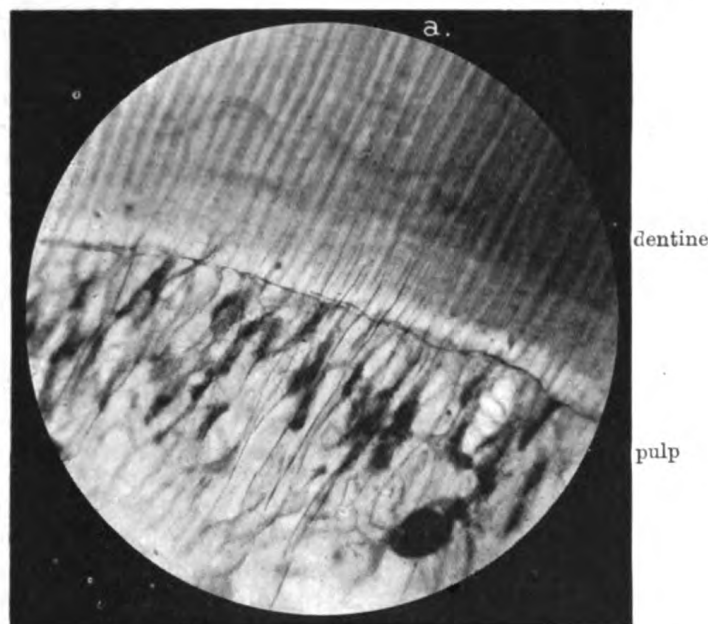


FIG. 4.—From a transverse section near the cornua of the pulp, showing the nerve-fibres entering and traversing the dentinal tubes. Opposite *a* they can be seen deeply in the tubes.  $\times 400$ .

decolorized until transparent in acid alcohol and then stained in safranin, cleared in alcohol and mounted. In successful specimens the nerve-fibres retain the anilin blue stain.

In these specimens, one of which is under a microscope on the table, the very delicate nerve-fibres are seen stretched across the interval between the partially separated pulp and the dentine and being under tension are drawn out in straight lines. With the anilin stain these fibres are much less deeply coloured than with metallic stains. The specimen under the microscope shows these fibres at the lateral margin of the pulp slightly below the neck of the tooth.

Having thus, I hope, satisfactorily demonstrated that the dentine is supplied with nerves, and this in great abundance, it remains to describe

certain peculiar conditions in the mode of distribution of these nerves within the pulp.

I shall confine myself to a description of the histological appearances, and not on this occasion consider any theory or theories which might be deduced from them.

*The Nerve End-cells.*—These were first found in the pulp in 1918, and described in a paper contributed to the Royal Society, which was published in 1920.

Having met with great difficulty in obtaining uniform results by the gold impregnation of sections, which were often much disfigured by deposits, it was decided to try staining in bulk.

Small pieces of teeth, decalcified in formic acid, were suspended by threads in a weak solution of gold chloride (1 in 5,000) and were kept in this solution in the dark for from two to three weeks. They were then removed, rinsed with water and reduced in the light with carbonate of potash and potassium iodide, embedded in gum and cut on the freezing microtome.

The gold was in different stages of reduction throughout the thickness of the specimens, but some dozen sections in each piece were reduced to a deep purple colour, evidence of the complete reduction of the gold. That this is complete is further shown by the fact that after six years the preparations show no signs of darkening or other changes so characteristic of gold preparations.

The delicacy of this process is shown by the misfortunes which attend neglect of any detail. A large batch of specimens was lately spoilt owing to the gold solution not having been properly acidulated. In an alkaline solution impregnation does not appear to take place.

In the fully reduced sections and in them alone a row of cells came into view arranged at fairly regular intervals along the lower margin of the layer of odontoblast cells (fig. 5). The other specimens from the same pieces, although many showed the nerve-fibres very well did not reveal the cells, and it seems that complete reduction is necessary to bring them into view.

I had first thought that this appearance of cells was deceptive and due to an interlacing of the larger fibres passing to the dentine, but where the sections were sufficiently thin as in the one I am showing under the microscope it is quite evident that they are cells from the distal extremity of which a single beaded nerve-fibre, or axon, passes to the dentinal tubes (fig. 6). From these same cells lateral branches or dendrons pass to surround the odontoblast cells. In several of them synaptic processes can be seen forming communication with the fibres of the deeper plexus.

I will now show upon the screen the drawings and photographs illustrating this part of the subject.

The processes which pass towards the plexus beneath the odontoblasts are here seen to terminate in synaptic endings and lateral processes pass to the odontoblast region on either side. These are the nerve-fibres which are plainly seen in many specimens to arborize around the odontoblast cells and small nodes or enlargements such as are described by Huber and others may be detected in many places. Some of these are in the form of minute oval rings as described by Deppendorf in his paper in 1913.

It would appear that these nerve-cells are arranged in definite rows but there are intervals between the little groups of cells in all directions, consequently some sections have not passed parallel to the cell groups and only a few of them are seen, the appearances in such cases being like those I

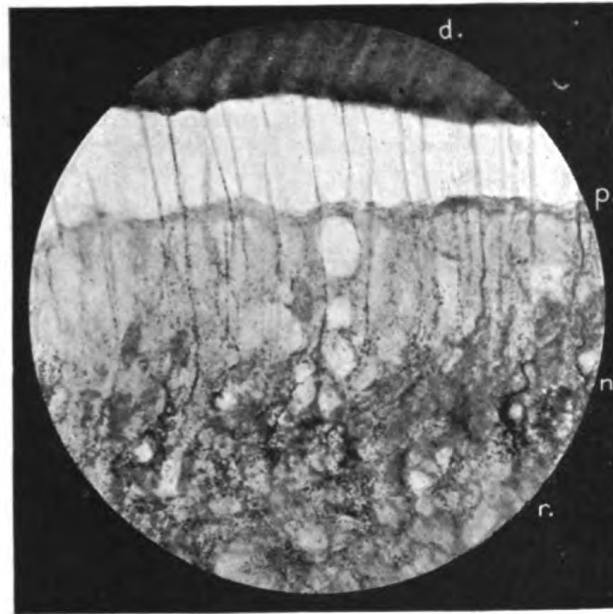


FIG. 5.—Four of the larger nerve end-cells. Beaded fibres passing from them to the dentine. Some unstained stretched dentinal fibrils can also be seen. *d*, dentine; *p*, pulp; *n*, nerve-cells; *r*, plexus of Raschkow.  $\times 700$ .

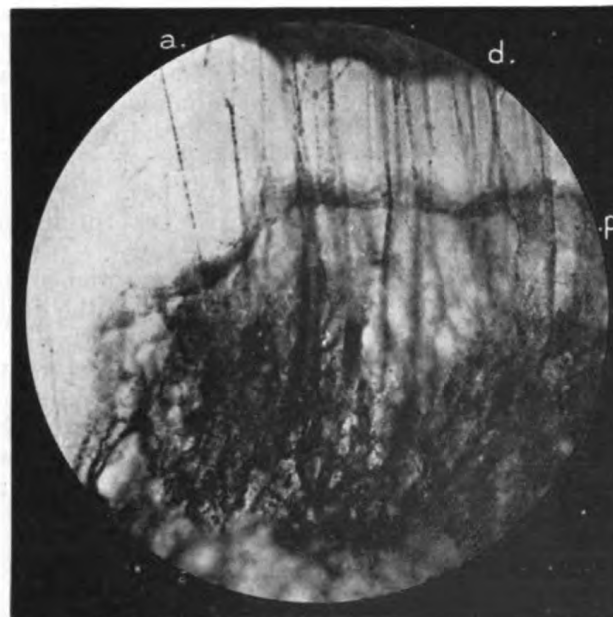


FIG. 6.—Extremity of same preparation shown in fig. 5. Deeply printed to show beading and the unstained dentinal fibril. The fibre at *a* is under high tension. *d*, dentine; *p*, pulp.  $\times 850$ .



described in the former preparations where they appeared as apparent thickenings of the fibre bundles.

The fibre on the extreme left of one of these photographs is seen to be drawn out from the dentine to a very great extent and stretches across the interval evidently under very considerable tension (fig 6). It is a typical beaded nerve-fibre and thus shows very clearly its great extensibility, in fact it was so tightly stretched that it very soon gave way and with others in the same preparation recoiled in a curved or twisted loop on either side.

I do not pretend for a moment that I am not very much puzzled by the intervention of these cells but there can be no doubt about the fact, the preparations, as I think will be acknowledged, are quite diagrammatic in their clearness. So far I have not been able to demonstrate them in longitudinal sections but I hope to do so as this may probably tend to clear up the question as to the actual distribution of these cells. This distribution by the intervention of cells is to a certain extent a distinct subject and does not affect the demonstration that nerve-fibres enter and supply the dentine.

There seems to have been some confusion in the minds of some authorities on these subjects, and the text-books finding a difficulty in fully understanding the exact relation of the cells to the distribution are unwilling to accept the conclusive proofs of the fact that there is a definite nerve supply to the dentine.

One text-book says the question is still *sub judice*, although the much less certain demonstration of the existence of lymphatic vessels in the pulp is fully acknowledged.

Another speaks of two *theories* regarding this question, one, that the fibril and odontoblast cell is the channel of sensation, the other that nerves pass into the dentine, but both are agreed in refusing to acknowledge its innervation. The latter cannot however be classed as a theory; it is capable of demonstration, as is the nerve supply of other organs and tissues of the body. It is a fact in anatomy, and in the presence of this fact there is no room for the highly improbable theory that a connective tissue-cell and its process serves as the path of conduction of nerve impulses from the dentine.

It is acknowledged by all the eminent authorities in physiology and histology to whom these preparations have been submitted that they represent true nerve elements and they have been demonstrated as such by Professor Halliburton, both at the Royal Institution and the British Association.

It is only in our own profession that cautious scepticism has been carried rather to an extreme and writers on the histology of the dentine have either cautiously avoided the subject of the innervation of dentine or treated it as an unproved and unconfirmed theory, although both Deppendorf and Fritsch have confirmed it since the publication of my first paper in 1912.

In conclusion, I hope I have proved to your satisfaction that the dentine is supplied with nerves. I feel that further research is necessary with regard to the cells, but the main fact of the innervation of dentine is, I hope, fully established.

In every research of this description there is no finality, there is always something more to be investigated; something further to be discovered.

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## DISCUSSION.

Mr. GABELL (President) said that he hoped Members realized the difficulty of demonstrating to a large meeting the course of fine nerve-fibrils. High power photomicrographs only showed a very thin layer of tissue and so unless the fibril lay flat in that plane its continuity could not be demonstrated. The setting up of specimens under high powers on strange microscopes and strange illuminants was also difficult to accomplish quickly. Personally he had had the advantage of viewing these specimens on several occasions under comfortable conditions, with time to use the fine adjustment and so follow the continuity of the fibrils to a far greater extent than any photograph could show, and he assured the meeting that Mr. Mummery's drawings were true and accurate representations of this more complete view, were indeed better records of what the actual specimens showed than the photomicrographs. He had been able to trace nerve-fibrils from nerve-trunks into the dentine. He had no doubt of the innervation of the dentine. The various methods of staining absolutely excluded the possibility of confusion with connective tissue. They were nerve-fibrils that entered the dentine and lay on the much larger dentinal fibril. They were deeply indebted to Mr. Mummery for the skill and perseverance with which he had, for many years, followed up this difficult work to a definite proof.

Mr. MONTAGU HOPSON said that it had been his privilege for many years past to have had the opportunity of carefully examining the actual sections that Mr. Mummery had prepared in his investigations as to the distribution of the nerves of the pulp and their ultimate terminations. He desired to say as emphatically as possible that he was absolutely convinced that the nerves of the pulp passed into the dentinal tubes. It was a somewhat curious fact that those who criticized the findings of the reader of the paper had themselves never examined the sections. He personally had no claims to be regarded as an expert practical histologist, nor would he presume to pit his opinion against that of Sir Edward Schafer, Sir Frederick Mott and Professor Starling, yet he ventured to think that he had had sufficient experience to know what he was looking at.

Mr. NORMAN BENNETT said he thought that the evidence of nerves entering the dentine provided by Mr. Mummery, especially in the slides which they had examined under the microscope, was conclusive. He considered that it was absurd to suppose that Mr. Mummery and other distinguished histologists who accepted his views, could not differentiate between nerve filaments and dentinal fibrils, connective tissue, or other structures.

Dr. A. LIVINGSTON said that he also wished to express his admiration for Mr. Howard Mummery's work, which had raised the question of the innervation of dentine above the region of disputable theory into that of known fact. He wished to lay stress on one side of Mr. Mummery's work to which reference had not been made, namely, the very beautiful drawings and photographs, which themselves exhibited extraordinary skill, and represented the last process of many processes requiring continuous patient endeavour.

## Hæmorrhage following Tooth Extraction.

By W. WARWICK JAMES, F.R.C.S., L.D.S.Eng.

THE object of this communication upon hæmorrhage following tooth extraction is to raise a protest against the common method of plugging the socket. It is time the procedure adopted for hæmorrhage in general surgery should be applied to a similar condition following the extraction of a tooth.

[February 25, 1924.]

## 48 Warwick James: *Hæmorrhage following Tooth Extraction*

The usual description of the condition given by the patient conveys the impression of very great discomfort and anxiety. The bleeding had "lasted all night," and in spite of keeping quiet, by lying down, and making applications of cold water, or styptics of different kinds, large clots of blood formed in the mouth, and blood and saliva (the latter increased in quantity) continued to flow from the mouth, soaking many handkerchiefs, the number at times being given, or a volumetric measurement of the amount of blood presumably lost (e.g., "at least a pint") being stated. The doctor had been sent for, who finding further local applications, such as adrenalin and ice useless, feels compelled to adopt the classical method of treatment by "plugging the socket," a procedure he delays for as long as possible on account of the great pain and discomfort caused. At first it not infrequently fails, and the operator, thinking perhaps it is due to tender-heartedness on his part, or to the want of courage on that of the patient, makes a more heroic attempt.

In due course the hæmorrhage is arrested, usually with considerable injury to the parts involved; more often than not a varying degree of necrosis results, the soft tissues become swollen, and there is practically always some sloughing. Such a picture may be regarded as an exaggeration, but from a considerable experience I venture to state that it is not uncommon.

In considering the question of treatment, it is necessary to determine what are the causes of the hæmorrhage, how it can be prevented, and if occurring how most easily arrested. It practically never occurs, if a tooth be extracted, without laceration to the soft parts or fracture of the alveolus. The first principle, therefore, is to extract the tooth with as little damage to the tissues as is possible. An error, I believe, has been committed in teaching students, by emphasizing an outward movement during extraction, which frequently leads to fracture of the outer alveolar plate. It would be more correct to urge a loosening of the tooth and then withdrawing it in its long axis. If, however, the character of the tooth is such that this method is impossible and the alveolus is broken, the question of possible hæmorrhage should immediately occur to the dentist. Should the hæmorrhage take place the general surgical principle laid down of determining where the bleeding point is situated applies equally in the case of a tooth socket. The hæmorrhage will be found in practically every case to be situated in the muco-periosteum, which has been torn from the bone (the margin of the socket usually having been fractured), but never from the socket itself.

For those rare cases where the hæmorrhage comes from the bone, a large part of the socket has been removed in the effort to extract the tooth. Even in cases where so severe an operation has been necessary, it is improbable that a vessel of an appreciable size has been damaged.

During the removal of some inches of bone from the iliac crest for bone-grafts, hæmorrhage was at times persistent, but was overcome without difficulty by placing a blunt-ended instrument over the bleeding point, and giving a gentle tap with a hammer, but such hæmorrhage implies an operation scarcely within the category of ordinary tooth extraction.

The procedure for treatment in cases of hæmorrhage should be as simple as possible, and great care taken to avoid injury of the parts. The muco-periosteum should be turned back gently and any loose fragments of bone removed. Any difficulty in determining the point from which the blood is coming may be overcome by applying lateral pressure to the soft tissues of the socket, first to one side and then to the other, sterile gauze being used for the purpose. When a fragment of alveolus has been removed, and pressure applied in this manner,

the hæmorrhage in most cases will be completely arrested. Should it recur on removal of the pressure, a roll of gauze upon which the patient can close the jaw is often effective, but the pad must be carefully applied so that pressure is brought to bear directly, or by tension on the gum, upon the bleeding vessel. Direct pressure is applied particularly to hæmorrhage from the quite large vessels situated in the muco-periosteum around a third mandibular molar—a tissue which is apt to be torn from the bone during the removal of this tooth. With other teeth the bleeding point is usually away from the upper part of the socket, when the muco-periosteum must be carried over the bone when applying the gauze. Should this procedure fail, one stitch through the gum pulling the soft tissues together across the socket will usually succeed, but if there is any doubt, greater security may be assured by a stitch commencing on the side which is not bleeding, then taken through the bleeding side, and back again through the same side. The vessel may be included in the loop, but when this is impossible the tension will usually suffice, and then back through the original flap.

Ligature of the bleeding vessel may be undertaken, but is difficult to carry out as one is apt to increase the tearing of the flap. If the hæmorrhage is upon a free surface, there is little difficulty in arresting it, but when it is situated more deeply, there is considerable difficulty in applying artery forceps. If, however, the vessel needs the direct application of a ligature, this is best carried out by including it in the loop of the ligature introduced with a curved needle.

Should the simpler methods fail, it is undoubtedly advisable to make a local injection of novocaine, with or without adrenalin, in order to render it easy to carry out the treatment, and to avoid injury of the parts as much as is possible. If adrenalin is used it is necessary to remember that when the action of the drug passes off, the hæmorrhage may recommence.

It has not been my intention to deal with those cases in which the clotting of the blood is imperfect—cases which, from records, would appear to be of greater frequency after tooth extraction than from injury to other parts of the body. How often do they occur during general surgical procedure? I would venture to state that they will be found to be of far less frequency than is generally implied if the hæmorrhage is dealt with in a surgical manner.

#### SUMMARY OF THE TREATMENT.

The following are the main points :—

- (1) Removal of fragments of fractured alveolus.
- (2) Lateral compression of the muco-periosteum, which can be continued by the patient.
- (3) Compression between the jaws of a pad applied over or making tension upon the vessel.
- (4) Tension derived from stitching the flaps of muco-periosteum over socket.
- (5) Ligature or stitch at actual point of hæmorrhage.

But I would add that the most important point of all is that during extraction of a tooth no laceration of soft parts or fracture of alveolus should occur if it can possibly be avoided.

## 50 Warwick James: *Hæmorrhage following Tooth Extraction*

Mr. F. COLEMAN said he was glad to have the opportunity of participating with Mr. Warwick James in his protest against the method of arresting hæmorrhage by the plugging of the tooth socket. During the last twenty years he had but rarely employed this method of treatment and had on many occasions condemned the principle, notably in the first edition of his book on "Extraction of Teeth" (1908) and later in the *Annual Reports of the Royal Dental Hospital*, 1912, in a paper entitled "The use and abuse of gauze plugging in the mouth."

## Section of Odontology.

President — Mr. DOUGLAS GABELL, L.R.C.P.Lond., M.R.C.S.,  
L.D.S.Eng.

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### A Compound Composite Odontome.

By E. W. FISH, M.B., Ch.B.Manch., L.D.S.

THE case which I have to record is one of compound composite odontome. It occurred in a girl of 12 years of age.

It was first noticed, as a swelling on the labial side of the mandible in the position of the unerupted permanent right lateral incisor, by Mr. Creemer Cooper, who had the condition radiographed and recognized as a composite odontome. Mr. Howell Evans was consulted and advised excision.

To suit the convenience of the patient's family, who live some distance from London, the case was referred to Mr. Gerald Sichel and myself.

On examination the two central and left lateral incisors, left permanent canine, both right premolars, and right temporary canine were found *in situ* in the jaw and fully erupted. There was a small rounded swelling below the root of the right temporary canine which gave no pain and was not inflamed.

Between the roots of the right premolars on the labial side of the jaw, there was another swelling, caused by the misplaced and unerupted right canine.

The radiograph showed a tumour of radio-opaque tissue, which might have been one or more distinct masses, but having a definite wall surrounding the whole tumour. The growth did not involve the neighbouring teeth but was placed immediately below, and in contact with the absorbing root of the temporary canine. The permanent right lateral incisor was missing and the odontome appeared in the place that this tooth should have occupied.

It was decided to remove the odontome. This was effected by means of a horizontal incision through the muco-periosteum over the swelling when, after chiselling away the outer alveolar plate and extracting the temporary canine, eighteen denticles were removed from the cavity. Some came away easily, others with considerable difficulty. The largest lay at the bottom of the cavity and was extremely difficult to dislodge owing to its irregular shape. The

incision was extended and the unerupted permanent canine removed, the roots of the right first premolar and right central incisor were not disturbed, and upon complete removal of the denticles a bare, smooth, rounded cavity was left in the bone, some 20 mm. in diameter. The wound healed by the organization of blood clot. One suture was inserted but no packing. The patient recovered within a week.

The denticles were eighteen in number, varied in size from 12 by 10 mm. to 2 by 1 mm. and fall roughly into three groups :—

(1) Simple peg-shaped teeth with enamel tips, which appear macroscopically to have dentine, cementum, a periodontal membrane and root canal. In one the root canal is not yet closed.



Compound Composite Odontome.

(2) Fused masses of similar peg-shaped denticles showing, where fracture has occurred, a central root canal in individual members.

(3) Twisted, stunted denticles still consisting of enamel and apparently dentine and cementum with fibrous tissue attached. Sections of one of the denticles were kindly prepared by Mr. Howard Mummery, who reports the presence of cementum.

The case is of interest from the obscurity which surrounds the origin, or rather causation, of such growths, and on account of their extreme rarity.

Cases are recorded by : Tellander [1], Mathias [2], Windle and Humphreys [3], Ward-Cousins [4], Bland-Sutton [5], and Harborow [6], which, with one or two others, complete the series.

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**A Compound Composite Odontome.**

By G. J. HARBOROW.

I REMOVED this odontome from a woman, aged 26. It was situated in the region of the roots of the right maxillary canine and lateral incisor and judging by the X-ray appearances seemed to be between them.



FIG. 1.



FIG. 2.

The patient had a small osteoma on the same side, above the premolar, and this gave a prominence to the cheek which I mistook at first for a swelling caused by the odontome. Further examination proved it to have no direct connexion with the dental tumour, which was impossible to diagnose without X-rays; there being no external evidence whatever of a growth.

The patient presented herself for treatment on account of a persistent neuralgia in the canine region, and examination failing to reveal a cause she was sent for X-ray examination. The result showed a small compound composite odontome apparently between the roots of the right canine and lateral



incisor (fig. 1). Under a 4 per cent. novocaine application, I made a curved incision over the roots of these teeth and reflected the muco-periosteum. I then chiselled away the alveolus to expose the area between the roots where the tumour appeared to be seen in the X-rays. I could find nothing, so I extracted both teeth, hoping that the odontome would be then easily seen. I was again disappointed and had to remove the outer alveolus wall and chisel away the posterior wall of the sockets before I came upon the growth.

The two denticles were found as seen in the X-rays (fig. 2). No capsule was present and the denticles, which were firmly embedded in the bone, were removed by a straight elevator.

## Section of Odontology.

President—Mr. DOUGLAS GABELL, L.R.C.P., M.R.C.S., L.D.S.E.

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### The Clinical Aspect of the Care of Children's Teeth.

By GEORGE NORTHCROFT, O.B.E., L.D.S.Eng.

NEVER having been associated with School Dentistry, Children's Clinics or Welfare Centres, it may seem somewhat bold on my part to give you the outcome of thirty years' experience of the care of the teeth of children, for even in that time it has not been possible to follow up the treatment of as large a number of cases as many of my colleagues. It is felt that nothing very new can be said, yet institutional treatment necessarily differs in some respects from private practice, and lessons may be learnt from the latter which can seldom be acquired from the former.

Upon looking over the recent literature on this subject one is struck by the divergent opinions still expressed as to choice of methods, filling materials to be used, and the general principles underlying all.

It is understood that the clinical aspect of the care of children's teeth implies a study of the best procedure to adopt at the chairside in order to conserve dentally in the most durable way the present and future health of the growing organism.

It has been said recently that function is to be regarded as one of the most important factors, perhaps the most important factor, in development, therefore the teeth of the growing child must be kept in the highest state of efficiency. W. H. Rushton's and M. Hopson's congenitally edentulous cases might seem to contradict this statement, and also J. G. Turner's playful and well known attitude as to the value of teeth in general; but as the ages of the first mentioned cases were fourteen and seventeen respectively, one does not know, may never know, their subsequent life history, and the peculiar method of feeding of adults of a primitive African tribe may have been indifferently observed, and of their children not at all, and compensating aids to digestion also may be adopted in this last case of which we know nothing.

To maintain a high state of efficiency of the teeth must be the dental surgeon's first duty. When the maximum efficiency is not maintained, or does not exist, it must be restored, or created, or a result obtained as near this ideal as possible. As a clinician the dental surgeon can do these things in every one of three ways, by Prevention, Restoration, Regulation.

#### FIRST: PREVENTION.

It must not be thought that propaganda in oral hygiene is outside the province of a clinician's work. "Prevention is better than cure," and to prevent disease is surely the surgeon's noblest aim, and is the most pleasant part of the intelligent treatment of disease.

It is a mistake to think that temporary teeth must be regarded as ephemeral

elements of little utility whose inevitable disappearance taking place sooner or later renders them encumbrances, and which must never have any time spent on them, except in case of dire necessity.

The "compleat" dental surgeon must be an adept at tactful instruction, the education of the lay mind is a labour of love for the most part, but should not be shirked on that account, and knowledge is easiest imparted and gained at the chairside, where so often practical illustrations can be given of precepts to be observed or dangers to be avoided.

Most of us still believe that caries is primarily caused by fermentation, following the stagnation of carbohydrates; therefore underline the principles which must be followed to prevent this stagnation.

Discuss the point with mothers and nurses before the young patient. In other words, preach the wholesome and simple truths contained in the various essays of Sim Wallace, which are the logical outcome of accepting the stagnation theory. The danger of the sweet habit, of soft and sticky foodstuffs taken the last thing at night, should be pointed out, and emphasized as being especially disastrous to mouth-breathers.

The possibility that calcium metabolism may play a part in the degeneration of tooth structure after eruption must not be lost sight of, and at any rate during the tooth-developing period any deficiency in diet which causes a diminution of lime salts should be avoided, and the value of a diet with full vitamin content explained.

According to some authorities the use of that doubtful help to oral hygiene, the inevitably septic toothbrush, if recommended, should be hedged round with all sorts of precautions, such as its shape and size, its sterilization, the use of two up to seven alternately, the brush to be used first dry, then wet, the importance of its use at night emphasized, the correct down-and-up method of brushing, and the minimum use of paste or powder with the maximum of time and power insisted on. Dental writers have even invoked the muse to encourage the young in the "toothbrush habit." After an intimate and careful study of the human boy, one remarks that the "toothbrush habit" is generally notorious by its absence. But as children must feed, and have this habit born with them, a properly regulated diet seems to secure more naturally a hygienic state, and has certainly been attended in many instances with most gratifying results.

Children should certainly be taught to rinse their mouths vigorously, thereby promoting fully functional buccinators, which when active certainly help to lessen stagnation. To advocate the use of dental floss silk at an early age is of very doubtful utility.

One point is quite clear, that in all preventive measures intelligent co-operation between parents, teachers and dental surgeons is most essential. It is no use for the surgeon to preach if the parents and teachers do not see that the practice is carried out; it is equally disastrous for a child to leave a home where oral hygiene is daily practised and have the evil fortune of going to a school where it is totally misinterpreted or even despised.

A discussion on the virtues of natural feeding as against the use of a bottle, on the general development of the teeth and jaws and the future welfare of the offspring, together with the danger of oral sepsis, especially to the expectant mother, should be included in all talks that have prevention for their aim.

One side of prevention that appeals to some child specialists on the other side of the water is that of "prophylactic treatment" once a month; it is

perhaps right to charge people for what they ought to do, but will not do for themselves, but a hazel switch or a little lettuce and water seem a cheaper and on the whole as satisfactory a plan.

One preventive measure to limit the area of disease that all dental surgeons endorse, differing only as to the age of commencement (some writers give the age for first examination as early as the appearance of the first temporary molars) is the value of periodical visits at least three times a year for the growing child, so that potential exposures may be prevented and recurrence of decay arrested with the minimum of fatigue, pain and danger.

The above presupposes willing and intelligent parents, who are ready to submit to and follow out definite instructions from experts.

But there are children whose mouths, unhappily, have been neglected, who, when brought for treatment, already may have experienced pain from one or more of several cavities. This brings us to the second of the three methods by which a dental surgeon tries to restore a mouth to its maximum efficiency.

#### SECOND : RESTORATION.

This is carried out first by the removal of caries and then replacing the lost tissue. Or, secondly, by re-creating biting surfaces where these have been lost through enforced early extractions.

It has been seen that one and a half to two years is not too young to first examine a child, and if dental surgeons had been in the habit of examining more children at this age a good deal more would be known to-day about the dates of eruption of the temporary teeth and deviations from the normal as to both times of eruption and position.

It must be acknowledged, however, that where properly given instructions to young mothers have been followed out, three is quite early enough ; some children even at this age resent having to be examined.

A. T. Pitts pleads for an earlier age for the treatment of school children, based on the correct physiological age of eruption of the first permanent molars, and suggests that five is a far sounder age than six.

Due regard for a rough and ready knowledge of psychology should not be forgotten. A child (where at all possible) should be introduced gradually into the environment of dentistry, first accompanying a parent or elder brother or sister just to have a look round. It must always be remembered that dread of the unknown makes as painful an impression on a sensitive mind as pain itself. Advise parents as to the foolishness of extravagant suggestion ; it is not pleasant to hear oneself called "the horrid dentist," or the engine spoken of as "that dreadful, buzzy thing." Where the reverse preliminary educational course has been adopted it is surprising to find how amenable little patients are, and if their confidence is not abused how amenable to treatment they remain.

Very young children have not yet experienced the benefit of school discipline, and when coming from an over-indulgent home may be in bad need of the first simple lessons of obedience. An American suggestion that the use of celluloid ducks floating in our fountain spittoons is a desirable object lesson to divert the young mind from nervous prostration when in the dental chair, is evidence of unbalanced thought, and surely might evoke the Homeric laughter of the gods. A. T. Rogers' advice is in contrast to these methods and teaches the avoidance of bribery, the cultivation of courage and the obliteration of maladroitness.

Crying increases the blood-pressure and should be avoided, if possible, when any inflammatory condition is present.

Never work long at a time for young patients whose resistance is easily lowered, and who are generally at their best in the middle hours of the day.

Ulcerative stomatitis is not often met with in private practice, but when present Talbot's solution of zinc iodide has been found very efficacious for treatment. Aphthous ulcers more frequently occur, and 10 per cent. nitrate of silver is quite strong enough and easily applied on a small pledget of cotton wool; pure carbolic is also useful on account of its analgesic property. Thrush and other inflammatory conditions of the mucous membrane are more often first seen and treated by the family physician.

To maintain the efficiency of the teeth and jaws at a maximum it is necessary to see that all teeth should be made fully functional and remembering that temporary teeth have a broad contact surface, restorations should be fully contoured to prevent the packing of food and to avoid tenderness when biting, so that mastication can be carried on at the full muscular power of the individual.

In preparing cavities for the very young use the engine as little as possible, and see that the handpiece is running smoothly and that the burs are sharp and small enough to prevent chatter. Cavities in temporary teeth with unexposed pulps are not very sensitive and it is noise rather than pain that causes alarm. In such cavities it is quite unnecessary completely to cut away overhanging edges of enamel, as they will be found to increase retention considerably. Sharp spoon excavators, especially the type with the spoon bent at right angles to the shoulder, will be found invaluable for clearing away decalcified dentine from underneath enamel cusps. If a cavity is deep and the gum inflamed and caused to bleed easily do not attempt to fill the tooth permanently at one sitting. It is far better policy to fill temporarily with gutta-percha and build up a properly contoured alloy, filling later on.

To relieve aching temporary teeth nothing is more efficacious than zinc oxide mixed with eugenol in which a small amount of finely powdered silver nitrate has been incorporated. Carbolyzed resin on cotton may be used, but is less cleanly. After the final toilet of the cavity it is very good practice to paint it with a 10 per cent. solution of nitrate of silver or precipitate a solution of ammoniated silver by eugenol, and dry thoroughly before inserting the filling.

Temporary teeth with exposed pulps present a great problem. If found septic the tooth is better removed; some traumatic exposures may be successfully treated for the time being: whether the attempt is made depends largely on the age of the patient. If root-filling is ever attempted, ammoniated silver precipitated by eugenol followed by paraffin wax and iodoform have been found useful. A root-filling capable of absorption is certainly indicated in temporary teeth. An upper second temporary molar is often worth trying to save at the age of nine, when the corresponding lower tooth is firm and free from caries. Extraction is the best treatment in the majority of cases, but the freer use of splints of some kind to prevent the improper movement of teeth adjacent to the gap is most earnestly advocated, and is the only logical way of maintaining a crippled mouth at its maximum efficiency.

Some time ago it was thought that this splinting mattered only when the second temporary molars, especially the upper ones, were lost too early, but J. G. Turner's recent description of the wandering of temporary teeth after extractions is fully corroborated, and splints are used now in all cases where indicated.

If an exposure has been allowed to occur in the first molar before the age

of nine or before the apex is completely closed, the tooth should be removed, but a very careful prognosis should be made before deciding to sacrifice more than the one tooth.

The choice of filling-material must be made of that which under all the circumstances of the case can restore the teeth to as near their maximum efficiency as possible. Silver tin alloys have been advocated by the writer for many years, but are wholly condemned by some American authors, who maintain that their use is constantly followed by the death of the pulp. No mention is made of the technique employed in the cases quoted and whether the cavity was sterilized or not. The pulps of temporary teeth seem to become readily infected in some patients, and it has been shown that infection may occur from a carious cavity without exposure of the pulp. Copper amalgam is strongly advocated by some, and may be used with advantage in clinics where rapidity is a *sine qua non*, and one mass may be prepared for several cases at once, but its slow setting and rapid wasting with early loss of interdental and occlusal contact seem to militate against its use in most cases. To advocate its use because it can be inserted under blood and saliva does not seem a very sound or hygienic argument. Zinc oxides are better for temporary teeth than silicates when white fillings are indicated, but no hesitation should be felt about the use of silver tin alloys in the front of the mouth if teeth have to remain longer than two years. Temporary gutta-percha fillings already have been suggested where patients can be seen again soon, but as a routine it is far better to use base plate gutta-percha in all temporary teeth as one never knows whether tiny patients may not succumb to some infantile complaint before the next visit and so make the softer filling of no service. When using gutta-percha it should always be dipped in oil of cajeput before insertion.

To treat first molars having deep fissures before caries has commenced is sound practice. Dehydrate, remove any débris with a probe, and rub in black copper phosphate, leaving the mass as flat as the occlusion will allow—a method which may be called mechanical prophylaxis. This does not discolour the teeth as claimed by some operators, and if copper amalgam is advocated without condemnation of its staining properties why blame copper phosphate for this supposed quality?

### THIRD: REGULATION.

It is not to be expected that the complicated subject of the regulation of teeth to procure a maximum efficiency of mastication and to attain more perfect prophylaxis can be adequately dealt with in this short paper. In other words the clinical aspect of orthodontia can be discussed only very shortly here.

In the first place all dental surgeons who treat young patients should cultivate a habit of regularly and systematically obtaining models of all young cases. This will be found of inestimable value in after years for the purpose of scientific treatment or research. Ætiology should be the basis of our treatment, and as many conditions may be due to one of two or more causes, diagnosis may be greatly clarified by the help of the early models of a case. These models should be kept numbered and labelled in boxes together with a catalogue based on the decimal system of Dewey or any other equally scientific method. The models can be divided into two great groups, untreated B and treated A B, so that one group may be shifted into the other by the prefixing of one letter. The minute classification and outstanding features can be grouped under different decimal numbers noted first at the time of examination and enlarged as alterations take place during growth. Invaluable

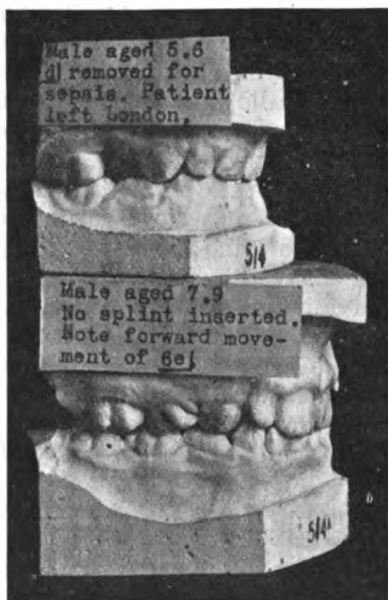
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data may thus be accumulated in a few years, which can be referred to readily without confusion and loss of time.

I should like to emphasize my astonishment at the recent repetition in this room of that oft-repeated fallacy that irregularity does not occur in the temporary dentition. Papers read over twenty years ago, and the writings of Norman Bennett, seem to have had little effect in eradicating this baseless supposition, which is repeated about as often as one hears tales of the sea serpent or the giant gooseberry. It sincerely is to be hoped that one will never hear again from any member of the Section of Odontology of the Royal Society of Medicine that malocclusions of the temporary teeth seldom if ever occur.

Children of three and over can be treated perfectly well orthodontically, but this does not imply that every child with a crooked temporary tooth is to be looked on as a case for treatment.

The suitability of fixed or removable appliances for temporary or other dentitions cannot be discussed profitably here, but the clinician should be



A Common Cause of Malocclusion.

fortified with the knowledge of the use of both these methods, as well as the carefully considered and not haphazard extraction of teeth in certain well-defined cases.

The time is arriving when the ingenuity of some operators is overcoming the difficulties under which the English school system makes us work, and by which the interval between visits can be greatly lengthened.

Let us in our clinical work be broad minded enough to acknowledge the good intentions of others even if opposed to our own ideals, and one and all try to obtain for our patients what we are conscientiously convinced will produce the maximum of efficiency from all points of view, forgetting the fatigue, annoyance or difficulty to ourselves.

[Twenty-five slides were shown to illustrate some of the malocclusions which arise in the temporary dentition, one of the most striking of which is here reproduced (see above figure.)]

## "THE CLINICAL ASPECT OF THE CARE OF CHILDREN'S TEETH."

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## DISCUSSION.

Mr. GEORGE THOMSON said he would discuss Mr. Northcroft's valuable paper rather from the side of institutional treatment. The serious condition of our children's teeth was evidence that we did not know how to feed them.

He exhibited the models of the mouth of a boy, aged 6 years, whose teeth were sound and showed marked attrition. His mother attributed the condition to the teaching she had received at the Welfare Centre and the boy had been given hard unsweetened foods and allowed no sweets. For some years he (Mr. Thomson) had been attending expectant and nursing mothers and their children under 5 years of age, and the marked improvement in the teeth was most encouraging. The treatment of the teeth of very young children from 2½ years onward was simple, painless, inexpensive and preventive. School dentistry from 6 to 8 years came too late; this was now generally admitted, and led to the sacrifice of many temporary molars which should be functional up to the ages of 10 or 11 years. He agreed with Mr. Northcroft as to the need of propaganda and instruction to parents. The Medical Officers of Health Society had issued a simple leaflet for parents which was as far as possible non-controversial.

He considered such a question as calcium metabolism should be left out of such teaching because it was theoretical. As to recommending foods containing lime salts, the trouble was that mothers and many doctors were not content with Nature's provision of human milk for babies but added to it cow's milk, which contained an excess of calcium. He urged that instruction should be on the lines of things we knew; and we did know how, practically, to prevent dental caries. The objection to copper amalgam could be overcome by expressing the mercury and using it in a drier condition.

Mr. F. J. PEARCE said that, although he agreed with most of what Mr. Northcroft had said, he was nevertheless surprised to hear that he (Mr. Northcroft) did not believe in rendering septic or pulpless, teeth in the deciduous dentition. It had been his own practice for a long time to treat those teeth if there was a reasonable chance of their being made healthy and if there was sufficient tooth left to hold a filling. He had found that it was far easier to get these teeth back into a healthy condition than teeth of the permanent series. After treatment of the canal, he filled the pulp chamber with some good antiseptic root filling and then filled the cavity with cement and amalgam; and he found that in the majority of cases these teeth were retained until the permanent



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teeth were ready to erupt. Even from a masticatory point alone this was important, but the question of retaining the space for the permanent teeth was also solved. The roots of pulpless deciduous teeth were not absorbed as completely as those that had remained vital, and it was therefore important to see that these teeth were extracted at the proper time, if not exfoliated. He had always been grateful to Mr. Northcroft for first advising him to use splints or plates for deciduous teeth that were lost early, as it was most important to retain the spaces for the permanent successors. He was not much in favour of copper amalgam for deciduous teeth, as he did not consider that it possessed any advantages over the alloys, and he agreed with Mr. Northcroft that it had several disadvantages.

Mr. G. F. CALE MATTHEWS (Birmingham) said that Mr. Northcroft's paper should be carefully studied by those who desired to give service to the child; his methods would in many cases prevent the development of malposition of the permanent teeth due to the premature loss of the temporary teeth. There must be no confusion between this condition and that arising from developmental defect of bone. The outstanding feature of Mr. Northcroft's paper was the insistence upon early inspection and treatment if necessary; and he (Mr. Cale Matthews) would especially emphasize this. It was his custom to ask young and anxious mothers to allow their children to be examined immediately the first dentition was complete. If this was done periodically all dread was eliminated and much valuable information would be obtained.

He would only like to suggest by way of criticism that providing no sinus existed, it was not always necessary to remove pulpless temporary molar teeth, as they could generally be rendered healthy for the time necessary by the method adopted by Mr. Northcroft, viz.: the application of zinc oxide, eugenol and silver nitrate, and the adoption of this method would reduce the anxieties as to crowding, which was so difficult to prevent subsequent to premature removal. Mr. Northcroft's caution regarding the natural spacing of the temporary dentition on completion and that due to subsequent bone growth should be carefully noted.

## Section of Odontology.

President — Mr. DOUGLAS GABELL, L.R.C.P.Lond., M.R.C.S.,  
L.D.S.Eng.

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### The Non-innervation of Dentine,

By Professor ARTHUR HOPEWELL-SMITH, Sc.D., M.R.C.S.

#### (A) INTRODUCTORY.

IN conformity with a statement made last year at a meeting of this Section of the Royal Society of Medicine; in compliance with expressed wishes from sundry sources, that the subject be again approached; and with a deep sense of duty and responsibility, I desire to sustain and reaffirm the position I have assumed with regard to the alleged innervation of human dentine. Lest silence on my part be misconstrued as acquiescence in the more-or-less current conception of dentinal sensitivity, I propose to discuss the *pros* and *cons* of the theory, to examine the evidence adduced to this end, and to expose, from the careful consideration and logical correlation of many points of view, the inaccuracy of the hypothesis.

The determination of the existence, or otherwise, of a nervous apparatus in dentine involves, not merely studies of the minute anatomy of that tissue, but of its physical properties, and its functions, together with the *raison d'être* of the fibrilloblasts, hitherto called odontoblasts, and the terminal mechanism of the sensory nerve-fibres in the pulp itself. It calls for more than the examination of microscopical material, stained not always with due regard to orthodox microscopical technique, but frequently by vicarious means. It includes the adaptation of well-known laws and principles governing the operations of the cerebro-spinal nervous system. It extends to clinical evidences, and embraces the co-ordination of collateral and ancillary subjects such as physics, pathology, and comparative odontology. And finally it is fixed on the basis of accurate and approved scientific interpretation. Huxley [11] summed up the methods of science as the observation of fact—including that artificial observation called experiment, comparison and classification, or the process of tying up similar facts into bundles, deduction, and verification. It has more recently been stated: [26] "It is the business of science relentlessly to determine all the evidences it uses, and not to build up vast speculations from material that will not stand the most elementary tests of stability."

The purpose of this paper is to show that the conception that dentine is sensitive by virtue of a nerve supply has been evolved from "material that will not stand the most elementary tests of stability." To deal at all adequately, on account of the exigencies of time, with the various issues involved in this

[June 23, 1924]

fundamental and important study, is obviously quite impossible. It must suffice to rapidly pass in review some aspects of the question which seem to be particularly pertinent to this occasion.

While working as assistant in the physiological laboratory of the medical school of Charing Cross Hospital, under the direction of Sir Frederick (then Dr.) Mott, I had abundant opportunities for observing the various phases of dental histogenesis, and the minute anatomy of the teeth of man and mammals. It was noted that the pulp fibrilloblasts (then called "odontoblasts") differed greatly in shape, size and arrangement, unlike the enamel cells where regularity in order, shape and size is a characteristic feature. On comparison with osteoblasts concerned in bone formation, it was apparent that the latter maintained a singularly even type of conformation. The only logical conclusion that could be reached was that the fibrilloblasts performed a higher function than that of generating dentine, and that the old belief that they formed matrix, sheath and fibril was erroneous.

Sir Charles Tomes subsequently came to the conclusion that this aspect of the question was true. Further investigations, spread over a number of years, confirmed the view: for there was unmistakable evidence to prove that osteoblastoid cells, and not "odontoblasts," were concerned in the direct calcification of adventitious dentines, pulp nodules, vaso-dentines, the dentine of odontomes, and even dentine itself.

This led to the assumption that the fibrilloblasts were intimately connected with the transmission of tactile and other external impulses to the sensory terminations on the pulp surface. Taking into consideration the facts that the dentinal tubes are filled in the fresh condition with lymph, the chief use of which is to keep dentine moist, that the dentinal fibrils also traverse the same tubes, that they are the processes of the fibrilloblasts, it would appear that these latter cells do, in a subordinate way, aid the lymph in its secondary function, viz., conductivity of impulses.

#### *Original Conception of Apparent Sensitivity of Dentine.*

Sir John Tomes in "A System of Dental Surgery," 1859, asserted (p. 281) that "dentine has no inherent sensibility in its own hard tissue." Again (p. 287) "that the dentine owes its sensation (*sic*) to the presence of the dentinal fibril cannot, I think, be readily doubted . . . It is by no means necessary to assume that the dentinal fibrils are actual nerves before allowing them the power of communicating sensation." And again (p. 281): "If the sole office of the tubes be the conveyance of nutrient fluid derived from the pulp, the difficulty of accounting for the sensation (*sic*) of the dentine remains, inasmuch as we have no instance of sensation being manifested in a fluid. We might seem to get out of the difficulty by assuming that the dentinal tubes are constantly filled with fluid, and that pressure made upon the fluid in the exposed ends of the tubes is felt by the pulp at their inner extremities."

The concluding phrase of the foregoing, clearly and concretely furnishes the correct explanation.

#### *The Nature of Dentine.*

Dentine is a connective tissue, resembling bone, in that it is non-innervated and calcified, and in this respect almost unique; but differing from bone, in that it contains no blood-vessels, and no cells. The processes of the fibrilloblasts form a component part of its structure. It is derived from the mesoderm, grows in a centripetal direction, and accords with the definition of connective tissues generally, because its cell processes are separated from

one another by a large amount of intercellular cementing substance. It is also a connective tissue in a dual sense, for it supports and connects the solid, inorganic, lifeless enamel with the living pulp. Devoid of cellular elements and a blood-circulatory system it cannot undergo inflammation, or give origin to tumours, but can and commonly does receive additions to its inner surface, either as secondary or physiological, and adventitious or pathological dentine. It is supplied extensively with lymph which, emanating in the capillaries distributed freely to the circumference of the pulp, passes into the tubes—a second distinguishing feature.

Descriptions of its origin, appearances, physical properties, chemical composition, measurements, relationships, varieties, and reactions to injury are irrelevant to the subject engaging our present consideration. But statements about the tubes and their contents are necessary for the development of the argument.

A dentinal tube may be aptly compared in appearance under the objective of the microscope to a leafless Lombardy poplar with trunk, boughs, branches and branchlets. In number the tubes are unevenly distributed, being more numerous in the root than in the crown, though their dimensions are greater in the latter than in the former situation. In arrangement they are generally parallel, separate from each other, acquiring as they pass radially from without inwards, primary and secondary curvatures, evidently for the conservation of space. Attaining a maximum length of 5 or 6 mm. the largest dimensions at their basal extremities average 1.7 to 5 microns—too minute to allow the entrance of the smallest erythrocyte, but allowing enough room for the occupation of the dentinal fibril and lymph (fig. 1).

When examining a longitudinal section one plane only is seen, the impression derived from such an examination being the same as if a tree were divided in a vertical median plane. No one has ever seen, or can possibly ever see a dentinal tube in its entirety. The tubes seldom divide dichotomously, as Kölliker originally affirmed, but present the same architectural features as a tree.

Several modes of termination have been observed—in spherical or oval *culs-de-sac*, in the interglobular spaces, in those developmental defects called enamel "spindles." The descriptions given by various writers of their ending usually in anastomotic loops, in cementum, or in the intercolumnar substance of enamel are undoubtedly incorrect (fig. 2). It is impossible for them normally to penetrate the cementum, for this tissue is developed in a centrifugal direction, that is, exactly opposite to that of dentine. Some authors have gone so far as to assert that the tubes terminate in the root in the lacunæ of cementum, holding the view that normal cementum is like bone. If it is a fact, as stated by some [17], that there exists "a chain of communication of protoplasmic material between the periodontal membrane and the pulp, as is clearly shown in a human molar in fig. 187" (p. 290), it is a most unfortunate mistake on the part of nature, because in the case of an infected pulp, bacteria would easily and invariably gain access to the alveolo-dental periosteum via the tubes and their branches, through the granular layer of Tomes, the homogeneous layer and cementum. The intervention of the homogeneous layer of dentine, and the solid, structureless nature of normal cementum absolutely prevent the occurrence of such a disaster (fig. 3).

#### *Modern Developments of the Theory.*

The middle of last century saw the beginning of the search for dentinal nerves. It started inauspiciously and continued unfruitfully for those engaged in the exploration. Boll [2] used chromic acid, unaware that this histological

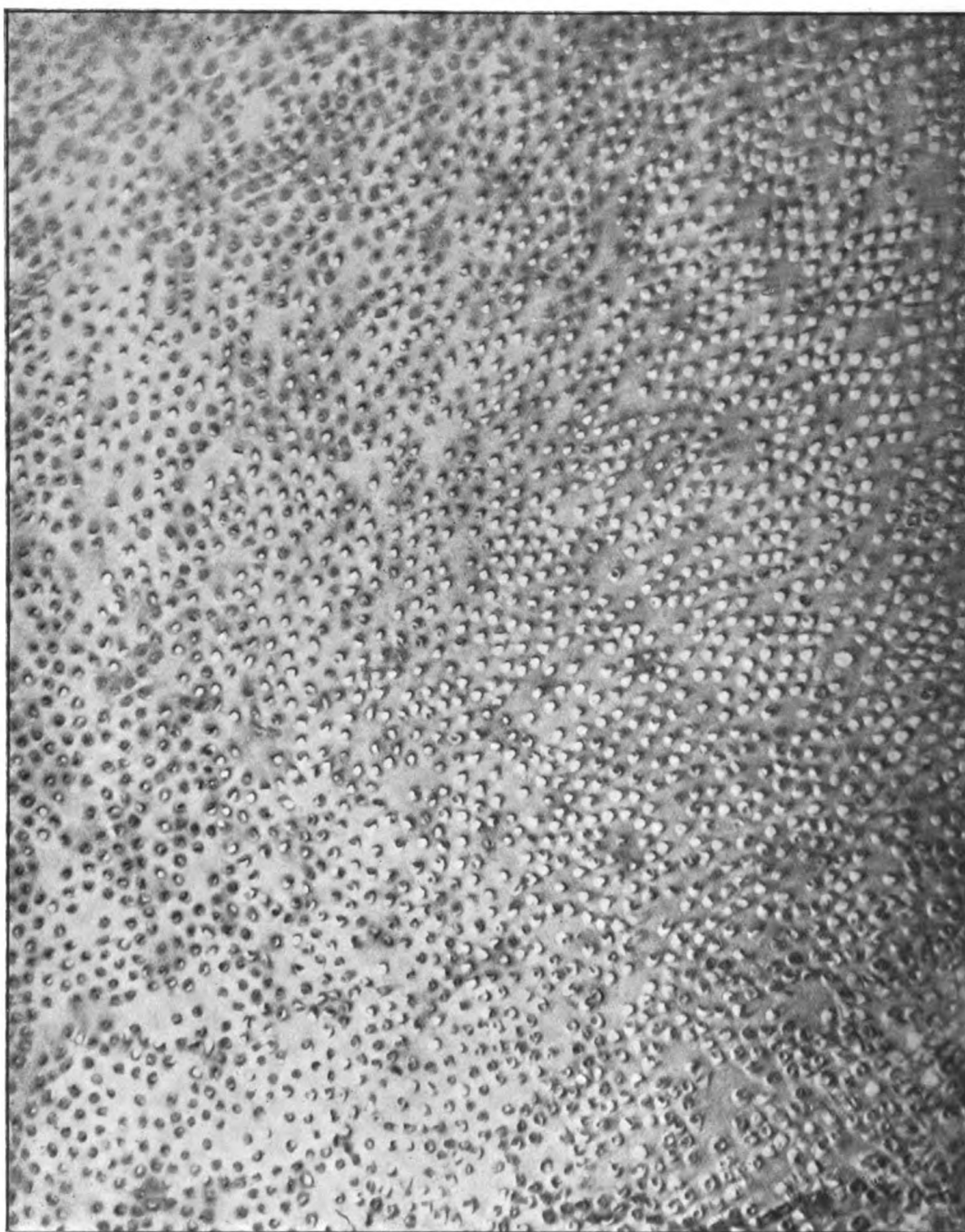


FIG. 1.—Transverse section of human dentine showing the smallest tubes in the radicular region. Many dentinal fibrils, appearing as dots, remain *in situ*. Magnified 1,200 times.



FIG.2.—Terminal portions of radicular tubes of human dentine. At A, apparent, but not actual, anastomotic loops. Magnified 1,100 times.

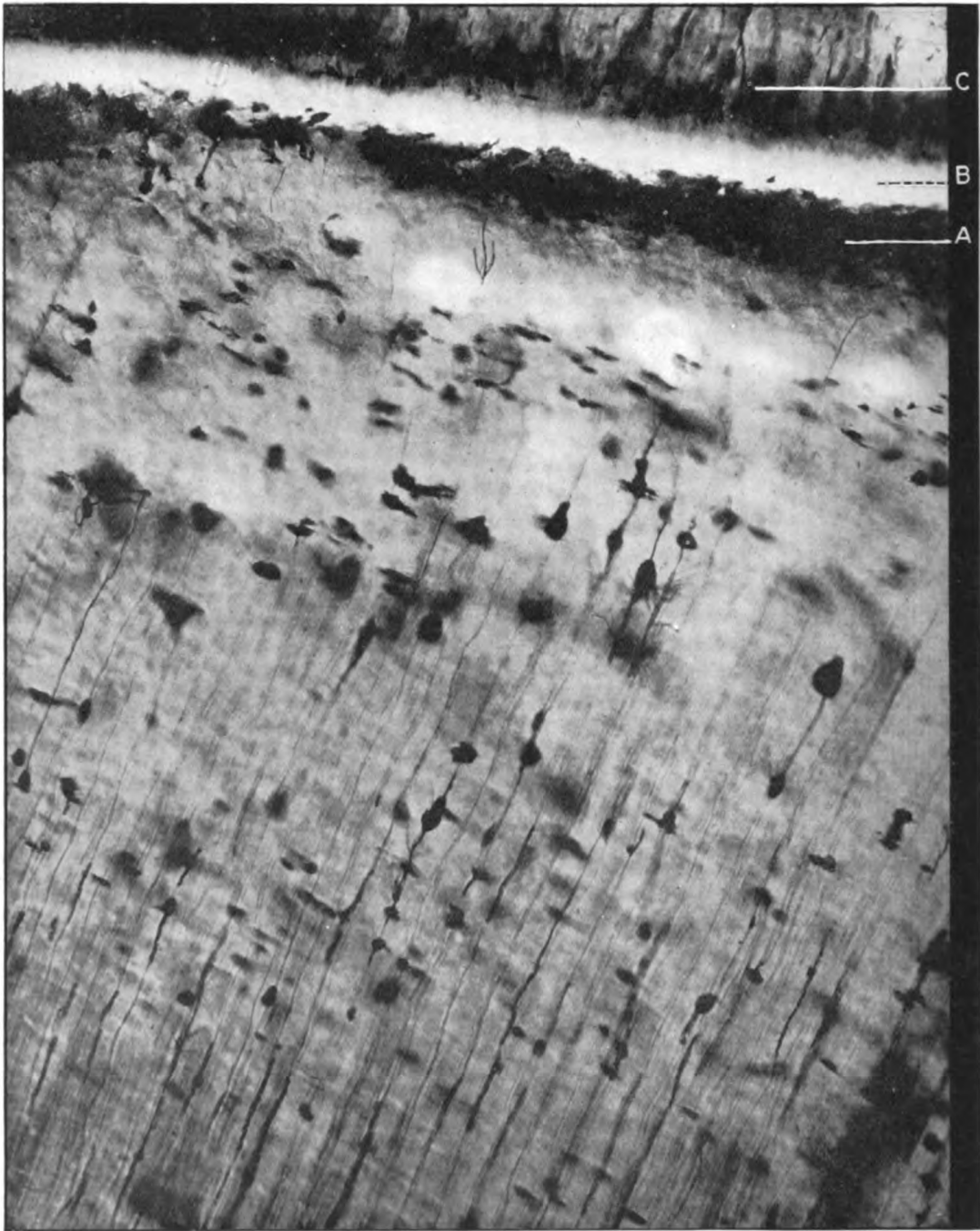


FIG. 3.—Terminal portions of radicular tubes of human dentine; A, the granular layer of Tomes; B, homogeneous layer of dentine—solid and structureless in normal conditions; C, cementum. Magnified 1,250 times.



reagent does not, *per se*, stain nerves. Morgenstern [15] described medullated nerves as entering the dentine in special tubes and terminating in the enamel. These special tubes are non-existent. Römer [24] originally conceived the sensory nerve-fibres as piercing the interiors of the dentinal fibrils, which is contrary to what obtains elsewhere. Cell processes are not hollow; nerves nowhere terminate in cell processes. As mentioned in last year's paper, Römer [25] has, however, altered his opinion, and now says: "I am doubtful about the conception." Dependorf [4] described and illustrated nerve-fibres in the matrix or ground substance of dentine, totally disregarding the general principles of histology, physiology, and phylogeny.

With regard to Howard Mummery's contentions, I have reviewed them at some length elsewhere [9]. I cannot accept them nor advise their acceptance, nor are they endorsed by Hanazawa [7] or Walkhoff [28]. Why the pulp—an organ which sooner or later "undergoes retrogressive metamorphoses and maintains its vitality at a low ebb . . . which is seldom capable of adjusting itself successfully to functional and organic disturbances" [8]—which, in short is a degenerate tissue—should be specially singled out as an organ containing a complex arrangement of nervous elements dissimilar to that of any other part of the body, cannot be satisfactorily explained. The lack of uniformity and agreement between these exponents of the theory is remarkable and significant, and completely shatters their main argument.

#### (B) EVIDENCES AGAINST THE THEORY OF THE INNERVATION OF DENTINE.

The hypothesis of the innervation of the dentine can be contravened logically, cogently, and with the utmost certitude, by evidences easily adduced by studies of histology, physiology, pharmacology, pathology, comparative anatomy, ethnology, and those based on clinical knowledge and experience. Diverse as these subjects are, each clearly reveals the slender, nebulous and unstable character of the theory.

(A) *Histological Evidences.*—(i) The theory briefly postulates that one or more nerve-fibrils occupy each dentinal tube. As pointed out twelve months ago [8] the numbers of the tubes may vary, in different areas of dentine, between 600 in one square millimetre—a low estimate—to 37,600—a high estimate. That means there are present, at least, a like number of nerve fibrils in a square millimetre. But more than this. The statement has been further amplified, in that it is asserted that *each branch of each tube* contains a nerve-fibril. Thus: "These fibrils end in arborizations beneath the enamel and cementum following the fine branches of the dentinal tubes" [16], and "The dentinal fibril contributes delicate protoplasmic offsets to the branches of the dentinal tubes which also contain the finer subdivisions of the neuro-fibrils" [17]. If the branches of the tubes be counted (and there are less in the crown than elsewhere) it will be found that there may be as many as 100 or more arising from one tube. I have counted in one plane as many as forty-six in one twenty-fifth of a millimetre. Thus, taking the lower estimate, there would be 60,000 arborizing neuro-fibrils in a square millimetre, and taking the higher estimate, no fewer than 3,760,000 in a square millimetre; truly a preposterous figure. If, however, a tube be regarded as an arborescent canal then the figure would run into several hundreds from a single tube. To give anything like an adequate estimate of the average total numbers of the branches and branchlets of a single tube cannot possibly be attempted.

(ii) As just noted, the coronal tubes are less freely supplied with branches than the radicular tubes. The obvious and justifiable inference, therefore, is



that the root portions of dentine must contain more nerves than the exposed coronal parts.

(iii) The lumina of the majority, if not all, of the ultimate radicular branchlets of the tubes are too minute to admit even the passage of a dentinal fibril, as well as "one or more" ultimate neuro-fibrils—diminutive and imponderable as these divisions of the axones may be. The branchlets, even under so high a magnification as 2,000 diameters, appear only as delicate threads. It is absolutely impossible for a single tube with branches and branchlets to accommodate 100 or 200 neuro-fibrils! (See fig. 4.)

(iv) Axones of nerves have never been known to terminate in naked neuro-fibrils or arborizations of 5 or 6 millimetres' length, which they would be required to do if they "end in arborizations beneath the enamel" [16], for the dentine in the crown frequently attains the dimensions just named. The terminal ramifications of sensory nerve-fibres in the tactile corpuscles, in the conjunctival or peritoneal end-bulbs, in the Pacinian bodies, in the organs of Ruffini and Golgi are few in number and restricted in length. If the sensory nerves of the pulp are prolonged into the tubes and their branches, there exists a most extraordinary and unique arrangement of the nervous system, which is entirely unknown to the general histologist, passes all comprehension, and does not obtain in other parts of the body.

(v) As evidence in support of the theory diagrams and photomicrographs have been published, which exhibit nerve terminations in the pulp in the shape of a brush. This brush-like formation of sensory nerve termination has never been described or figured by any general histologist. Nothing similar, or even approximately similar, occurs in the highest and most complex of nervous tissue. It is not seen in the structure of cerebrum, cerebellum, medulla, cord, or the peripheral parts of the nervous system of man. A serious lack of parallelism in the published descriptions of this "brush-like" termination may be here noted. The legend under the drawing fig. 1 (*Phil. Trans. B.*, vol. 202, plate 18), reads: "A bundle of medullated fibres . . . The axis cylinders are seen breaking up into a multitude of neuro-fibrils . . . Magnification 180." The legend under fig. 128 in the "Microscopic Anatomy of the Teeth," a very poor photomicrograph of the same section, runs: "Axis cylinder of medullated nerve expanding in a brush-like form at crown of pulp. Magnification 600" (writer's italics). The figures show identical structures, viz., a bundle of nerve-fibres more loosely separated from one another in the upper than in the lower portion: but there is an unwarrantable discrepancy in the descriptions.

(vi) It is freely admitted that the so-called "end-cells" in the pulp "are not seen elsewhere in the body . . . Such a mode of termination of sensory fibres in 'end-cells' is not met with in other organs" [18]—a remark all the more noteworthy on account of its lack of authenticity, an admission which may be regarded as startling and subversive, and, considered *per se*, upsetting and demolishing the theory. In this connexion it is enough to say that, for scientific investigation, all *ex parte* statements must be judged on their own merits.

(vii) The indefinite character and unreality of appearance of the so-called "end-cells," and the failure of reproduction by the camera of the so-called "synapses," warrant the conclusion that these so-called "end-cells" are connective tissue pulp cells, and not elements of the nervous system at all. This contention has been sustained and corroborated recently by the opinion of an eminent and widely experienced histologist and anatomist to whom the

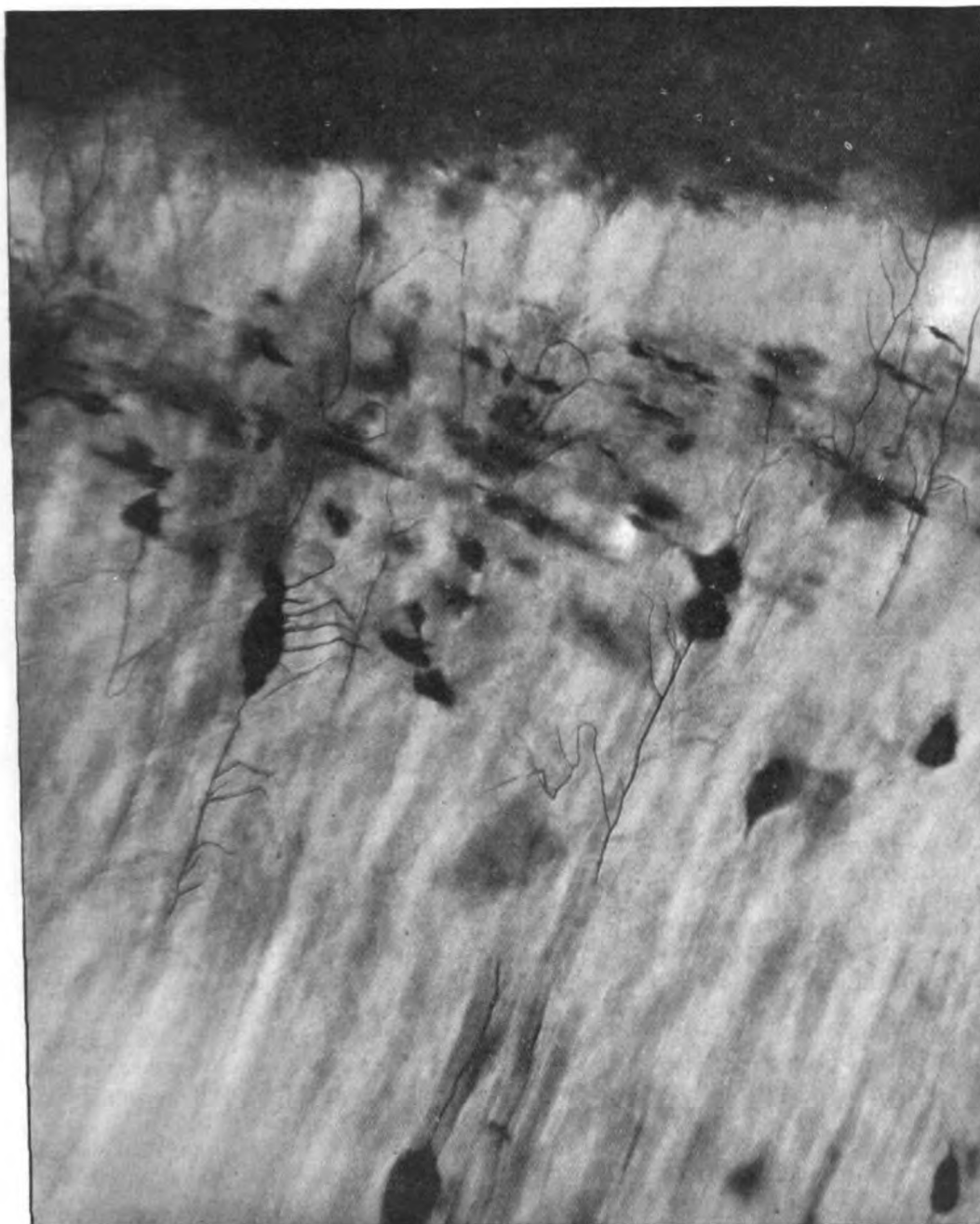


FIG. 4.—Ultimate and penultimate branches and branchlets of the radicular tubes of human dentine. Magnified 2,000 times.

argument and accompanying illustrations were submitted. Also, to repeat, it is extremely unlikely, even inconceivable, that a degenerate organ like the pulp, the most important function of which is to supply dentine with lymph, would be provided with a nervous equipment wholly dissimilar from and more intricate than that found in other fields of anatomy—a mechanism highly organized and complex. The “end-cells” and “synaptic terminations” and “axone” shown in a diagram intended to clarify the conception of the innervation of the dentine remind one of the unipolar neurone which occurs in some of the lower vertebrates and invertebrates.

(viii) No published photomicrograph has demonstrated definitely and unequivocally, both dentinal fibril in continuity with the fibrilloblast, and a neuro-fibril as an extension of the axone of a myelinic (medullated) nerve in the same dentinal tube at one and the same time.

(ix) If nerves make synaptic connexions with “end-cells” which transmit neuro-fibrils to the dentinal tubes, more complete evidences and proofs and fuller descriptions than have yet been available, should be given, on account of the importance of the subject. One can merely explain this lack of more detail to a hesitancy on the part of the originator of the theory which amounts almost to incredulity in his own hypothesis. For, on being asked whether he had “seen the terminations of the nerve-fibres in the dentine,” he replied: “I should say I was doubtful . . . I have never traced any nerve-fibres into the cementum, but I am not at all convinced that they do not pass in. It is very difficult to see them” [19]. This indecision on the part of the author is significant in the light of the illustration (there numbered fig. 3) which accompanies a paper read before this Society where he evidently represents what he considers to be the case.

(x) The histological evidence is undoubtedly based on misinterpretation of a certain type of artefact obtained after modification and re-modification of well known and much used and established methods and principles of staining, specially adapted to nerve elements. Not only are the dentinal tubes more or less filled to capacity at their basal embouchures—and nowhere else—with dots or “beads” or “gemmules” of varying size and shape, which are considered to represent the varicosities of true nerve-fibrils, but these “beads” are by far too numerous. Neuro-fibrils present a few “beads,” the numbers of which can usually be readily estimated. Further, the “beads” in the tubes are not connected to one another by a neuro-fibril; they are mainly discrete, being by no means confined to the tubes and their branches, but visible also in countless numbers in the pulp and in or on the matrix of the dentine. In other words the so-called beads are nothing more nor less than the artefacts above-mentioned—minute precipitates or granules of reduced metallic impregnation stains, which have, unfortunately, been misapplied to a tubular tissue like dentine.

Similar results can be obtained by the use of carmine, which has no special affinity for nerves, and cannot be regarded as a specific nerve stain.

In all the above the appearances depicted by photomicrograph or drawing are capable of a different interpretation to that which has been ascribed to them.

(B) *The Physiological Evidences* may be regarded as negative and positive.

(i) The well established fact that the branchings of the radicular tubes of dentine are infinitely more numerous than those elsewhere has already been noted. If the supposition that each branch of each tube were penetrated by a neuro-fibril was correct, it would be equally a fact that this part of the dentine, protected normally from injury and disease by the coverings of

cementum, alveolo-dental periosteum, alveolar process and gingival tissues, would be more percipient of impulses than the exposed parts; that, in short, the root would be more sensitive than the crown. But this, of course, is not so. Sensory nerves are not required here. The function of a sensory nerve is either protection, or for special purposes, but not motor or trophic. Even if protection by the nervous system were needed, there would be no reason for an extraordinarily abundant supply of nerves.

(ii) The explanation of the existence occasionally of an apparently sensitive area of the cervical surfaces of premolars and canines—in seeming confirmation of the theory under discussion—is that, according to Thorsen [27], in 5 to 10 per cent. of specimens he examined, there is a developmental defect of the arrangement of the three calcified tissues, in which the usual overlapping of enamel by cementum is not found, but where they fail to meet, and dentine is actually exposed. Tactile impressions and thermal changes are immediately conducted to the pulp in such circumstances by *pressure* on the parts and by *variations of temperature* respectively.

(C) *Pharmacological Evidences*.—The mere application of solutions of cocaine hydrochloride to exposed surfaces of normal dentine without pressure has no appreciable effect—therapeutic, obtundent or otherwise. In a private communication to the writer, Professor Hermann Prinz states:—

“True local anæsthetics, i.e., cocaine or its substitutes when applied to exposed sound dentine without pressure do not produce any pharmacological effect. Even if sealed into a fairly deep-seated cavity in which the underlying dentine is not softened, no effect is obtained. . . . When cocaine or its substitutes is forced into the living protoplasm of the unobstructed dentinal tubes under pressure, its anæsthetic action is manifested in a few minutes. The vital resistance of protoplasm is readily overcome by comparatively slight force, which quickly transfers the anæsthetic solution, by an increased osmotic interchange, to the surface of the pulp. The phenomenon is to be explained as an anæsthesia obtained by the intimate contact under pressure, either by mechanical or by electro-motive force as in cataphoresis. The pulp of a tooth may be completely desensitized by any one of the well known methods of contact pressure or that derived from a hypodermic syringe or some other more complicated apparatus, or by electricity.”

It is unnecessary to do more than merely mention the fact that cocaine desensitizes the peripheral ends of sensory nerves—a fact which leads to its universal employment by ophthalmic surgeons before operating on the eye, or the rhinologist and laryngologist on nose and throat. When painted on the dorsum of the tongue, sapid substances can no longer be perceived, the effect lasting from half an hour to two and a half hours. If there were a distribution of sensory nerves to the dentine they would certainly be affected similarly to those in the cornea, conjunctiva or tastebuds.

(D) *Pathological evidences* negative this theory. Attrition of enamel and dentine is invariably accompanied by the formation of secondary dentine laid down by the pulp nearest the breach of surface. The condition is generally painless. There is no proof available to show that the formation of any of the varieties of adventitious dentine is associated with pressure on sensory nerve fibres. Complete calcification of the pulp may be, and in the case of non-carious mandibular incisors is, invariably unattended by any odontalgia whatsoever.

(E) The position is invalidated also by studies in *comparative anatomy*.

(i) Nerves have never been demonstrated in the dentines of mammals, fishes, or reptiles. Retzius [22] examined the pulps of *Mus*, *Gobius*, *Gaster-*

*osteus*, *Lacerta agilis*, the larvæ of *Salamander maculata* and *Triton cristatus*; Huber [10] those of *Canis familiaris*, *Felis domesticus* and *Lepus*. In no instance were nerves observed entering the dentinal tubes.

(ii) The erupted parts of the persistently growing incisors of rodents are but partially covered with enamel, the remainder being exposed dentine. No evidence can be adduced to prove that this dentine is sensitive; no sections have ever been made which exhibit any trace of a nerve supply.

(iii) The attendant on the larger vertebrates in the gardens of zoological societies can and does with perfect equanimity and impunity operate when necessary on an irregularly growing tusk of elephant or hippopotamus without inflicting any pain or discomfort on the animal. Incisors are sawn or filed, and the exposed dentine is free from nervous reactions.

(F) *Ethnological Evidences*.—It is a well-known fact that the natives of certain parts of Africa, North, Central and South America, Australasia, Polynesia, and Melanesia have practised even from neolithic times, and do still continue to practise the art of mutilating the teeth by chipping and filing. Testimony as to this has been given by Livingstone [13], Galton [5], Sir Harry Johnston [12], Sir George Grenfell [6], the Duke of Mecklenburg [14], du Chaillu [3], van Rippen [23], and many others. In writing of the African natives, van Rippen says:—

“I could safely advance the theory that mutilations are practised for reasons of adornment only. . . . After the teeth are once shaped to a point, the subject in question seems from time to time to file them in order to keep them so. This conclusion is based on the evidence that the older the subject is, the smaller his mutilated teeth are, although they are still very pointed. It seems that when the tip fractures the process is begun over again.”

Werner [29], writing on this subject, asserts:—

“As to the teeth, it was a standing wonder to me that the way they were treated did not ruin them entirely; but it does not seem as if chipped teeth decayed more readily than whole ones. Naturally, as most travellers have reported, natives usually have splendid teeth; though Dr. Fulleborn, in his observations on tribes at the north end of Lake Nyassa, says he found a considerable percentage of people with decayed teeth. I have come across one or two cases of toothache myself, but should say, that on the whole, there is no need to revise the general opinion. The Yaos chip the edge of the four upper front teeth into saw-like points. This is usually done to boys and girls at about 15 or 16. I never saw the operation performed, but fancy that a mallet and chisel are the instruments used. They are brought up to face the prospect, I suppose, and seem to contemplate it with more equanimity than most of us do going to the dentist. . . . A triangular gap between the two upper front teeth is made by different tribes, the Anyika of North-west Nyassa being one. . . . Some of the Makua tribes file each separate tooth to a point; this is also done by the Basenge, and I believe other tribes near the Luangwa.”

No comment on the foregoing is required to indicate the impossibility of the continuance of these customs if there were sensation in the dentine.

(G) It is unnecessary to deal at all exhaustively on this occasion with the counter-evidences and records gained daily by *clinical observation and experience*. But it is important to recall several instances afforded by common knowledge in this connexion.

(i) As was pointed out last year [8]:—

“It is a well-known clinical fact that the formation of carious cavities in teeth with living pulps may be absolutely unattended by pain or other subjective sensation on the

part of individuals. The usual *prodromata* of nervous disturbances in the pulp associated with dental 'caries' are frequently entirely wanting. If nerves were present in the hard dental tissues this could not possibly occur."

(ii) Fracture of a tooth, e.g., maxillary incisor, induced by a fall, blow from a cricket ball, &c., when confined solely to the enamel—a somewhat rare contingency—is unnoticed except for cosmetic reasons. Even when dentine is exposed, pain is sometimes only experienced on conduction of thermal and chemical stimulations. When the pulp is exposed, odontalgia is at once complained of. If dentine were really innervated it would always respond immediately to the traumatic cause, as do the conjunctiva, cornea or tongue.

(iii) Operations of any kind on the dentine would be virtually impossible if it were provided with sensory nerves. The scraping of an excavator or drilling of a bur produces sensigenous impulses which are conducted to the pulp by the pressure which it exercises on thousands, probably millions, of columns of inelastic fluid, lymph, and the dentinal fibril in the tubes, branches and branchlets. A sharp instrument, like a sharp razor blade, inflicts less pain than a dull blunt instrument, not by virtue of its thin cutting edge, but because less pressure is needed to remove the softened tissue in the floor and at the sides of the carious cavity. A light touch is invariably appreciated by the patient. Heat generated by a rapidly revolving bur produces a like effect.

(iv) Several contradictory statements have been made in describing the character of the pain experienced during the excavation of a cavity:—

(a) "In the excavation of a carious tooth, although little pain is felt on removal of the superficial layers, when the lowest layer is raised from the healthy (*sic*) dentine beneath, the pain is acute" [21]. (b) "In excavating a tooth it is frequently found that deeper in the cavity less pain is experienced. . . . In certain cases excavation does not cause any appreciable pain during the whole operation. This is probably due to the action of the acid produced by the bacteria, which penetrate in varying degrees in different cavities beyond the range of bacterial infection. This acid probably first irritates and later numbs or destroys the nerve-fibres in the affected area" ("Sensation in Dentine," *British Dental Journal*, March 1, 1924).

This is an ingenious, but not plausible nor acceptable explanation. If sensory nerves were present in the dentine there would be no appreciable alteration of, or difference in the quality or intensity of the pain. Subjective symptoms of injury to the cornea are all alike.

(v) Any elevation or lowering of the thermal scale induces pain in the pulp of a normal tooth, conduction taking place through enamel and dentine. The application of a blast of hot air to a cavity in carious dentine results in the immediate surface dehydration not only of the dentinal matrix itself, but the drying up of the lymph in the tubes, thus reducing to some extent the lengths of the inelastic columns, and so—during excavation with an instrument—slightly reducing pressure on the anatomical threshold of the pulp. Dry dentine does not conduct physical or chemical or electrical impulses so quickly as moist dentine for the same reason. The absorbent and refrigerant properties of absolute alcohol lower the temperature of the parts, with an appreciable effect on sensation in the pulp.

(vi) Some years ago Robert Arthur [1] advocated and practised filling the teeth for removal of superficial "caries" of enamel and dentine. When alluding to the question of the likelihood of pain following exposure to the latter he wrote:—

"The separations, made at a period so early that they can be rendered great or slight at pleasure are rarely the cause of any serious annoyance. I have made most careful inquiry of many persons whose teeth have been preserved by the means described and the almost universal reply has been that no inconvenience is experienced from that cause."

(vii) If the conception and doctrine of the innervation of dentine were founded on fact, then the current operative and therapeutic procedures in dealing with exposed dentine would be illogical, and would long ago have been revised or abandoned. If the opposite is true, there would be no need for modifications. And there has been no recorded fundamental change.

#### IN CONCLUSION.

This is not a matter of disputing the existence of histological appearances in the basal extremities of the dentinal tubes, and on the surface of the pulp, after the use of certain technique, and adjective staining of the metallic impregnation type. But it is a matter of questioning, in research of this kind, the advisability of employing reagents and stains of a doubtful value for the elective and special demonstration of nerve tissues—which, as the advocates of these methods themselves acknowledge, are not altogether reliable so far as satisfactory results are concerned—and of drawing deductions from them. It is not difficult to "Golgify" a potato; and there are no nerves in starch!

Inferences have been based on the interpretation of appearances produced by material subjected for three or more days to a temperature of 90° C. (194° F.); and sections nineteen years old "which," for some extraordinary but unexplained reason "had improved by keeping!" (*Preliminary note*, 1911). Above all, it is on the interpretation of the appearances produced in these and other ways, and the absence of convincing, irrefutable, collateral evidences to support the theory that criticism and scepticism rest. It is a mistake to restrict the consideration of this subject to the mere examination of microscopical material.

The subject of the so-called innervation of the dentine has hitherto been regarded by some with a limited vision. . . . It has been simply and incautiously adopted without confirmatory investigation or even serious attempts at confirmatory investigation, or due regard to the principles of anatomy, physiology, and, one may add, common sense. No authoritative support has been offered or supplied by extrinsic witness or endorsement based on independent impartial and reliable research and experiment, and verification. The general anatomist, general histologist, general physiologist is not fully conversant with dental anatomy and physiology. Otherwise he would revise his existing nomenclature, overhaul the inaccurate and worn-out pictorial aspects of his transcribed descriptions, and adapt his references to the dental organs to more modern and more exact methods. A new orientation and thorough re-valuation of the opposing contestant facts and fancies associated with the question are generally needed.

There are no nerves in dentine: the animal economy does not require them in this or any calcified connective tissue. There is, consequently, no sensation in dentine. There is no hypersensitivity of dentine.

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### DISCUSSION.

Mr. HOWARD MUMMERY said that as Professor Hopewell-Smith's paper was to so great an extent a criticism of his work he might perhaps be permitted to make a few remarks on the subject.

A very fine series of photographic enlargements of the dental tissues had been exhibited but both these and the specimens under the microscopes appeared to have no bearing on the question of the innervation of the dentine. He (Mr. Mummery) did not propose to discuss the various physiological, clinical, and other objections but to confine himself to a consideration of the histological portion of his paper.

The attempt to disprove a histological fact by considerations of this nature was an inversion of the proper order of scientific investigation: anatomical demonstration should precede, not follow, explanation of function. It was always found that when the actual structure of an organ or tissue had been demonstrated all the phenomena exhibited by such tissue were explained by the anatomy of the part. He could not, therefore, consider that the discussion of these phenomena, as subversive of the facts, was of the slightest value. It was a question of anatomy and no consideration of a theoretical or hypothetical nature could bear any weight.

Professor Hopewell-Smith said the evidence of the innervation of the dentine was unconvincing and unconfirmed. It was no doubt unconvincing to him but not to others, and it was not unconfirmed, as both Dendendorf and Fritsch had shown nerve-fibres in the dentinal tubes. The photograph shown upon the screen did not fairly represent Dendendorf's work, as the illustrations showing nerves in the dentinal tubes were not shown, but only those showing nerve-fibres crossing the matrix, this abnormal condition being considered to throw discredit on Dendendorf's whole work.

Taking from the abstract a few of the histological objections the author adduced against what he was pleased to call the "theory" of the innervation of the dentine, he said in the first place that the fine branches of the tubes were less numerous in the crown than in the root. This was a statement which had been repeated from one text-book to another, but although more difficult to bring out by stains, being deeper in the substance of the dentine, they were seen in great abundance beneath the enamel when a section was suitably treated with silver nitrate. Again, he did not know on what grounds or on what authority the author limited the size of nerve-fibres. Sir Charles Tomes in his "Dental Anatomy" said, when speaking of the probability of nerve-fibres entering the dentinal tubes:—

"It must be remembered that in those tissues which are naturally so thin as to present great facilities for examination, nerves of a degree of fineness unknown elsewhere have been demonstrated, in other words, the easier the tissue is to investigate, the finer the nerves which have been seen in it, while dentine is among the most difficult substances conceivable for the demonstration of fine nerve-fibrils."



This author again stated that brush-like terminations of the medullated fibres of the pulp were unknown; they had, however, been shown by him (Mr. Mummery) and others, and Professor Hopewell-Smith might have forgotten that in his "Patho-histology of the Teeth" he himself said: "*In many instances the bundles stretch up to the sub-odontoblast region and then very suddenly burst forth in minute scopiform strands,*" and he referred to the illustration in Rose and Geysi's atlas.

He (Professor Hopewell-Smith) boldly asserted that the so-called end-cells were only found in lower vertebrates and invertebrates, but as Sir Arthur Keith pointed out, the tooth was a very primitive structure formed by an involution of the ectoderm, and the nerve distribution might be also of a primitive nature resembling that in Nereis and the earthworm.

Professor Hopewell-Smith said that there was no proof of the existence of both dentinal fibril and nerve-fibre side by side in the tubes. He (Mr. Mummery) had shown them entering together.

The author, again, considered that what he (Mr. Mummery) had described as nerve-fibres were artefact deposits of gold or silver misinterpreted as nerve-fibres. He made this statement without having seen the preparations he was discrediting and if substantiated it would nullify the value of the nerve researches in recent years carried out by the methods of Golgi, Ranvier, Ramon y Cajal and others. He showed on the screen an example of artefacts but certainly no observer would ever mistake these blobs of carmine in the tubes as indicating structure.

Apart from metallic impregnations he (Mr. Mummery) had shown fine nerve-fibres passing into the dentinal tubes by the aniline blue and safranin method of Stroebe.

He regretted that so much of the personal element should have been brought into this question on many occasions. It was one of great general importance to the profession and remarks made as to improper methods of preparation and unsuitable staining which the author of this paper had so frequently made were most undesirable. Also the fact that Professor Hopewell-Smith had never seen his (Mr. Mummery's) gold preparations or expressed any wish to do so, very seriously detracted from the value of his criticisms.

Nearly ten years ago Professor Hopewell-Smith said he would leave the decision to expert histologists. When their opinion had been presented to him, however, and their confirmation of the nervous nature of the fibres had been recorded, he receded from this position and still maintained his original statements unaltered.

He (Mr. Mummery) was quite content to abide by his paper and demonstration before this Section in April last and the remarks in the discussion which followed.

Mr. A. W. WELLINGS said he was one of those who believed in the innervation of the dentine. For years he had been trying to obtain convincing histological proof of what his experience in practice as a dentist had set up in his mind as an established fact, i.e., that sensation in dentine was due to direct stimulation of nerve-fibres. That this had not been demonstrated to the satisfaction of Professor Hopewell-Smith did not prove the non-existence of nerves in the dentine; rather did it suggest faulty histological technique. The matter was very difficult, as we were dealing with a combination of calcified and soft tissues, and the deposition of gold and silver was notoriously uncertain. He had, however, succeeded in making preparations which showed nerve-fibres passing across the odontoblast layer to the under surface of the dentine, which he was convinced they entered. But this latter fact was obscured, as the edge of the dentine was frequently completely black from the amount of reduction that had taken place, or at other times there might be no reduction at all. If, however, the audience was still unconvinced, he would have to refer those present to familiar chair-side phenomena. The sensitiveness of dentine at its periphery beneath the enamel was just what one would expect if all the minute terminal branches of the tubules contained nerve-fibres. The peculiar intensity of the pain here was such that it was quite common for a patient to exclaim, "You are on the nerve!" The dentine of some of his own teeth was more painful in this situation than the pulp itself.

Was it not in keeping with nervous phenomena generally that the stimulation of a sensory nerve at its ultimate distribution should bring about a more vigorous reaction

than if the stimulation were applied at some intermediate position? If that was so, it afforded an explanation of the fact that once this spot was passed the dentine nearer the pulp was usually much less sensitive. But the effect of continued irritation on the terminal branches might be to induce in the main fibre a condition of extreme excitability, in which case the dentine would be painful right up to the pulp. The variability in sensitivity might be explained on the supposition that in some cases the nerve-fibrils were destroyed by the bacterial toxins, while in others the action of the bacterial products and the acids generated was to induce a condition of extreme irritability so that the slightest impulse would be transmitted as pain. One might here suggest that normally the fibrils of the dentine did not transmit impulses as pain. They were probably more concerned with tactile sensation which served to regulate the force exerted in the act of mastication and the rejection of unsuitable material. It was only under excessive stimulation, such as the heat of friction or when their nature was altered by exposure to the fluids of the mouth that dentine became painful. It was well known that a cavity which had been cut up one day without pain and packed with a non-adhesive temporary filling like gutta-percha, might have developed acute sensitiveness at the next visit. The vascular disturbances of the pulp, often associated with quite early stages of caries long before infection of the pulp usually took place, were more easily understood if one accepted the fact of the presence of sensory nerve-fibres in the dentine. The fifth nerve, as it was known, contained some sympathetic fibres from the cervical ganglia, and doubtless some fibres were supplied to the pulp, although they might not yet have been demonstrated; and it was possible that vaso-dilatation was induced reflexly through irritation of the sensory terminals or by way of the first collateral acting directly on the pulp vessels, i.e., by axone-reflex. He (Mr. Wellings) was not much disturbed by the comparative evidence offered by Professor Hopewell-Smith. The difficulty of investigation was as great here as in the case of human teeth. It might be, of course, that nerves in dentine appeared only recently in the evolutionary process, confined possibly to the human species.

Mr. MONTAGU HOPSON said that whilst he had nothing but admiration for the very beautiful photomicrographs of the skillfully prepared sections shown upon the screen, he was unable to agree with the deductions Professor Hopewell-Smith drew from them as to the impossibility of nerve-fibres passing into the dentinal tubes. As he (Mr. Hopson) had definitely stated at a previous meeting of the Section, careful examination of Mr. Mummery's sections had convinced him that dentine was innervated, and nothing he had seen or heard that evening tended to alter his expressed opinion.

Mr. A. T. PITTS said that Professor Hopewell-Smith had adduced a large number of reasons to support his view that dentine was not innervated. It seemed to him that there was only one part of the paper which really mattered, and that was the statement that the microscopical appearances described by Mr. Mummery and interpreted by him as neuro-fibrils penetrating the dentine, were, in reality, artefacts. The rest of the paper had no scientific significance, for, if the sections prepared by Mr. Mummery were true representations of the anatomical distribution of the nerves of the pulp, then the various hypothetical objections against such a distribution were without validity. He (Mr. Pitts) had had many opportunities at his leisure of examining the actual sections prepared by Mr. Mummery. He had no doubt whatever that the fibrils entering the dentine, and the layer of nerve-cells, were neither artefacts nor connective tissue fibres or cells, but were what they purported to be, namely, a layer of nerve-cells from which axones passed into the dentinal tubes in company with the protoplasmic fibrils.



## Section of Odontology.

President—Mr. DOUGLAS GABELL, L.R.C.P., M.R.C.S., L.D.S.E.

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### Ultra-violet Radiations ; Their Uses and Limitations.

By I. S. SPAIN, L.D.S.R.C.S.Eng., and F. N. DOUBLEDAY,  
L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

#### (I) Mr. SPAIN.

DURING the past ten years there have been many forms of apparatus placed on the market for the treatment of pyorrhœa by electrical means. A number of these deal with radiations which can be classed as ultra-violet. It is my intention to-night to confine myself to the radiations produced by the following means: True ultra-violet lamps such as:—

(i) Carbon arc; (ii) tungsten arc; (iii) mercury vapour lamp; (iv) low penetration X-rays, and, although not strictly in the ultra-violet category, (v) high frequency currents.

I propose to deal first with the description of the different apparatus, the type of radiation produced, and, in brief, the properties of that radiation.

Both Mr. Doubleday and myself are prepared to have pointed out to us that very little, if any, good has ever resulted from their use in the treatment of pyorrhœa; but we must take into consideration the fact that they are all being used with considerable success by medical men for the treatment of many diseases, one of which is certainly similar to pyorrhœa in its aspect, and therefore it becomes us, as dental surgeons, at least to understand their properties and limitations.

At the commencement of this paper I will explain briefly the position of the ultra-violet radiations in the spectrum, and their relations to X-rays on the one hand and wireless waves on the other.

In 1666 Sir Isaac Newton, by causing a beam of light to be cast through a prism, produced the first spectrum, and it was found that the reason why the different colours were refracted to different degrees was because they differed in wave lengths. The figures above the spectrum give us roughly the wave lengths of the colours making up ordinary light. We shall see later that the sun's rays contain waves of greater and lesser length than can be seen with the eye.

In our early studies of physics, which doubtless many of us have forgotten, we learnt what a wave length was. For the sake of those whose minds have been

occupied by other more important things since that day, may I briefly explain it. When an electric current is passed through a piece of wire such as an electric lamp, exceedingly rapid vibrations or oscillations are set up, which communicate themselves in the form of impulses with the surrounding ether in a similar way to that in which water is disturbed in a pond when a stone is thrown into it. All these impulses travel away from their source of origin at the same speed, that is roughly 180,000 miles per second. If therefore, the number of vibrations be 180,000 per second, one impulse would have travelled a mile when the next impulse started, and each following impulse would follow at the same rate. These waves or radiations would, therefore, be one mile in length. The vibrations are, however, very much more rapid, so that one impulse can

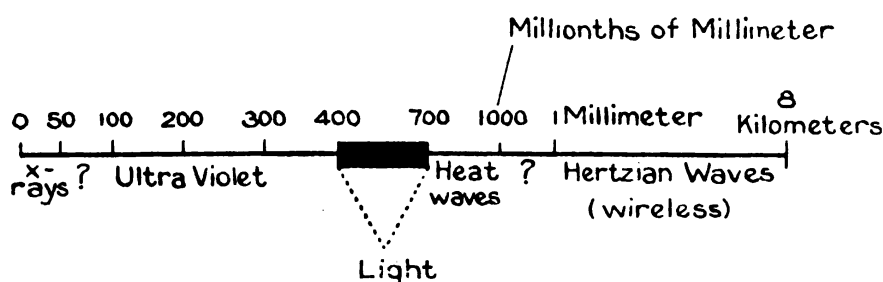


FIG. 1.—Full spectrum.

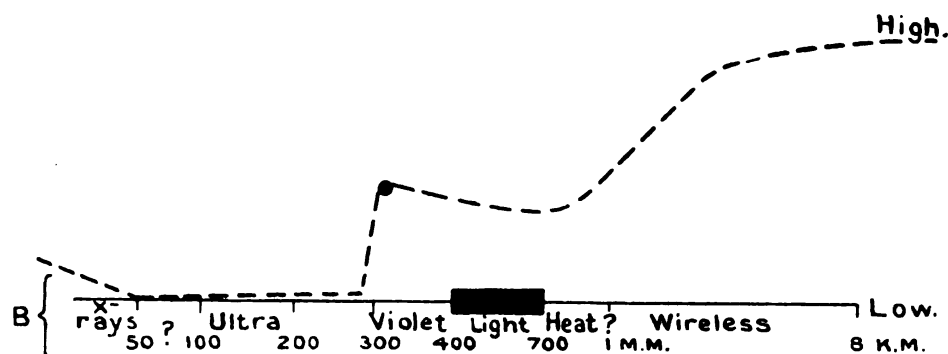


FIG. 2.—Penetration of full spectrum.

only proceed  $\frac{500}{100000000}$  mm. (five hundred millionths of a millimetre) before being followed by the next. These waves are therefore five hundred millionths of a millimetre in length. The human eye is susceptible to vibrations round about this wave length, and we call it light.

Newton did not know, as we know now, that at each end of the spectrum there exist, if we had the means to detect them, radiations of infinitely lesser and greater length and up to the present time we have been able greatly to extend this spectrum from waves of miles in length on one hand, to those of infinite smallness, on the other (fig. 1).

It is exceedingly interesting in regard to the penetrative properties of these radiations to compare the whole range (fig. 2) because the *limitations* of the

ultra-violet in the matter of penetration are of great importance to us in regard to their use for treatment. Thus, the long Hertzian or wireless waves penetrate most things to a great degree; the infra-red, or heat waves penetrate also in a lesser and varying degree both metals and glass and other substances. The light waves penetrate substances such as glass, quartz, and many liquids, and we call these substances "transparent." The shorter ultra-violet are the least penetrative of all, then we come to the X-rays which penetrate all substances according to their atomic weight. Now we come in detail to the ultra-violet. I have purposely, as we cannot in any way call ourselves a physical society, referred to the length of these waves in millimetres or fractions of them, but science has given other units such as micron and Angström units to denote their infinite smallness. I will, however, for the sake of clearness stick to millimetres and speak in millionths of a millimetre. This is equivalent to a millimicron. The length of the shortest visible wave—e.g., violet light—is roughly 400. Shorter than this, we can divide into three regions (fig. 3).

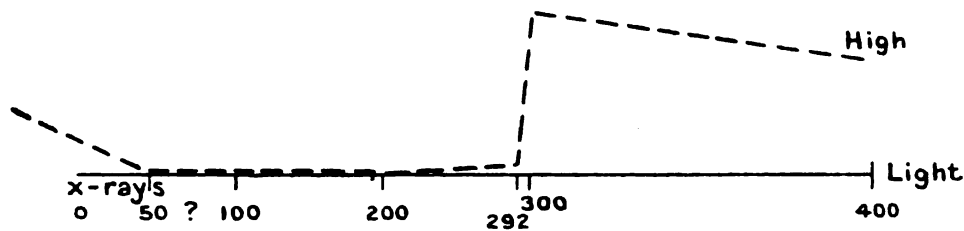


FIG. 3.—Spectrum of ultra-violet rays with penetration, also X-rays.

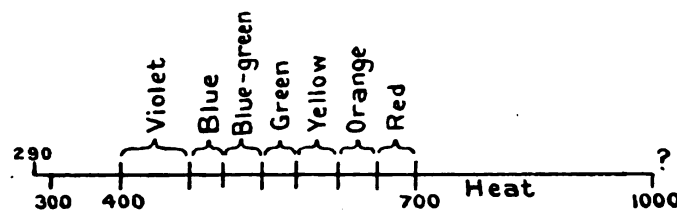


FIG. 4.—Spectrum of sun's rays.

*Extreme region* 0–200; this includes radium rays, X-rays, and extreme ultra-violet.

*Middle region* 200–300.

*Near region* 300–400.

The first important point to consider is the penetration of these rays. On this slide we see that the penetrations, which between 300 and 400 are fairly high, drop very rapidly at less than 300, and at under 200 are almost nil.

Glass will only pass rays longer than 340; quartz 185; fluorite 120. Hence the use of quartz lens for focussing shorter wave lengths. When we get to the X-ray, however, the penetration slowly rises until with the very hard or highly penetrative X-ray, the penetration is very considerable. The question of penetration is a most important one, as it is obvious that unless the radiations reach the site of the disease in sufficient quantity, no *direct* beneficial result can accrue.

I would like to emphasize the word "direct" here, as it is highly probable that the results obtained are in the majority indirect. Some interesting experiments have been lately tried on rats, when it was found that ultra-violet radiation had a similar effect on these animals to the administration of vitamins. Also, in the treatment of rickets the whole trunk is exposed to the radiations, the majority of which cannot penetrate to any great distance, hence the assumption that the main action of these radiations may be general.

The next point of importance is the effect of the radiations on living matter. It is well known that sunlight possesses germ-destroying properties; these properties are largely due to the presence of ultra-violet rays. The spectrum of sun's rays reaches from, roughly, 295, to infra red, well over 1,000 (fig. 4).

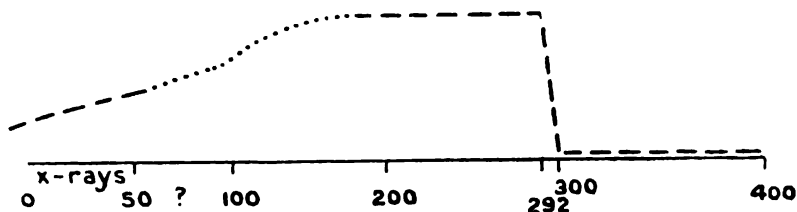


FIG. 5.—Diagram of germicidal properties of ultra-violet rays and X-rays.

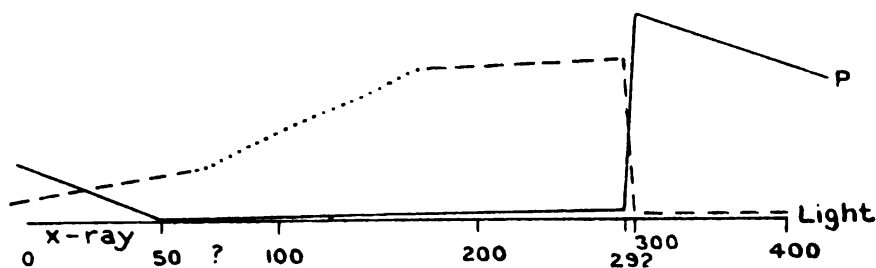


FIG. 6.—Diagram of penetration and germicidal properties, X-rays and ultra-violet.

Now experiments have shown that in waves longer than 300 there are practically no germicidal properties, so that all the effective germicidal work is done from this source on a very short wave band. The sun itself, of course, gives out ultra-violet waves of a much shorter length, but these waves by a merciful dispensation of Providence, are absorbed before they reach us in the outer layers of the atmosphere.

It is interesting to give the modern scientific explanation of that. The waves shorter than 295 act upon oxygen and produce ozone, so that when first the sun's rays reach our atmosphere, ozone is formed in some quantity. At the same time ozone has the property of absorbing the radiations less than 295, hence only those above that figure reach the earth. Below 295 the ultra-violet rays are powerfully germicidal (fig. 5).

Shorter than 200—down to 60—the radiations have not been much explored, but below 50 they become X-rays, and then, as we have seen, become more

penetrative, and still possess germicidal qualities. These properties, together with their great penetration, make the X-rays so dangerous to handle. To recapitulate the facts at which we have arrived, we see that while the longer ultra-violet radiations penetrate to a marked degree, they have no harmful effects on bacteria, and that it is only when the penetration becomes almost nil that the effect on bacteria becomes marked. When we get down to X-rays, however, we get both penetrative and bactericidal properties (fig. 6).

Passing now to the high-frequency current which is perhaps used far more at the present time than ultra-violet radiation, we are not, strictly speaking, dealing with ultra-violet radiation at all, in spite of the fact that it is popularly referred to as "violet ray."

This apparatus, while possibly producing a *very* small quantity of ultra-violet radiation, in the main does its work by producing an electric current of very high voltage and exceedingly low amperage, so that the current leaves the

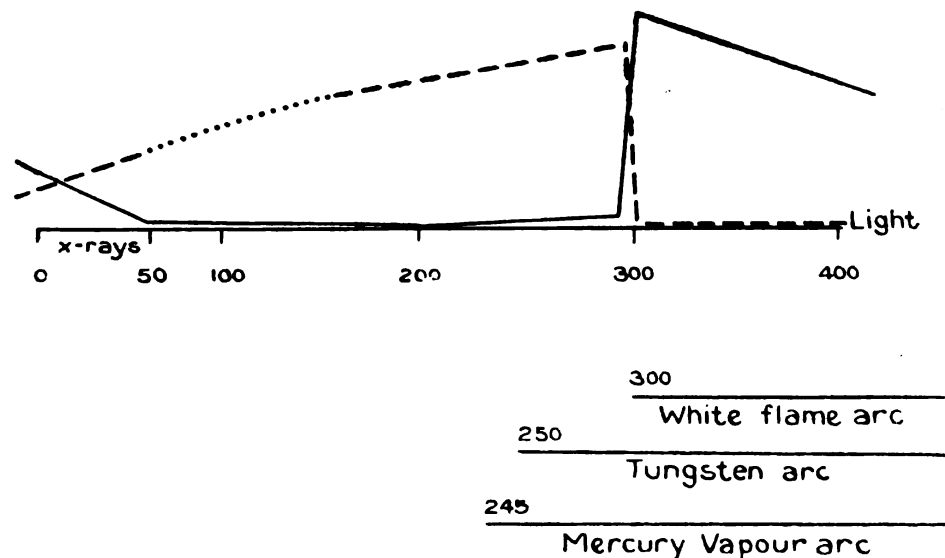


FIG. 7.—Diagram of extent of spectrum of three lamps.

end of the vacuum tube electrode in the form of a "brush" discharge. This discharge enters into the tissues and is said to carry with it the medicament which is placed in its path. This, however, is very unlikely. It is sometimes used without any intervening pad; in that case, the spark either acts as an electrocuting agent or by the formation of ozone in the tissues, which has an oxidation effect. It seems, however, quite likely that an electric discharge produced in a partial vacuum must produce a small quantity of both short ultra-violet rays or even soft X-rays, which would have definite germicidal effects, but the quantity would be very small indeed. Its effects, however, are very little known, and it is impossible to dogmatize about them.

The three main types of lamps for producing ultra-violet radiation are the following:—



(i) *The Carbon Arc Lamp*.—In this lamp the radiations are produced by an electric arc between two carbon rods. The current used is about 6 or more amps. If ordinary carbon rods are used, although the majority of the radiations are above 300 and therefore approximate to sunlight, there are a small quantity of the shorter rays. If, however, carbons are used which are impregnated by various substances, the radiations are materially altered, and in the lamp employed a good deal at present, by using carbons impregnated by the rare earths, a "white flame" arc is produced which gives radiations very rich in long ultra-violet radiation, but with none of the germicidal short rays.

(ii) *The Tungsten Arc Lamp*.—This is an arc produced between two tungsten rods, the lamp being kept cool by circulating water. This arc gives, in addition to the longer ultra-violet radiation, a good quantity of the shorter ones (250 to 300) which have very definite germicidal properties.

A lens of quartz is often added to this lamp for dental purposes which will concentrate the rays. Great care must be exercised in using this lamp to avoid injury to the eyes.

(iii) *The Mercury Vapour Arc*.—This lamp consists of a quartz tube or glass tube with a quartz window partly filled with mercury. By tilting the tube a spark is struck between the two terminals in it by the flowing mercury and an arc is formed. The ultra-violet radiations produced are from 240 upwards.

The quartz window has to be used as these shorter wave-lengths would be stopped by the glass; the current used in this lamp is upwards of 6 amps at 110 to 220 volts with a suitable resistance in circuit. Here again, great care must be taken to protect the eyes.

(iv) *X-rays*.—I need not go into detail about the production of X-rays as it is familiar to you.

By the use of a blunt focus Coolidge tube and a suitable current generator, these rays can be produced in any degree of penetration and their dosage worked out accurately.

(v) *High-frequency Apparatus*.—The high-frequency apparatus is also one with which you are acquainted. In this case a high tension current is produced by means of an induction coil. This in turn, by means of a suitable condenser and spark gap, is converted into high-frequency current which is led into a vacuum tube and so applied to the gum.

The following conclusions may be stated:—

(1) In cases of pyorrhœa, owing to the fact that the bacteria are usually deep-seated, the use of the ultra-violet short waves, as a bactericidal implement, is of little value owing to their lack of penetration.

(2) We must therefore rely on the general effects of the radiation, as is done in the treatment of rickets.

This paper is merely intended to point out the properties and limitations so far as we know them.

In conclusion, I must express my indebtedness to Mr. Schall for his assistance, both with advice and loan of an apparatus, and also to Mr. Lucish, whose recently published book is of the greatest assistance to those who are interested in this subject, which is in its infancy, and about which so little is known.

(II) Mr. DOUBLEDAY.

I fully agree with those who believe that in cases in which general infection is shown to be due to mouth sepsis, extraction must follow. But I also believe in the value of a patient's own dentition as compared with any artificial one. If this position is accepted, it logically follows that we should examine and comment upon any treatment which may help us to check the early stages of mouth infection.

When Mr. Spain asked me to make experiments in order to ascertain the utility of these new forms of electro- and radio-therapy which he has brought before us to-night, I carried out the following experiments:

(1) A mixed growth of *Micrococcus catarrhalis*, *Staphylococcus*, and *Micrococcus tetragenus* from cultures taken from pyorrhœa pockets was incubated in broth for twenty hours, and then plated on agar, on Petri dishes. In the first series of experiments the white flame carbon arc only was used. These cultures were exposed for twenty minutes, at a distance of 20 inches, no concentration of rays being used. There was no difference in the resulting growth after forty-eight hours' incubation. The exposed culture grew as freely as the control.

*Second Series of Experiments.*

Five agar Petri plates were inoculated with a mixed growth and incubated for twenty hours.

[Five plates.]

- (1) Control.
- (2 and 4) Were exposed to the tungsten arc at a distance of 6 in. for ten minutes; there was little inhibition of growth.
- (3) Was exposed to a "white flame" carbon arc at a distance of 18 in. for fifteen minutes. No inhibition of growth resulted.
- (5) Was exposed to soft X-rays at a distance of 8 in., for four minutes; there was no inhibition of growth.

*Third Series of Experiments.*

[Six plates.]

- (1) Control.
- (2) Thirty-five minutes' exposure. Distance 6 in. from the tungsten arc. Three amps and 220 volts. Slight inhibition of growth.
- (3) Thirty-five minutes' exposure, white flame carbon arc. Distance 18 in. from the lamp. 220 volts. Definite inhibition of growth.
- (4) Same as (1) except that the culture was covered with two thicknesses of gold-beater's skin. No inhibition of growth.
- (5) Mercury vapour arc fifteen minutes. Distance 18 in. Five amps. 220 volts. No inhibition of growth.
- (6) Ten milli-amps medium soft radiation from X-ray tube. Distance 8 in. Time fifteen minutes in one minute doses. No inhibition of growth.

[These experiments were illustrated by lantern slides.]

Taking the experiments as a whole we conclude that the methods of treatment inhibit the growth of organisms in varying degrees, but that they do not appear totally to kill them.

I desire to record my appreciation of the help which was received from Miss Butler in carrying out the bacteriological part of these experiments.

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SIR JOHN Y. W. MACALISTER

UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

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**VOLUME THE SEVENTEENTH**

SESSION 1923-24

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SECTION OF OPHTHALMOLOGY



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1924

## Section of Ophthalmology.

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## SECTIONS OF MEDICINE, NEUROLOGY, OPHTHAL- MOLOGY AND OTOTOLOGY.

(JOINT MEETING.)

**February 26, 1924.**

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Sir HUMPHREY ROLLESTON, K.C.B. (p. 1), Dr. GORDON HOLMES (p. 6),  
Mr. SYDNEY SCOTT (p. 10), Mr. J. HERBERT FISHER (p. 12), Sir JAMES  
DUNDAS GRANT, K.B.E. (p. 17).

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## Section of Ophthalmology.

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### Persistent Swelling of Conjunctivæ.

By J. F. CUNNINGHAM, F.R.C.S.

J. T., MALE, aged 42, by occupation a clerk. Served in France, 1916; trench fever, 1917; invalided home, went out again 1918. Thinks the swelling began about this time. Treated at Brighton 1920. The case was at first regarded as one of episcleritis.

A piece was removed and examined microscopically by Colonel Herbert, who has kindly informed me that the bulk of the tissue was made up of



lymphocytes. He suggests that possibly it may be the early stage of the condition which leads to hyaline (colloid) degeneration.

Patient was recently under the care of Mr. D. Heron, to whom I am indebted for report on the patient's general condition.

Results of general examination, negative; result of examination of nose, negative; Wassermann reaction, negative. Slight enlargement of submaxillary lymphatic glands.

## 2      Cunningham : *Persistent Swelling of Conjunctivæ*

The swellings are shown in the accompanying drawing (p. 1) by Mr. J. S. Monro. Both globes are rather prominent as compared with photographs taken some years ago, and there are limitations of upward movement, especially in the right eye.

### DISCUSSION.

Mr. F. A. JULER said he thought that proptosis was present in this case, and that hyaline degeneration such as had been suggested was an unlikely condition to find in the orbit. He inclined to the view that the condition was of a chronic inflammatory nature, which might be brought within the compass of the term lymphoma.

Mr. N. BISHOP HARMAN said he had wondered whether it would be of any use to try drainage with silk, in the manner carried out by Mr. Sampson Handley after amputation of the breast. In a case of severe conjunctival œdema following excision of the maxilla he had utilized this procedure; the silk drain had reduced the swelling.

Mr. A. L. WHITEHEAD (President) suggested that X-rays might reveal a block at the back of the orbits; it seemed like a case of chronic œdema following some such blockage.

Mr. MALCOLM HEPBURN thought that if there were a block at the back of the orbit, the œdema by this time would have become more general. Probably this was a case of brawny scleritis. He had had a case similar to this, but less extensive, in which the lower part also was involved. It was difficult to be positive about drug treatment in such rare conditions, but the patient in his case had been much benefited by the application of dionine.

### Case of Epibulbar Growth.

By F. A. JULER, F.R.C.S.

S. D., FEMALE, aged about 65, gave the history that a reddish spot had appeared on her right eye nine years previously, and that this had increased gradually since then. Three and a half years ago a piece of the tumour had been removed at a hospital.

The right eye shows a nearly hemispherical epibulbar growth with its centre opposite the outer part of the limbus. Inwards it extends to the centre of the cornea, to which its base is adherent; outwards it reaches some 6 mm. towards the outer canthus, in this region it involves the conjunctiva, but the greater portion is not adherent to the sclerotic. In colour the mass is dull red, its surface is smooth and not ulcerated, some enlarged vessels course towards it in the ocular conjunctiva from the outer quadrant. There is a scar over the right pre-auricular gland, which had been palpably enlarged. At the operation this gland was found to contain pus. Microscopical examination of a piece of the tumour showed evidences of malignancy, the cells being of carcinomatous type. Portions of the gland showed cells of the same type, in addition to inflammatory tissue.

### DISCUSSION.

Mr. E. TREACHER COLLINS said that glandular involvement in cases of primary epibulbar carcinoma was very rare. Without doubt excision of the eye was the best course in this case; it did not seem one suitable for merely excision of the growth and the subsequent application of radium. In early stages of carcinoma of the cornea it was possible to produce permanent cure by such treatment. In one case of his own in which it was carried out, the patient lived for a number of years without any recurrence, and death was due to a totally different malady. In the present case he recommended removal of the pre-auricular gland as much as possible, and then the insertion after a time of a tube of radium.

Mr. A. L. WHITEHEAD (President) said the experience of epibulbar carcinoma in the case of any one member must be very limited. He remembered seeing only three such cases. In two, he removed the eye, and exenterated the orbit, and in one he adopted the treatment Mr. Treacher Collins mentioned, and for some years afterwards that case remained free of recurrence. The old lady eventually died of chronic bronchitis. In those three cases there was no involvement of glands. He agreed that in the present case the eye should be removed, and the orbit exenterated. And as there seemed to be a recurrence in the pre-auricular gland, he would remove that too. If it could not be got away completely, he would apply radium. The margins of the lids should be removed.

Mr. T. HARRISON BUTLER said that fifteen years ago he had operated upon a case of epibulbar carcinoma; after that the man had been well for about eleven years, and then the growth had recurred. The patient neglected to report the recurrence for rather a long time, but he (Mr. Harrison Butler) removed it again. He came in a year with another recurrence, and as the condition was then somewhat outside the scope of an ophthalmic surgeon he (Mr. Harrison Butler) handed the case over to a general surgeon, who, when he operated, removed the margins of the lids, and sewed them together, leaving a small aperture at the inner aspect.

### **Case of Epibulbar Melanotic Sarcoma.**

By M. H. WHITING, M.B.

PATIENT, a female, aged 70. Five years ago she noticed a spot on the eye and this began to enlarge eighteen months ago. The right eye now shows a darkly pigmented tumour encircling the inner half of the limbus extending back towards the inner canthus for about 10 mm. and raised 3-4 mm. above the level of the sclera. The surface is irregular. There is slight lens opacity. The fundus appears normal. Vision, with glasses,  $\frac{6}{24}$ . Left eye appears normal. Vision, with glasses,  $\frac{6}{8}$ . An interesting point is that there are two pigmented patches in the conjunctiva of the upper lid opposite the growth, and probably quite independent of it.

There seems to be a tendency to pigmentation in the whole of the conjunctival tract. Whatever the pathological explanation may be, it is clear that the whole of the conjunctival tract should be removed at the operation; and I think that exenteration and removal of the margins of the lids are indicated. I shall be glad to hear of any other suggestions.

#### **DISCUSSION.**

Mr. A. L. WHITEHEAD (President) said he agreed that the orbit should be completely exenterated, the margins of the lids removed, and, if necessary, such parts of the lids as seemed to be involved. He asked whether there were pigmented moles in other parts of the body.

Mr. WHITING replied that he had not searched for other pigmented moles in the patient.

### **Case of Traumatic Arterio-venous Lesion of the Orbit.**

By N. BISHOP HARMAN, F.R.C.S.

F. W., A BOY, aged 12, was sent to me in July last by Dr. Flett, my colleague at the Radiological Department of the West London Hospital, on account of proptosis of the left eye.

The patient stated that he was looking through a fence eighteen months

#### 4 Harman: *Traumatic Arterio-venous Lesion of the Orbit*

previously when another boy poked something in his eye. He thought it was a piece of wire. The eye became bloodshot a few days later. His mother states that it was very red when she saw the eye fourteen days after the injury.

The eye gradually became prominent; it is thought the prominence is increasing. The boy complains of a noise like the hissing of a steam engine going on continuously. It prevents his getting to sleep.

The left eye is some 5 mm. in advance of the right. The lids will cover the globe. The conjunctiva is not injected. The movements of the globe are slightly less full than the right. Retinoscopy shows 1D of Hy. in each eye. Vision in each is  $\frac{5}{6}$ , and with + 1D sphere  $\frac{6}{8}$  slowly. Muscle balance is good, there is a very slight amount of hyperphoria and one degree of esophoria. The media are normal. Fundi normal, left physiological cup deeper than the right. The fields show that the left blind spot is a very little larger than the right, and the periphery to 2 mm. red, a little larger for the right eye than the left; the differences are small.

Pressure upon the eye with an elastic pad causes it to recede into the orbit, but not to the same depth as the right eye. Pressure cannot be kept up for long, since it causes pain. The whiff synchronous with each pulse beat can be heard distinctly and constantly with the stethoscope or when the ear is near the boy's head. It is checked by pressure on the common carotid artery of that side. There is an occasional slight thrill on palpation. The X-ray photographs of the skull are normal.

October, 1923.—Patient has been admitted to the West London Hospital with a view to the common carotid artery being tied by my colleague Mr. Oswald Addison. The general state of the eye is unchanged. There is some injection of the perforating ciliary vessels; the eye can still be covered by the lids, and pressed back into the orbit. The bruit and the thrill are both more pronounced and are spread over a wide area. A second set of X-ray photographs, taken by Dr. Flett, showed no changes in the bones of the orbit.

Mr. Addison and myself examined the boy together, and we agreed that though pressure upon the common carotid stopped the sound and thrill, yet we thought that on occasions a minimal persistence of the sound could be detected with the stethoscope over the temple even when the firmest pressure was made upon the common carotid.

Dr. Sydney Owen has supplied the following report on the vascular conditions:—

“As you noted, the long systolic humming-top bruit can be heard over a wide area, i.e., the whole of the bony skull, right and left side; down the left neck very markedly, and also to a lesser extent, over the right carotid. It is very well heard in both axillæ and down the spine behind for a short distance. There is a well-marked thrill over the orbital area on the left. It is very well-marked in the vessels of the neck on the left side and especially over the subclavian area on that side. There is a “border-line” enlargement of the heart, i.e., the outer limit of the heart's impulse, as the boy stands, is in the nipple line. There is no valve disease. There is an inconspicuous systolic bruit over the cardiac area which may be a faint conduction of the bruit generated above, but it is very short and some may deny its existence altogether. There is no diastolic murmur in any posture. In my experience (and I have seen a good number of these traumatic arterio-venous lesions since the war in various situations of the body) this slight, or border-line enlargement of the heart is quite common. I do not pretend to explain it. His blood-pressure is within the normal range, i.e., 90 systolic, with a diastolic of 70. I could find no signs of moment in his lungs, central nervous system, or in the abdomen.”

## DISCUSSION.

Mr. BASIL LANG said that he had seen this patient some three months after the injury. At that time the proptosis was less marked than at present. No bruit was to be heard and therefore the orbit was examined by X-rays. Dr. Finzi could find no abnormality. In view of the fact that proptosis was sometimes found associated with ethmoidal sinusitis, his nose was examined by Mr. Douglas Harmer, but again nothing abnormal could be discovered. Some three months later the boy was seen again. At this date a definite bruit was to be heard. He (Mr. Lang) had not seen him again until this evening.

Mr. J. HERBERT FISHER thought that the wire which had been poked into the orbit had lacerated some comparatively small branch of an artery, and that there was an extravasation of blood into the orbital tissues. This had probably developed an envelope and had formed a traumatic aneurysm. The later onset of the bruit seemed to show there was direct communication between an aneurysmal sac and a vein; the aneurysm he thought must be primarily traumatic.

Mr. A. L. WHITEHEAD (President) said he had seen four of these cases, and in all of them he had ligatured the common carotid himself. Two of the cases were traumatic: one followed a fracture from a fall; one was due to injury by a pellet when the patient was rabbit shooting. The other two were syphilitic cases, in which there was, apparently, an ordinary syphilitic aneurysm, which ultimately burst through into the cavernous sinus. The result of tying the common carotid had been satisfactory and permanent in three of the cases. In the fourth case the bruit did not return, but there was some renewal of the proptosis. In all the cases the sight had been definitely impaired before the operation, and no restoration of sight followed the operation. The patient in Mr. Harman's case had vision  $\frac{2}{3}$ , and he (the President) would not hesitate to advise ligature of the common carotid.

Mr. R. FOSTER MOORE reminded Members that a discussion took place on this subject at the Ophthalmological Society's Congress last May. As a result of some remarks on the danger of hemiplegia ensuing from ligature of the large vessels, Mr. Wilfred Trotter said the only doubt he had was as to whether it was not best to ligature the internal and external carotid on both sides.

Sir JOHN PARSONS said that all cases of the kind which had occurred up till 1908 had been collected by De Schweinitz and Holloway, and in that collection it was very interesting to see the results of various modes of treatment. Practically every treatment seemed to have had its successes. The only fatal course was to tie both externals or both common carotids at the same time. Patients in whom this had been done died of cerebral anæmia. If both sides were tied, as could be done in the case of recurrence of proptosis and bruit, a considerable interval should elapse between the two operations, to allow of some collateral circulation being set up. The consensus of opinion was that it was better to tie the internal and the external than the common, though it was a more difficult operation. If anything operative was to be done, it was important to do it as soon as possible. In some of the cases in which there was recurrence the large veins in the lids and neighbourhood were tied and excised. The male cases were mainly traumatic, but the female cases were practically all syphilitic.

*P.S.*, November, 1923.—The common carotid has been tied with a successful result; the boy will be shown again later.—N. B. H.

## Bone-free Radiographs : an Advance in the Photography of Small Fragments in the Eye.

By T. HARRISON BUTLER, M.D.

MEMBERS know how difficult it is to get reliable radiograms of small metallic fragments in the interior of the eye, and that it is practically impossible to detect pieces of glass or stone. The reason is that the rays have to pass through the skull bones when the picture is taken in the ordinary way. Many years ago Sir James Mackenzie Davidson and others obtained bone-free photographs, but their method was not taken up and was apparently forgotten.

Professor Vogt, of Zürich, has resuscitated and elaborated the procedure, and by means of his method it is both possible and easy to obtain a clear picture of the smallest metallic fragment of bone, stone and lead-free glass, if situated in the anterior part of the eye.

I now exhibit a radiogram that I have taken of a metallic fragment in the eye. The picture shows the upper and lower lids, the cornea and the margin of the bony orbit. The metallic fragment stands out clearly just outside the limit of the bone-free area. This particular spicule was localized just behind the ciliary body by Sweet's apparatus. The eye was obviously septic and was excised. The fragment was found imbedded in a sclerosed mass of tissue in the vitreous. All attempts to remove it with giant magnets had failed.

The actual procedure is simple. An oval dental plate is pressed in between the eye and the internal margin of the orbit as far as the patient will permit without too much discomfort. The tube is located laterally on the outer side and somewhat behind. An exposure of three seconds with a soft tube is sufficient. A second exposure is made with another plate that is inserted above the eye, the tube being below. For this purpose we allow the patient to hang his head down over the edge of the table and bring the tube above him. With a little practice it is not difficult to hit off the correct position.

[Mr. Butler with the aid of the epidiascope exhibited some diagrams from Professor Vogt's paper on the subject. He stated that at the Birmingham Eye Hospital and at the Coventry Hospital he now used this method as a routine, and that he found it a real help.]

Mr. BERNARD CRIDLAND said he was able to speak in favour of this method, to which Mr. Butler had kindly drawn his attention some ten days ago. His skiagraphist, Mr. Dyas, had obtained good results in a case which had come under his care only three days ago, and although the finding as regards a foreign body was negative, the films obtained were very promising.

## Muscle Recession for Strabismus.

By T. HARRISON BUTLER, M.D.

I HOPE that you will regard the few remarks that I have to make upon this subject not so much as a paper but as an introduction to a discussion. We have first to answer the question: What are the objects of a squint operation? The most important object is a cosmetic one, to place the eyes in the normal position. The second is to help in the cure of an amblyopia, if present. I regard the prospect of the development of fusion as somewhat visionary, but

in a few cases the attainment of parallelism of the optic axes may result in the appearance of fusion.

We have many of us thought that amblyopia is a condition that develops in infancy, and that it can be cured only if treatment is begun early in life, before the child is five years old. Probably this is the main true, but there are exceptions. I have come across a few children who have become amblyopic after ten years of age, and some of these and of the earlier amblyopias have been cured by treatment begun in children well over five years old. I know at least one case which was definitely improved by a squint operation.

The squints upon which I operate are generally over  $35^{\circ}$ , and frequently  $45^{\circ}$ , and I beg that you will consider my remarks as referring in the main to such cases, and not to the lesser and more easily cured squints of low degree.

The age of choice for operation is eight: this is the period when children behave better than at any other. Older children are apt to be more imaginative, and younger have naturally less control, and are apt to be difficult to manage. I have, however, operated on children of five without a general anæsthetic and without trouble.

All my patients have been treated by the full correction of their hypermetropia for from one to three years, and an operation has been undertaken only when it has become obvious that spontaneous cure is unlikely.

As regards the cause of strabismus: Although I fully agree with Worth and others that the ultimate origin is a lack of the fusion faculty, and the actual cause is the presence of hypermetropia, yet I feel certain that in cases of long continued squint of high degree— $40$  and more—there is an actual anatomical change, and if such be the case we cannot expect cure without operation. In many cases in which I have operated I have found the external rectus thin and ribbon-like, and very feeble and lax; whereas the internus has been stout and tight, so much so that I have occasionally found it difficult to replace the strabismus hook by Prince's forceps. I shall be glad to hear whether Mr. Worth agrees with this view.

Now with regard to the methods to adopt: We can attack a convergent or a divergent squint by many methods. We can operate upon one eye or upon both. We can advance both lateral recti or attack both median recti. We can advance the rectus of one eye and tenotomize the internus, or perform many modifications of these methods. In a few cases we are forced to make use of all four muscles.

I am sure that many of my colleagues will agree with me that if it is possible to cure a squint by an operation upon one eye alone then it is advisable so to act. Often one eye is amblyopic, and if it should be lost as a result of the operation it will not be such a serious matter as it would be were the good eye to suffer damage. It has happened—I personally have knowledge of one such tragedy, although I have been fortunate myself—that a suture has perforated the sclera and has led to fatal iridocyclitis. In one of my own cases a very nasty peripheral corneal ulcer developed that gave me many anxious hours. These facts and the use of some common sense make it quite obvious to me that operations should, whenever possible, be reserved for the worst eye.

In the case of a squint, I think it is quite unusual that it can be cured by an operation upon one muscle. I have compiled the statistics of about 100 operations for strabismus, and I find that the combined effect of an advancement and a tenotomy is on average  $27^{\circ}$ . This being the case, it is clear that advancement alone will not effect more than this.



If we admit, and personally I have no doubt of it, that we may be faced with a situation in which the external rectus is unduly weak, the internal unusually strong, we must attack the strong muscle. If we merely advance the greatly weakened lateral rectus we are not going to the root of the trouble—the stout contracted median rectus. We are therefore thrown back upon tenotomy or a modification of it.

The later results of tenotomy, however carefully performed, are so bad that I regard it as an operation which should be given up. My school clinics afford me about sixty squint operations a year, and they are all followed up till the patients are aged 14. My tenotomies have been confined to the central tendon and have been followed by an anchor suture which should have prevented the muscle falling far back, and yet I have seen children, some of them eight years after the operation, who show definite lack of convergence, and not a few with divergence. I have seen eyes remain parallel for as long as eight years and then for no apparent reason diverge to such an extent that re-advancement has been called for.

Tenotomy being a dangerous operation in that it leaves weakness behind it, we are left with *muscle recession* as the only practical way of dealing with the internal rectus.

Some months ago I read a paper by Chalmers Jameson in the *Archives of Ophthalmology*, on "muscle recession." It is to be found in the number for September, 1922. I at once saw the value of the method, and from that time I have given up tenotomy and have performed recession on every case with or without attacking the external rectus. After I had done a few by Jameson's method I thought that I could simplify the operation without impairing its efficiency. [Mr. BUTLER here threw on the screen the diagrams contained in Jameson's paper, emphasizing the necessity for great care in passing the scleral sutures as described by Jameson.] The simplified operation is performed as follows :—

The eye is cocaineized and a few minims of codrenine or novocaine with adrenalin are injected over the muscles to be dealt with. Then either a semicircular incision is made round the caruncle as described by Jameson, or a horizontal incision from the limbus to the inner angle. The internal rectus is picked up with a short strabismus hook, and Prince's forceps introduced and clamped. The tendon is now separated from its attachment and the whole muscle gently freed from its surroundings.

The eye is grasped by the insertion of the tendon, and pulled outwards till the sclera is freely exposed. A double armed suture is now taken and one needle passed into the sclera to grasp a few fibres exactly as described by Jameson. The suture is middled and we now have two needles to pass through the end of the tendon. This is done and the tendon freed from the forceps. In the case of the horizontal section the two needles are passed through the conjunctiva well away from the wound edge so that when the suture is tied the conjunctiva comes to cover the muscle completely. One more suture closes the wound. In the case of the circular wound it is necessary to let the needles come out close to the semilunar fold: otherwise there will be some difficulty in closing the wound at the corneal aspect, for the outer edge would be anchored too near the caruncle. I am rather of the opinion that, although the horizontal incision gives a neater wound, the circular is more effective. The reason may be that in the case of the horizontal incision the Tenon's capsule is only incised and if it is in tension that tension is not relaxed and may be a factor in maintaining the eye in the convergent position. The

circular incision lets Tenon's capsule fall back, and there is then no question of it holding the eye inwards. It is most essential to follow Jameson's advice in the matter of passing the scleral sutures: never lose sight of the needle, and see that the suture is visible through the whole of its course in the sclera. The muscle is thrown back and, unlike advancement, there is no tension upon it. A few fibres of the sclera will hold it in position.

The operation that I have been performing for the past sixteen years has been Worth's advancement, mostly with tenotomy, and, looking back, I am far from satisfied with the results obtained. There have been divergences and lack of converging power, and not a few of the operations have been complete failures in the reverse sense; the strabismus has not been cured. Of the first 100, 50 per cent. were perfect, 35 per cent. improved, having about seven degrees of residual convergence. The remainder were failures. Recession has been far more successful, and I have had very few failures, and only in one case was there loss of converging power. Squints of under twenty-five degrees have been cured by recession alone, and the remainder by means of the combination with advancement or tucking.

I am not at all certain that the reefing operation is as good as advancement, but my series is not yet large enough to furnish reliable statistics, nor has time enough elapsed to enable me to make any definite pronouncement.

In using Bishop's tucker I at first employed buried catgut sutures, passing them as recommended by Dr. Burch (*Archives of Ophthalmology*, July, 1922). I have given up catgut because one of my surgical colleagues has had two deaths from tetanus, both from the same tube of catgut. I have heard of other similar fatalities, and so I consider that catgut is too dangerous to use. I now employ a silk suture on each side of the tuck which I pass as follows: a horizontal incision is made, and when the tendon is in the Bishop's tucker I enter a needle well back in the conjunctiva from without inwards, then pierce the tendon under the tucker, and bring the needle out alongside the point of entrance. The suture is now tied and the tuck secured above. A similar procedure on the other side secures the tuck below. The tucker is now removed, and the conjunctiva sutured over it.

In using the tucker one screws up about 4 mm. which should give a shortening of 8 mm. As a matter of fact the tendon stretches like elastic, and the actual effect is very far short of what might be anticipated.

#### DISCUSSION.

Mr. CLAUD WORTH said the chances of success of measures designed to cure amblyopia ex anopsia depended largely on the proportion of the child's life during which the eye had been totally excluded from vision. If the child had squinted constantly with one eye, so that that eye was, for all practical purposes, totally occluded from vision and the squint had lasted more than half the child's life, as a rule one would not be able to cure the amblyopia by strapping up the fixing eye. With regard to the age, one could assign 7 or 8 years as the maximum, although to that there were exceptions in the way of acquiring or curing amblyopia.

He was surprised to hear that Mr. Harrison Butler had found a large number of cases of squint in which the internal rectus was shortened and thickened. He (Mr. Worth) saw many cases of squint in a year, and often he saw cases in which the external recti were deficient in function; but he very rarely saw a case in which there was any evidence that the internal rectus was either shortened or too strong. That was particularly noticed when one came to operate on the cases: when the patient was under a general anæsthetic the eye could be rolled out without apparent tension.

There was such a condition as strabismus fixus, but he believed cases of it were very rare.

With regard to operating on both eyes, he agreed that if an eye was quite amblyopic, it was advisable, if one reasonably could operate, and could still get a good balance, to operate on that eye only. But, as a general rule, one obtained a better balance by dividing the effect between the two eyes.

Years ago he had tried a similar method of tenotomy to that now described by Mr. Harrison Butler; he had also tried tendon-lengthening. The operations were all tenotomies, and all tenotomies were bad, some worse than others. He had two reasons for that statement—a statement not due to prejudice. One reason was, that he very rarely met with a case in which he was satisfied that the muscle was too strong; therefore why weaken it? The other reason was, that if one had done tenotomy, no matter of what kind or by what name it was known, it made accuracy in operating impossible, as one did not know what the effect of that tenotomy would be. To anticipate the question which Mr. Butler would probably ask in his reply, "what was meant by accuracy?" he (Mr. Worth) would reply, not mathematical accuracy; there was no such thing as absolute accuracy outside mathematics. He meant accuracy to this extent: if the patient had any fusion sense, he would get binocular vision after the operation. If he had no fusion sense, the result of the operation might be considered accurate if the delicate mirror test failed to reveal any deviation. Anything less than that he would not consider was accuracy. And if tenotomy had been done, either alone or in combination with advancement, no one could be anything like certain of getting such a result, and he should not expect it. Tenotomy was a confession of failure. He was sure that nobody who could do an advancement with certainty and accuracy would ever want to do tenotomy again.

Mr. BISHOP HARMAN said he had studied Jameson's paper, and he failed to see that the method he advocated was safer than an ordinary tenotomy, which most of them rarely did. A good operator could cut a tendon without destroying the lateral expansions, and examination of the anatomy of the tendon would show that the tendon could not shrink back more than 4 mm. if the expansions were uninjured. Jameson deliberately cut all the expansions clean back so as completely to free the tendon from the globe; the tendon was detached from all connexions except the conjunctiva. Failure to obtain re-attachment of the tendon must result in serious deformity. The sutures were inserted into the thinnest part of the sclera, a procedure not unattended with danger. Further, the complete detachment of the tendon presented the risk that the tendon might not be re-attached in its true axis, with most disturbing effects. He regarded the operation as one of considerable difficulty, especially in children, in the case of whom the anatomical relations were so confined, that he thought the risk, in their instance at any rate, too great to be justified.

He did not agree that there were structural alterations in the internal rectus in those cases in which the external rectus appeared thin. In these cases he thought the external rectus was undeveloped, a condition that might be exemplified by the regular state of that muscle in the flat fishes. In shortening a thin external rectus one could not get all the effects desired, so that operation must be extended to the other eye.

In his reefing operation, and in other forms of tucking, he was sure success lay in thoroughly rasping the surfaces of the tendon to be folded, so as to get raw surfaces that would readily adhere. He had a school clinic of squinters to which cases requiring operation were passed. His regular practice was subconjunctival reefing (without advancement in most cases) and partial tenotomy (jigsaw) of the antagonist. All were operated upon as out-patients; there was no trouble, and the results were gratifying. All did not succeed, but most did. He always balanced the shortening of one tendon by the lengthening of the other. That an effective partial tenotomy was possible he was sure; he had often demonstrated the slender remaining strand which held the tendon after his jigsaw operation. The orthopædic surgeon dealt with tendons in much the same way; he slipped his tendons and obtained graduated results; so also could the ophthalmic surgeon.

He considered catgut a thoroughly bad material for subconjunctival sutures. He did four cases with such sutures in one week, and never another since. Catgut

needed absorption, therefore leucocytic infiltration, and a granuloma. In his cases he had excised the lumps to shorten the after-effects. The temporary silk suture was the correct material.

Mr. GRIFFITH said that he had been doing an operation like that described by Mr. Harrison Butler, during the last eighteen months. He gave up simple tenotomy a long time ago, as he was dissatisfied with the uncertain results, and also because he saw some late divergences. He then took to doing only advancements. The trouble with these, when there was a big squint, was that one had to do such a full advancement of each external rectus that there was sometimes undue fixity of the eye, and a loss of convergence. Eighteen months ago he did this calculated tenotomy with fixation of the muscle to the sclera. At that time he thought he was doing an original operation, but he later found that Mr. Brooksbank James had done it, and also Dr. Jameson, of Brooklyn. It was as scientific an operation as advancement; one saw where one was putting the muscle, and it was not necessary to go deeply with the needle; the slightest superficial fibres of the sclera were sufficient to hold it. When he had the patient on the table under cocaine, he seized the eye and abducted it, thus judging the amount which the internal rectus was resisting. If abduction was easy, he did an advancement; if not, he did the retirement of the internal rectus.

He thought Mr. Harrison Butler had not dwelt sufficiently on the difficulty of the operation. It was not as easy as advancement, because in old-standing squint, there was very little room; there was a shortening of muscle, of Tenon's capsule and of conjunctiva. Having dealt with muscle, in a long-standing squint, he did a plastic operation on capsule and conjunctiva, sewing up so that the vertical incision became a horizontal line. He intended to continue the operation, as he liked it.

Mr. HARRISON BUTLER (in reply) said that he was glad that he had obtained some support for recession.

Mr. Worth had said that there was no accuracy with a tenotomy. He agreed with this for he had seen tenotomies that had had no effect whatever, whereas an exactly similar operation had produced a divergence of  $45^{\circ}$ . The operation he had described differed materially from tenotomy. If it was a tenotomy, in the sense that Mr. Worth claimed, then advancement was equally a tenotomy, as in actual fact both were. Mr. Worth cut his tendon and sewed it to the eye farther forward; whereas he (Mr. Harrison Butler) cut his in exactly the same way, and sewed it to the eye farther back. If the tendon adhered where it was put both operations were equally accurate. As a matter of fact the advanced tendon did not always remain where it was put, but the recessed tendon did. Mr. Worth preferred to put his tendon say ten units forward; he put his five units forward and the antagonist five units back. The total effect was the same but he obtained a more equal distribution of labour. It was not correct to claim that one method was accurate and the other inaccurate.

With regard to results: the last fifteen tenotomies that he had performed showed muscle weakness in five instances. Most of them were apparently excellent cosmetic results, but investigation showed that five had convergence insufficiency. He had now performed recession about fifty times and only one case showed slight insufficiency. Mr. Bishop Harman rightly said that the sclera was very thin at the point selected for recession, about 5 mm. back from the original insertion, and that it was here dangerous to sew the tendon on to the sclera. He agreed, but he thought that a man who had not the requisite skill to do so safely should take up a less exacting branch of surgery. It was, as he had emphasized, necessary to take up only a few fibres. The sclera was very strong here and Jameson found that his sutures would support over three pounds before they tore out! There was still more danger in trying to get a good grip for the advancement sutures, for here the muscle was in tension, and if one did not obtain the requisite grip, the sutures would pull out on the third day, the muscle would fall back, with failure of the operation. His house surgeon told him that the reason why some of his advancements were failures was that he was not sufficiently brave in inserting the sutures. Shortly after he heard of a case in which the surgeon was really heroic in this direction, and the eye was lost!

He asked whether Mr. Bishop Harman had tried the operation himself. Before condemning an operation it was wise, unless it was obviously an absurd method, to try it outright, and then if the results were bad to condemn it.

Both Mr. Harman and Mr. Worth had said that there was no such thing as contraction of the internal rectus, although Mr. Worth agreed that the externus could be very feeble. He (the speaker) had had several cases in which the internus was quite obviously abnormally tight and very stout.

With regard to rasping the tuck: he had never done this, and yet the majority of his cases had been successful.

He agreed that recession was difficult, especially in children, and when working without a general anæsthetic. Operations on the inner aspect of the eye were always more painful than on the outer side. The child should be made to look towards the operator so that the muscle was relaxed. Pain was not generally complained of, but several children said that they had a headache while the operation was in progress.

## Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

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### Retinitis Pigmentosa (? Pseudo-).

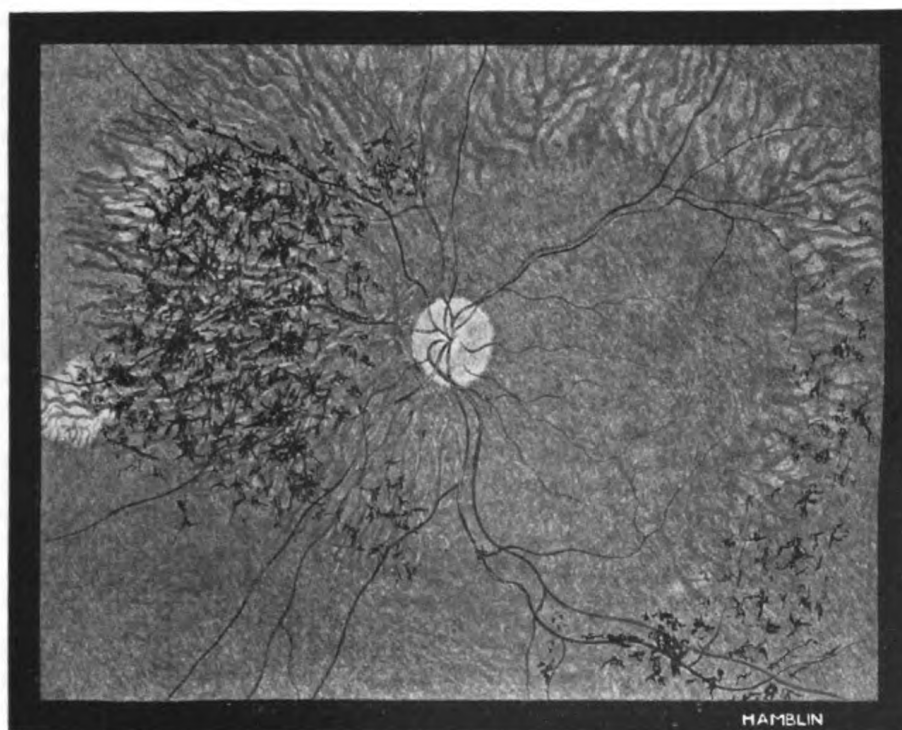
By H. M. JOSEPH, M.C., M.B.

PATIENT, a female, aged 63, first seen a month ago, complaining of headache. Has never noticed any defect of vision, or any night blindness.

The fundi (fig. 1) show the picture of retinitis pigmentosa and the fields (fig. 2, p. 14) the annular scotoma.

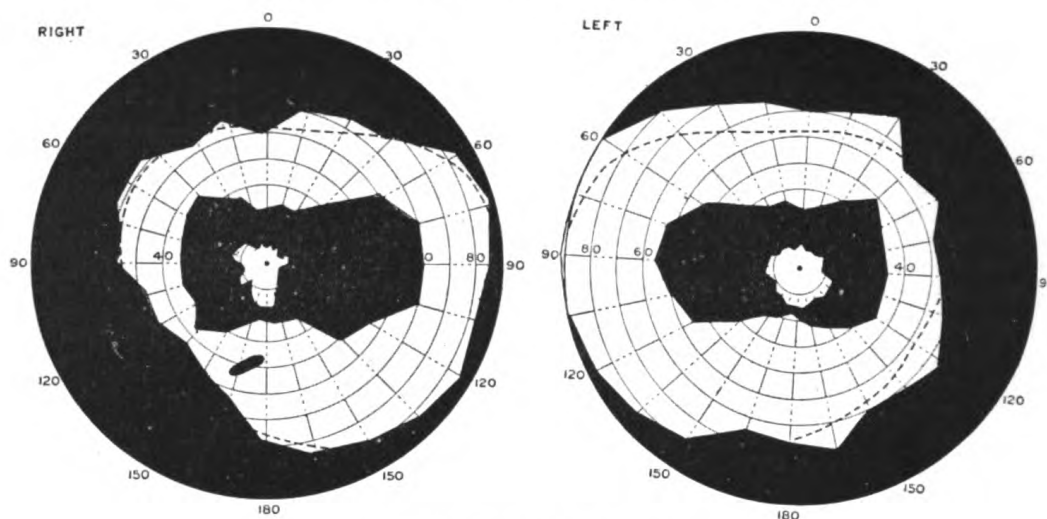
There is no night blindness—the vision is normal to Young's threshold test.

V. A.: right sph. + 2'5 =  $\frac{6}{9}$ ; left sph. + 2'5 =  $\frac{6}{9}$ .



Retinitis pigmentosa (? pseudo-).

FIG. 1.—Fundus oculi.



Retinitis pigmentosa (? pseudo-).

FIG. 2.—Visual fields.

### Case of Monocular Retinitis Pigmentosa.

By MONTAGUE L. HINE, M.D.

PATIENT, a male, aged 34. By occupation, a labourer. Duration of disease not known.

*History.*—Vision on enlistment, April, 1915. Right eye,  $\frac{6}{9}$ ; left eye,  $\frac{6}{6}$ . Wound over right frontal sinus (kick from mule), November, 1918. Vision failing since. The points of special interest in connexion with this case of monocular retinitis pigmentosa are the following: Wavy disc, atrophic arteries, but rather large masses of pigment. Post-polar cataract. Field contracted to  $5^{\circ}$  circle. Right vision  $\frac{3}{80}$  ( $\frac{6}{36}$  (11.)). Left vision,  $\frac{6}{8}$ . Left fundus normal. Full peripheral field. No ring scotoma.

#### DISCUSSION.

Mr. CYRIL WALKER, discussing Mr. Hine's case, said that years ago Mr. Jonathan Hutchinson, jun., was much interested in pigmentation of the choroid coming through into the retina as a late result in the concussion of the eyeball, where there was no evidence of rupture of the choroid or anything of that kind in the first instance. If Mr. Hutchinson published his results, it would be interesting to compare them with what was seen in Mr. Hine's case. He believed Mr. Hutchinson collected nothing closely resembling the present condition, but found cases in which the characteristic "bone-corpuscle" arrangement of pigment was developed as a late result of injuries, i.e., five or six months, or longer, afterwards.

Mr. ERNEST CLARKE asked whether the Section could not take a stand on terminology and get rid of the term "retinitis pigmentosa." He said that abroad, the condition was called "pigmentary atrophy of the retina." What had once been called keratitis punctata was now styled "K.P." (in inverted commas); why not call this "R.P.," also using inverted commas?

Mr. GRAY CLEGG said he had seen cases which he had called retinitis pigmentosa, in one eye, similar to this condition now shown by Mr. Hine. The condition here seen

did not seem to be a state which could be attributed to injury, either a direct blow on the eye or concussion. Mr. Joseph's case he did not regard as retinitis pigmentosa.

Mr. BASIL LANG asked whether the Wassermann reaction had been tested.

Mr. F. A. WILLIAMSON-NOBLE said he had seen one similar case, which was under Mr. Paton's care; he saw the patient while Mr. Paton was out of town. Mr. Paton had seen her several times, and he considered there was pigmentary degeneration in one eye. The other eye was normal, and had normal vision.

Mr. A. L. WHITEHEAD (President) said the exchange of experiences in this matter must be beneficial. His own experience was that migration of pigment was a very common occurrence in direct injury to the eye. It also occurred in "windage" cases, i.e., in which a shell had exploded close to the eye, but there had been no direct injury of the cornea; in these there was often a curious irregular migration of pigment. But he had never seen anything corresponding to what Mr. Hine's case showed. He regarded the "bone-corpuscle" arrangement as very characteristic of the true retinitis pigmentosa, which he had never seen in war cases; in the latter the disc underwent ordinary atrophic changes. But in this case the discs had the waxy appearance seen in true retinitis pigmentosa.

In the first case (Mr. Joseph's), the migration of pigment was unusual; there were peripheral areas free from pigment; he agreed it had not the distribution of true retinitis pigmentosa.

Mr. R. AFFLECK GREEVES said a condition of the kind shown might be due to an injury which affected the posterior ciliary artery the blood supply of which was interfered with. He understood by retinitis pigmentosa a condition in which the pigmentary changes in the fundus were of the fine "bone-corpuscle" variety, and not coarse and patchy as in Mr. Hine's case.

Mr. M. H. WHITING said he also saw Mr. Hine's patient at the Pensions Department, and he was of opinion that it looked like retinitis pigmentosa, but he did not proceed further. It certainly differed from all the cases he had seen with concussion changes in the retina. Sometimes concussion changes were slight, but if they were extensive in area pure white atrophic patches were practically always found as well. He never saw, either in France or in this country, concussion changes produce an appearance such as in Mr. Hine's case. And he did not know of anybody who had seen cases in which concussion changes were present as a result of "windage." He did not wish to say that Mr. Hine's case was retinitis pigmentosa, but it seemed indistinguishable from it.

Mr. HINE replied that the Wassermann reaction had not been tested in his patient.

### **Tuberculous Conjunctivitis, with Enlargement of the Pre-auricular Gland.**

By PHILIP DOYNE, F.R.C.S.

FEMALE child, aged 10, attended the casualty department at St. Thomas's Hospital two months ago on account of suppurating left pre-auricular glands. About this time a swelling was noticed in the left lower lid.

There is now to be seen a reddened indurated swelling, somewhat ill-defined, in the left lower lid on the conjunctival surface; in addition, there are cockscomb-like excrescences arising from the fold of conjunctiva at the lower fornix. There is a slight excrescence on the inner palpebral surface of the left upper lid. There is a scar with some induration on the site of the left pre-auricular gland and the upper left cervical glands are enlarged. There



## 16 Doyne: *Tuberculous Conjunctivitis* ; Lang: *Nose and Eyelids*

are no obvious physical signs of tuberculosis to be found elsewhere in the body. There is a family history of tuberculosis, a maternal aunt and grandmother having died of consumption.

### DISCUSSION.

Mr. ERNEST CLARKE said this child had very characteristic signs in the teeth, such as were seen in tuberculous children: large flat central incisors, and very much smaller, screwdriver-shaped lateral incisors. He did not know whether that was recognized as a common physical sign.

Mr. J. B. LAWFORD said he considered that, clinically, the appearances were very suggestive of tubercle; he had seen several very similar cases. He asked whether it was intended to have an X-ray examination made of the intrathoracic glands. A year or so ago there was published, in a French paper, a series of cases in which such examination was made in children who were suspected of being tuberculous. A series of control cases were also examined. In the case of the former valuable evidence was obtained, showing involvement of deep glands, even when superficial glands showed no signs of affection with tubercle. In all doubtful cases, this seemed to be a valuable additional method of diagnosis.

Mr. R. FOSTER MOORE said that in spite of the frequency of infection of the conjunctivæ by virulent organisms, his experience was that suppuration of the pre-auricular glands did not take place, and that it was only in tuberculous disease, as in this case, that the glands did break down. He would be interested to know whether that was in accordance with other Members' experience. With regard to Mr. Ernest Clarke's observation, it had to be borne in mind that the lateral upper incisors were normally very variable in size; they were always smaller than the central, were often very small, and not infrequently were entirely absent.

Mr. A. L. WHITEHEAD (President) said he had no doubt that this was a tuberculous case. With regard to Mr. Lawford's remarks, it might be remembered that when he (the President) gave his introductory address he mentioned the point to which Mr. Lawford had alluded, and quoted an experience gathered from a number of post-mortem examinations made in France, in which tuberculous disease of the thoracic and mesenteric glands was found to be very common in apparently otherwise healthy people. In the same address he also brought forward the point that in some of the cases of tuberculous keratitis and conjunctivitis, the primary cause was probably in the thoracic or mesenteric glands, when there was no obvious disease of the cervical or other glands.<sup>1</sup>

Mr. BASIL LANG remarked that Calmette had stated that tubercle was never primary in the lungs, and he represented some of the best views in France on the subject. Calmette said tubercle was acquired before the fifth year of life, and remained dormant in the thoracic or abdominal glands, and at a subsequent date, following upon mal-nutrition, became active once more. All patients with suspected tubercle should have their abdominal and thoracic glands examined.

## Case of Associated Movements of Nose and Eyelids.

By BASIL LANG, F.R.C.S.

PATIENT (R. T.), a female child, aged 8. The condition is congenital. When she blinks she twitches the end of her nose. When she screws up her eyes the skin over the end of her nose goes into rugæ.

<sup>1</sup> *Proceedings*, 1922-23, xvi (Sect. Ophth.), p. 5.

## DISCUSSION.

Dr. WORSTER-DROUGHT regarded the case as very interesting from the neurological standpoint. The dimpling of the nose only occurred when the patient *closed* her eye. She probably had some abnormally developed platysma fibres under the skin of her nose. When the seventh nerve was called into action to contract her orbicularis palpebrarum, her nose dimpled synchronously because of an unusually close association between the nerve fibres. One saw a similar phenomenon after recovery from facial paralysis; in nearly every case, when the patient closed the eye on the affected side, there was a simultaneous elevation of the angle of the mouth. It was, indeed, a good sign in ascertaining which side of the face had been paralysed.

Mr. D. LEIGHTON DAVIES said that in this case probably the muscular fibres of the pyramidalis nasi were highly developed and prolonged further downward than normally, and that might account for the occurrence.

Dr. WORSTER-DROUGHT, in reply to Mr. Leighton Davies, said that the dimpling seemed too superficial for the pyramidalis nasi to be at fault, as the contraction occurred immediately beneath the skin.

**A Pair of Diving Spectacles.**

By J. A. VALENTINE, M.D.

THE difficulty in seeing under water is partly due to suspended matter absorbing light, but also in a much greater degree to the change of refraction caused by the immersion of the cornea in a fluid of a refractivity almost equal to that of the cornea and aqueous humour of the eye (fig. 1).

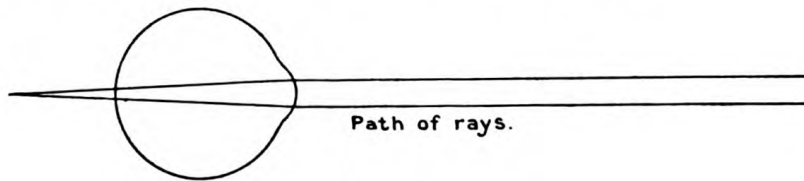


FIG. 1.—Immersed eye.

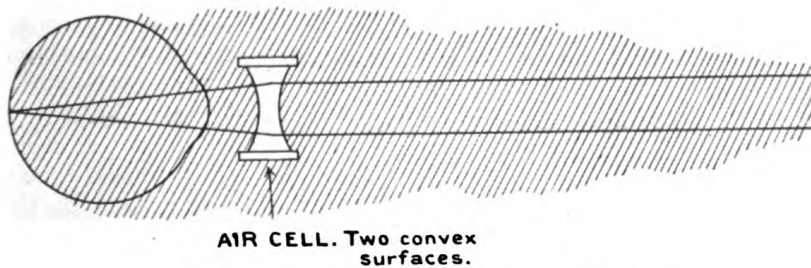


FIG. 2.—Immersed eye, with diving "glass."

This change is equivalent to 30 dioptries. That is to say, that under water the eye becomes hypermetropic to the extent of 30 dioptries.

To overcome this hypermetropia is practically impossible by means of ordinary glass, which has a refractive index of 1.5 approximately, which is so near that of water (1.33) that a sufficiently strong glass could not be made. The refractive index of diamond is about 2.5, which is amply sufficient, but

## 18 Valentine: *Diving Spectacles*; Rea: *Interstitial Keratitis*

this material has not been used in the pair of glasses which I am showing. Each "appliance"—for I cannot call it a glass—consists of a biconcave cell which contains air, and is hermetically sealed, so that when immersed no water can gain access, and it remains empty. The sides of the cell are composed of convexo-concave glass with parallel surfaces, and are so placed that the convex surfaces are on the inside, forming as it were a biconcave lens made of air, which has no refractive power till immersed (fig. 2). If one cup or concavity be filled with water, its convex curve has a value of 15 dioptries, so that together the two form, when filled with water, a lense of 30 dioptries.

Dr. RAYNER BATTEN said it was rather difficult to determine the visual acuity under water with test types. The hydrophthalmoscope (shown) enabled one to determine the visual acuity without putting one's head under water. He found experimentally the vision with the hydrophthalmoscope in place to be  $V. = \frac{1}{80}$ , or counting fingers at 1 ft. A + 30 D. lens in front of the hydrophthalmoscope restored vision to nearly normal allowing for the distortion of the high lens.

### A Preliminary Report on the Treatment of Interstitial Keratitis.

By R. LINDSAY REA, M.D., F.R.C.S.

(ABSTRACT.)

Mr. LINDSAY REA discussed, as a preliminary communication, the treatment of interstitial keratitis, based upon personal observation and study of ninety-one cases. Having paid tribute to former workers on this subject, such as Jonathan Hutchinson, Nettleship, Langendorff and others, he proceeded to relate his observations on the cases as they had presented themselves before him. He said he had never yet seen healthy temporary incisors in cases of children affected with interstitial keratitis under 7 years of age. This series showed the proportion of males to females as 1 : 1'87. He also pointed out that many forms of syphilitic disease made themselves manifest after an attack of interstitial keratitis. Six illustrations were mentioned, including gummata of the tibia, of the iris, and symptoms of general paralysis of the insane. The Wassermann reaction of the blood-serum had been done in sixty-four new cases and all were positive. In eight old cases the Wassermann reaction of the blood-serum was negative, of the cerebro-spinal fluid it was also negative, but in each case Lange's colloid gold test showed a luetic curve. Mr. Lindsay Rea described three modes of onset together with their appearance as seen by the corneal microscope and slit lamp. As to the course of the disease, he said the shortest time recorded for an eye to see  $\frac{1}{8}$  again after the onset of the attack was four months, the next, four and a half months; sometimes it required over a year. His treatment consisted in giving eight to twelve injections of novarsenobillon followed by mercury and iodides, or mercury alone in the case of children. If there was any sign of recurrence, as was seen in 3 per cent. of his cases, then a further series of injections were given.

Three charts were thrown on the screen, which illustrated the results of treatment. The first showed the results in the case of those who had never had injections of any kind, the second the results in those cases in which the treatment was not begun for several months, i.e., delayed treatment; while the third chart showed the results of prompt treatment. Of the latter, many

patients showed  $\frac{6}{8}$  and  $\frac{6}{9}$  vision. It was pointed out from one or two samples that a perfect result could be spoiled by the omission of the daily use of atropine.

#### DISCUSSION.

Mr. HUMPHREY NEAME said he was interested to hear of the six cases of later syphilis after the interstitial keratitis had cleared up; that was not a common occurrence.

As to the Wassermann reaction findings, he reported a series of cases in 1921. Of thirty-nine recent cases, thirty-eight gave a positive Wassermann. He did not agree with the statement that every old case of interstitial keratitis gave a negative Wassermann, or that even the majority of old cases did so; in the great majority it remained positive. The annularis type was not originally mentioned by Fuchs, but by Vossius. There was another type, which Igersheimer mentioned, which looked like a phlyctenule at the limbus. At the onset of interstitial keratitis there was often the development of a deep folding of Descemet's membrane, probably owing to oedema of the substantia propria, the latter swelling and taking the form of an in-folding of the deep surface of the cornea. He had seen interstitial keratitis begin two or three weeks after a blow which produced a black eye, and soon afterwards the condition developed in the fellow eye.

Reference had been made to an operation which determined the onset of an attack; there was a case at the Central London Ophthalmic Hospital, in which an operation was done for cataract extraction. There was complete healing of the operation wound, but within a week there was deep vascularization of the cornea and a faint diffuse haze. In the other eye faint, deep lines were seen in the cornea and a few very peripheral patches of choroiditis. Presumably the patient had had old interstitial keratitis with choroiditis, and the performance of the cataract extraction lighted up the disease in the operated eye.

There was one flaw in Mr. Rea's series of cases, namely, that the first sheet of old cases reported were all the accumulation of really bad cases for ten, fifteen or more years, which had received their treatment years ago and had cleared up more or less, but had left very defective vision. Those were cases in which the patients came back to hospital repeatedly and were the essence of all the severe cases drawn from a long period of time. These old cases could in no way be compared with a consecutive series of recent cases.

Mr. T. HARRISON BUTLER referred to the question of an accident initiating an attack of interstitial keratitis. He said that during the past year, since he wrote the paper quoted by Mr. Rea, he had not found a single instance in which the disease could be traced to an accident. If these cases were added to those he had used, the percentage would be less than the 20 per cent. he had found. That figure had been obtained from a study of all the cases seen at the Coventry Hospital during the past ten years. Perhaps 15 per cent. would be nearer the mark.

He had also had a case of recrudescence of interstitial keratitis after an extraction. The cornea began to cloud in the typical manner and ultimately the eye had to be removed. A careful examination of the other eye showed that it had suffered from interstitial keratitis. He was now treating a case in which the disease began some months after an operation for squint, but this might well be a coincidence.

Dr. WORSTER-DROUGHT said he agreed with Mr. Rea's statement that interstitial keratitis might not be the last manifestation of congenital syphilis. A patient under his care, in whom interstitial keratitis was noted two years ago, developed within a few weeks a popliteal aneurysm. Three weeks ago he was again seen with a complaint of "shooting pains in his legs;" the patient was then aged 23. He had sluggish pupillary reactions to light, had lost one knee-jerk and one ankle-jerk; the cerebro-spinal fluid yielded a positive Wassermann reaction with an increased globulin content and cell count. The case was now one of early tabes dorsalis due to congenital syphilis.

With regard to congenital syphilis in general, it was not unusual in his experience to find the blood Wassermann negative, but many of the cases showed changes in the

## 20 Rea: *Preliminary Report on Treatment of Interstitial Keratitis*

cerebro-spinal fluid. They were particularly liable to show a colloidal gold reaction curve of the "luetic" type. In conjunction with Dr. Roche Lynch and Dr. H. J. Fry, he had investigated a series of over a hundred cases of clinical neuro-syphilis; of these thirty-five showed a negative Wassermann in the cerebro-spinal fluid. Sixteen of these thirty-five cases yielded a positive Wassermann reaction in the blood and nineteen a negative. The nineteen negatives were mostly cases of tabes dorsalis, and the only pathological change that could be detected in the cerebro-spinal fluid was a luetic gold curve. Consequently this reaction appeared to be of very positive value; in many cases of clinical congenital syphilis it was found present.

With regard to treatment, as most cases of interstitial keratitis were met with in children, he urged the claims of sulfarsenol, which could be given subcutaneously; the results seemed to be quite as good as with novarsenobillon and other similar preparations.

Mr. J. F. CUNNINGHAM said it was necessary to wait for a considerable time before being able to say the other eye would not be affected; in his longest lasting case the interval was nineteen years. His own figures, not collected from this point of view of injury, were 2 per cent. after injury. Mr. Holmes Spicer's figures showed 3 per cent.

With regard to treatment, he was giving most of his cases novarsenobillon, and he thought it shortened the duration.

Mr. CYRIL WALKER said he concluded that most Members had seen cases in which they thought that injury was the determining factor; he believed that that was so with at least three of his own cases. But if injury was the determining cause, he wondered why a conjunctivitis, or other inflammatory condition in the neighbourhood of the cornea, might not also be a determining cause. He had had one such case of severe conjunctivitis during the war, when a Wassermann could not be taken, followed immediately by a typical interstitial keratitis. He wondered whether the case of phlyctenule followed by interstitial keratitis, mentioned by Mr. Neame, belonged to that category. He asked whether in some of Mr. Rea's cases there was a troublesome choroiditis, probably occurring when the cornea was inflamed, and persisting afterwards, so that although the cornea cleared a good deal, the sight got progressively worse.

Sir JOHN PARSONS said, in reference to Mr. Walker's remarks, that many years ago when doing Sir William Lister's work at the Great Ormond Street Children's Hospital, he saw a child whose case he labelled "phlyctenular conjunctivitis." Some time later, when again doing Sir William's work there, he saw the words struck out, and "interstitial keratitis" substituted. When he was on the staff at that hospital, he had observed cases of that type, and convinced himself that cases of interstitial keratitis sometimes began with phlyctenular conjunctivitis, in suitable subjects, i.e., congenital syphilitics, in the same way, he thought, as evidence showed, that injuries and foreign bodies and tenotomies set it up in suitable subjects.

Mr. CHARLES GOULDEN said he wished to mention two cases to show that a course of treatment with novarsenobillon did not prevent the patient from developing interstitial keratitis. The first case was that of a child who had come under notice with disseminated choroiditis and who had a prolonged course of treatment with novarsenobillon followed by mercury. That child subsequently developed interstitial keratitis. The other case was that of a young woman who had been under the care of Dr. Sequeira with periostitis of the tibia which had been diagnosed as due to congenital syphilis. The Wassermann test was positive. A complete course of novarsenobillon was given, in spite of which she afterwards developed interstitial keratitis.

Mr. HUMPHREY NEAME said that, after comparing the final visual results with the curve of Igersheimer, his (the speaker's) curve was found to be better. But he did not know whether Igersheimer took the same trouble to record the refractions. Possibly that authority's statistics were of cases called up and recorded without refraction observations.

Mr. A. L. WHITEHEAD (President) said that in the North there were a large number of these cases. For some years he had been in the habit of having all his cases

of the kind treated with galyl or similar injections, and he was satisfied the results were now better than formerly; he did not know whether that was because of the galyl treatment.

As Mr. Neame had said, there were many factors to be considered when preparing statistics. Owing to the increased intelligence of the population, and perhaps the spread of education, these cases were now seen at an earlier stage than formerly—a very important factor.

An important matter was the instillation of atropine to prevent iritic adhesions, and suitable treatment to improve the general health of the child; he always supplemented the special treatment by the administration of general tonics, such as iodide of iron. These were all factors which assisted in the restoration of vision.

The figures with regard to the acuity of vision were very difficult to criticize, because so many other factors came into play; but he was satisfied that in recent years his results in these cases were better than formerly.

Mr. LINDSAY REA (in reply) said that he had not intended that Tables I and III should be compared as Mr. Neame had suggested, but had merely meant to convey the impression that in not one single instance among the new cases enumerated on Table III had a result similar to any mentioned on Table I occurred.

## Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

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Mr. A. H. LEVY, F.R.C.S., showed a case of Unilateral Microphthalmos (Right Eye) associated with Coloboma of the Choroid in the Left Eye.

### Case of Injury of Right Upper Lid (Traumatic Ptosis).

By A. H. LEVY, F.R.C.S.

PATIENT, a boy, aged 7.

*History.*—Was knocked down by motor about three months ago, and received a cut across the right upper lid about 6 mm. above margin. This was treated locally and healed well. Two weeks afterwards a swelling appeared between the lids, which consisted of a conjunctival sac with the tarsal plate in front, and œdema. Under pressure bandage this swelling disappeared, and the condition as seen at present resulted.

Points of special interest are the following: The right lid droops, it can only be raised slightly by (?) action of the frontalis. When the upper lid is raised the tarsal plate becomes everted and the conjunctival fornix appears in the palpebral aperture; movements of globe full in all directions.

It would appear that all the fibres of the levator palpebræ superioris which are attached to the tarsal plate have been severed. The tarsal plate can easily be everted.

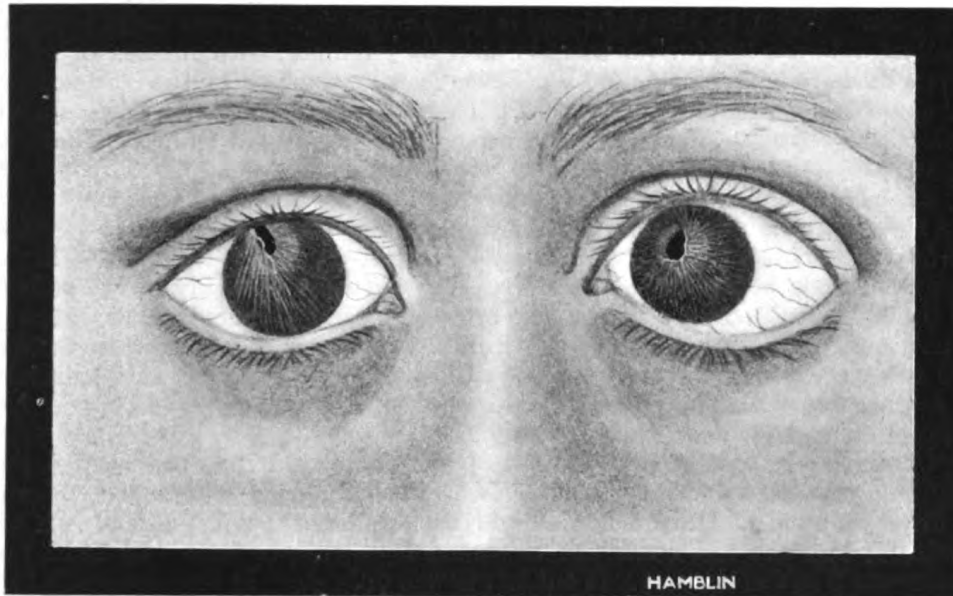
Mr. A. L. WHITEHEAD (President) said that a little time ago he had a very severe case, in which the whole upper lid was severed, and was hanging loose by a pedicle from the outer side. It was replaced and sutured, and a good union resulted. Complete ptosis remained, even after excision of redundant tissue on the inside and the outside, and he and his colleagues could not find any fibres of the levator to re-unite. Eventually, having got the lid into a good condition, he did one of the modified skin plastic operations, passing a tongue of skin from the upper lid under the eye-brow, with the idea of getting some attachment to the fibres of the occipito-frontalis. The result was a fairly good one. In the present case he thought it might be possible to dissect down and find some fibres of the levator and re-unite them.

[December 14, 1923.]

**Congenital Abnormalities.**

By R. CECIL DAVENPORT, M.B., B.S.

PATIENT, a small boy, aged 9. He is in Standard III in school, and there seems to be no intellectual defect. His parents are alive and well. There is another child, aged 7, whose eyes are said to be good, but the state of the heart is bad. The mother had three miscarriages before this child was born.



The only other gross congenital lesion one can see is a nævus. He is in hospital because of tuberculous lymphadenitis. There is some degree of buphthalmos and heterochromia of the irides, a persistent pupillary membrane in each eye, and congenitally deformed pupils. There is a complete coloboma up and out in the right eye, and a deformed pupil in the left, with a flap of iris folding over another portion of the iris. The fundi and media I regard as normal.

Dr RAYNER D. BATTEN showed a case of Infantile Interstitial Keratitis, in which the condition was noticed when the child was one day old.

**Cases of Exudative Retinitis.**

By HUMPHREY NEAME, F.R.C.S.

*Case I.*—Female, aged 48. She had retinitis circinata in her right eye. In November, 1922, the patient could not see well with her glasses, and there was pain in the back of the eyes; in the right eye there is typical retinitis



circinata, and a snow-white deposit extending from the disc. There were scattered small hæmorrhages over the area. The physician said the blood-pressure was raised, and the heart enlarged to the left. The urine is normal, and the blood Wassermann negative. In the upper and temporal region the artery was coated on each side with this white exudate, which seemed to be continuous with the spots of exudate in the so-called retinitis circinata, suggesting that the exudate was in the retina, as it was capable of lying on either side of the retinal artery.

*Case II.*—Male, aged 21, gave a history that in October last, while driving a cart, he discovered that his left eye was defective, through something getting into the other eye. He had had epistaxis, on and off, frequently, but none this year. Vision in the right eye is  $\frac{5}{6}$ . In the right macula there is some mottling. In the left fundus there is retinitis circinata, with masses of more or less discrete white spots, and a peculiar feature consists in the varicosities on some of the branch arteries; I have watched the inferior temporal varicosities change. There are also other little vascular swellings. There is apparently some abnormality of the left fovea.

*Case III.*—Male, aged 40. He was shown at the Section a year ago.<sup>1</sup> He had right retinitis circinata; the left eye is normal, except for angioid streaks. To-day the spots of white exudate are replaced by what look like delicate fibrous strands beneath the retinal vessels; they are probably in the retina. In the right eye there is a persistence of swelling in the macular region, but it is now only 1 D., while before it was from 2 to 4 D. It is whiter than before, and I suggest it is now mainly fibrous tissue, and that the mass at the macula is comparable with the fibrous masses which were shown by Coats in his description of so-called exudative retinitis.

*Case IV.*—In this next case, that of a woman aged 70, I have called the condition retinitis exudativa senilis. Twelve years ago she was under the care of Mr. Flemming, at University College Hospital, under the diagnosis of retinitis circinata, and she has continued to attend from time to time. In 1912 her vision was  $\frac{5}{6}$  in the right eye, in the left it was only  $\frac{1}{18}$ . It was decidedly bad in both eyes before 1920. In each macular region there is a mass of white, and in the right there are crystals, which are, presumably, cholesterin, in the periphery of the mass. I suppose that we may consider her condition twelve years ago was typical retinitis circinata, and now we have exudative retinitis coming into the mixed group, not properly Coats' disease. There is hæmorrhage, and a history of it, and I presume the cause in this case is the same as in retinitis circinata.

### Case of Exudative Retinitis.

By CYRIL WALKER, F.R.C.S.

I FIRST saw this patient two months ago, when he gave the history that his sight was good until about the beginning of June (1923). He first noticed the left eye was affected, then the right a fortnight later, and his sight rapidly became so bad that he could not read. He was admitted to hospital, and his heart, lungs and urine were found to be normal. Wassermann reaction strongly positive. There was no keratitis punctata, nor haze of the vitreous, and the fundus picture was bright, with the exception of the condition of the disc, which I will mention. He is the sixth in a family of ten, all of whom are

<sup>1</sup> *Proceedings*, 1922-23, xvi (Sect. Ophth.), p. 11.

alive and well, with the exception of one who was born five years after him, and who died at the age of a few weeks, though he seemed to be healthy at birth. His father is healthy, and there seems to be no syphilitic history in the family. The only thing to support the idea of syphilis is the positive Wassermann which I mentioned.

When first seen, the appearance suggested general diffuse choroiditis, with a white exudative choroiditis at the macular region, and I expected the usual haze of the vitreous to follow. Since then, however, I have "hedged" somewhat in the matter of diagnosis, because the swelling has slowly increased, it has become much more nodular, there has been no distinct haze of the vitreous, and the appearance has become more like massive exudative choroido-retinitis. There have been no hæmorrhages, nor traces of such. There is a suspicious-looking spot in the left eye, exactly in the fovea, which has not altered since I first saw the patient, and I do not think it is the remains of a hæmorrhage. The swelling is about 7 D. in each eye. There is a slight increase in the vessels, but they do not appear to have been altered, except secondarily. There is a fairly sharp demarcation between the white swelling and the general diffuse choroiditis; in some places it fades off; in others there is a sharply-defined margin. The white swelling comes quite up to the disc in both eyes and in fact slightly encroaches upon it. Both discs present a misty or veiled appearance.

There is very poor central vision in both eyes: right 1/60, left 1/60, with no central fixation. The patient has no night-blindness.

I shall be glad of suggestions as to what is the pathological condition, and also as to treatment. The patient has had four doses of novarsenobillon and mercurial inunction for about ten days.

Mr. R. AFFLECK GREEVES, F.R.C.S., showed a case of Exudative Retinitis in a boy, aged 14 ("Coats' disease").

### Case of Exudative Retinitis.

By J. F. CUNNINGHAM, F.R.C.S.

PATIENT, a female, aged 65. She has an enlarged heart and a mitral, systolic murmur. In the left macula there is a rounded whitish-yellow area, larger than the disc, which is slightly raised. In the right macula there are small hæmorrhages, and there are vitreous opacities in both. The Wassermann reaction is negative.

#### DISCUSSION.

Mr. J. B. LAWFORD said he had nothing to relate from his personal experience which was likely to be of value. His interest in the case of senile exudative retinitis was, that a paper had recently been published in France by two men who collected records of a considerable number of cases of exudative retinitis in old people and gave to them the name retinitis exudativa senilis. They had watched cases of the kind over a number of years and found their progress very slow, and the authors suggested that such cases should be placed in a group separate from those described by Coats and von Hippel and others. In that paper, too, there was a tabulation of the distinguishing features of the different groups of cases. He (the speaker) did not feel sure those French authors had made out a good case for the support of their contention; but in the senile cases there were features noticeably different from those in the younger cases described by Coats. One feature was that in the case of Coats' disease secondary glaucoma was a not infrequent result, but that had never occurred in the group described by the two Frenchmen; therefore there had been fewer opportunities of making a microscopical examination in the senile cases than in instances of Coats'

## 26 Cunningham: *Retinitis*; Williamson-Noble: (?) *Orbital Growth*

disease. The case shown by Mr. Neame seemed to agree in its principal features with the cases described by the French writers.

Mr. CYRIL DAVENPORT said he had collected five cases of exudative retinitis, every one of them bilateral to some extent. The patients all had vascular disease and very evident arterio-sclerosis with high blood-pressure. Two of them had for their years, however, a low blood-pressure, though they had diseased vessels. The French observers who had reported cases, however, said there was no evidence of vascular disease in any of these.

Mr. R. AFFLECK GREEVES suggested that Mr. Cyril Walker's case was one of active choroido-retinitis, affecting almost the whole choroid, a condition seldom seen in the active stage, though frequently seen in the atrophic stage when the whole choroid was studded with atrophic patches. The vitreous was filled with a hazy, dusty opacity, which was against the diagnosis of exudative retinitis.

Mr. A. L. WHITEHEAD (President) said he agreed with Mr. Greeves' view on Mr. Walker's case, as there was scarcely any healthy choroid to be seen. The important point to determine was as to whether it was a syphilitic, or a septic, infection. In the face of the strongly positive Wassermann, probably it was syphilitic, but very careful search should be made for a septic focus, considering the diffuse choroiditis, and the slight affection of vitreous as well.

Mr. HUMPHREY NEAME, F.R.C.S., showed: (1) Case of Annular Keratitis. (2) Section of a Microphthalmic Eye of a Pig.

Mr. M. W. B. OLIVER, M.B., exhibited a patient showing the result of a Plastic Operation on the Socket.

CASES shown by Mr. J. F. CUNNINGHAM, F.R.C.S.: (1) Persistent Swelling of Conjunctivæ shown previously at the October meeting of the Section.<sup>1</sup> (2) Old Peripheral Disseminated Choroiditis. (3) Macular Changes in Both Eyes. (4) Hæmorrhages in the Macular Region.

### Specimen from a Case of (?) Orbital Growth.

Shown by F. A. WILLIAMSON-NOBLE, F.R.C.S.

One of the specimens I have shown is that of the orbital contents in a case regarded as orbital growth; but on examination with the microscope, all one could find was a portion of muscle infiltrated with lymphocytes. I think it must be chronic inflammation. Mr. Treacher Collins thinks it may be a diffuse lymphoma.

#### DISCUSSION.

Mr. R. A. GREEVES said he thought Mr. Williamson-Noble's section had an organized hæmorrhage at the back of the eye, and it was very suggestive of sarcoma. He (Mr. Greeves) would have thought the case just mentioned by Mr. Williamson-Noble was one of chronic inflammation; he did not think it had quite the characters of a lymphoma; it was very diffuse in places—not in a mass, as was usual with lymphoma.

Mr. HUMPHREY NEAME agreed with Mr. Greeves that the condition was probably a chronic inflammatory one. In between the lymphocytes were some fibroblasts, he thought, and that would support the view.

Mr. WILLIAMSON-NOBLE (in reply) said that he thought it was a chronic inflammatory condition, but Mr. Treacher Collins said practically all the cells were of the same kind; very few plasma cells and few polymorphonuclears. The lymphocytes were mainly aggregated round the vessels.

<sup>1</sup> See *Proceedings*, 1923-24, xvii (Sect. Ophth.), p. 1.

## Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

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### A Case of Late Sympathetic Ophthalmia.

By Lieut.-Colonel H. HERBERT (I.M.S., Retired).

THE disease followed a trephining operation performed, without iridectomy, for congestive glaucoma in November, 1917; but more than five years elapsed before the other eye became involved, in January, 1923. The patient, a woman, was 62 years old at the time of the operation.

The operation failed to relieve the tension entirely, and the eye remained congested for more than a month. It then gave no trouble until about the time when the other eye was attacked; injection and tenderness then returned, as is usual in late sympathetic ophthalmia. The disease in the sympathizing eye was comparatively mild, and gave way, apparently completely, to three months' simple mercurial treatment, following the removal of the exciting eye.

These late cases of sympathetic disease have always excited considerable interest. The primary interest in this particular case lies in the fact that there is full histological evidence to show that the operated eye was only relatively quiet during the five quiet years. The patient was seen twice during the period, and the scanty notes mention only the development of a dense white pupillary opacity (the pupil remaining large), and the fact that the eye was blind. Actually there seems no room for doubt that the eye was the seat of a very slowly progressive infective process throughout.

The nodular character of the uveitis present was very noticeable to the naked eye when sections were being cut, the foci of greatest infiltration standing out as round white points in the iris, ciliary body, and choroid. For histological examination only the anterior half of the excised eye is available; vertical sections have been used. Taken with the clinical history the sections leave no doubt as to the nature of the disease; though, corresponding with the history of the case, there are unusual features: (1) The cytology of the case is somewhat different from the ordinary; (2) the infiltration has spread outside the uveal tract; and (3) here are some conditions indicating exceptional chronicity, probably requiring some years for their development.

(1) *The cell infiltration* is both diffused and localized. The diffused is very much more marked in the iris than elsewhere; in other parts of the uveal tract it is slight. It is made up chiefly of large cells, mostly plasma cells and their eosinophil derivatives—mononuclear granular eosinophil cells, and cells distended with Russell's bodies (hyalin spheres). The cells are noticeably largest where they are least numerous, i.e., in the ciliary body and choroid; and the highest proportion of eosinophil derivatives is found there.

The nodules present a strong contrast to the above, in that they are made up almost entirely of small lymphocytes. They thus differ from the nodules found in some published cases of sympathetic ophthalmia, in presenting no resemblance to tubercle, since they contain no epithelioid or giant cells. The only noticeable larger cells in most of the nodules are a few mononuclear eosinophils towards the periphery. It seems likely, therefore, that the collections of small cells are to be taken as the chief foci of the recent activity of the infective process, while the marked predominance of plasma cells among the diffused cells corresponds with the very low type of inflammation of the quiet years.

The largest nodules are in the iris, but they are the least defined, merging gradually into the surrounding infiltration; and they are compound, made up of two or more small collections. In the ciliary body and choroid, nodules are found almost entirely unconnected with diffused infiltration. One such is found in some sections within the trephine hole.

(2) As an indication, perhaps, that the unknown specific organism has become acclimatized *outside the uvea*, large-celled infiltration can be followed through the tissues filling the trephine hole, and through the sclera, along the anterior perforating blood-vessels above and below the cornea. It has spread into the conjunctiva and backwards immediately superficial to the sclera. There are some quite well-developed small-cell nodules near the limbus above the cornea, and there is some approach to the same conjunctival development even below the cornea.

Another extra-uveal site of activity is found beneath the thick pupillary membrane noticed, clinically, between the membrane and a thick capsular cataract, covering the whole of the front of the lens. Much of the laminated tissue forming this capsular cataract is old, containing no nuclei. The pupillary membrane, still largely cellular in structure, extends widely behind the iris, the retinal pigment layer of which is wanting in places. The membrane is in contact with portions of the lens capsule. But there are spaces beneath the membrane filled with round cells. And among these cells there are many typical giant cells, the only ones found in the eye. This cellular exudate has dissolved portions of the lens capsule, and, apparently also, of the underlying cataractous tissue, thus contributing to the marked irregularity of the anterior surface of this tissue. In places, the anterior lamellæ only of the capsule have disappeared. There is splitting of the capsule at the margins of the gaps in it.

(3) *Other advanced changes*: The detached retina is folded and thickened. The ciliary body and choroid are more or less detached from the sclera, and their layers separated widely. The inner surface of the choroid below merges into a thick layer of new fibrous tissue, containing masses of pigment granules, derived possibly from the retinal pigment layer, though mostly of much lighter colour than the normal melanin granules. The pigment has also wandered into the detached retina.

Another evidence of exceptional chronicity is found in thick laminated fibrous tissue behind the lower part of the cornea, extending upwards from the adherent iris. In some sections a thinner strip of tissue, more cellular, extends considerably higher; it has produced a folded detachment of Descemet's membrane. There is no recent punctate deposit.

As an interesting side issue, attention may be drawn to the detached non-pigmented epithelium of the orbiculus ciliaris. The separation has evidently been produced by bands which pass forward to the lens capsule, anterior and posterior, supplementing the normal zonular fibres, and perhaps

partly a product of the displaced epithelium. The point of special interest is the curious row of loops, projecting from the bases of the cells like bags of fluid, apparently one to each cell. That they are nothing very abnormal is shown by the fact that a somewhat similar row of vacuoles may be found at the bases of the normal undetached epithelial cells of some senile eyes. But, unaccompanied by any trace of vacuolation in other parts of the cells, they are certainly peculiar. They may have some significance with regard to the unsettled question of the normal attachment of the zonule. They seem to support other evidences that the normal attachment extends, in part, more deeply than the non-pigmented epithelium. In places fine lines can be traced from the loops up between the cells. These lines apparently extend to the fine membrane, covering normally the inner surface of the epithelium, from which the zonular fibres spring, and from which bulbous processes dip down in places between the epithelial cells.

The *cicatrix* supplies further evidence that the late activity of the disease was not due to late infection. There is no sign of drainage through the thick conjunctiva: and the trephine hole is almost filled with new tissue, mainly fibrous and fairly dense, derived mostly from the episcleral tissue. There is no more uveal impaction than is admitted to take place in the average trephine hole; and the small bit of iris lying in the deep portion of the hole is buried beneath much new tissue.

The question remains: Has the case any practical bearing upon the operative treatment of glaucoma? It may be taken that the eye was infected at the operation.

It is important that such occurrences as this should be made widely known. Some ophthalmologists seem to assume that the average trephining, i.e., without clinically visible uveal incarceration, is free from the danger of sympathetic ophthalmia; while they regard deliberate iris-inclusion as quite unjustifiable because of the supposed risk of the disease. This attitude does not appear to be supported by the available evidence. I have not heard of any instance of the disease following an intentional iris-inclusion operation for over twenty years. I believe that a correctly performed iris-inclusion operation is quite free from this risk, and from the risk of all serious ectogenous infective processes; but I hold that correct performance includes effective conjunctival antisepsis. I believe that the disease in the case now reported is attributable mainly to the want of conjunctival antisepsis.

#### DISCUSSION.

Mr. A. L. WHITEHEAD (President) said this paper raised many points of clinical and pathological interest. He thought that most of those who were in the habit of performing the trephine operation would join issue with Colonel Herbert when he said that in the majority of the cases there was incarceration of the iris.

Mr. RANSOM PICKARD said that three years ago he had a case which might possibly have been one of sympathetic infiltration after trephining. The eye did not do well, and about six weeks afterwards the patient developed keratitis punctata in the other eye. He had promptly removed the first eye, and the other eye recovered in a way unusual in sympathetic ophthalmia. That raised the point on which he desired to speak, namely, the proportion of cases in which no operation was done and yet serious inflammation arose in the other eye.

He had had two or three cases of chronic glaucoma affecting both eyes, in which suddenly, for no obvious reason, one eye developed a condition with severe irido-cyclitis leading to blindness, and its clinical appearance would pass for that of sympathetic

ophthalmia. He could not always microscope his specimens, therefore he could not bring forward that evidence. Certainly, however, there was a proportion of glaucoma cases in which that happened, more than in any other class of eye disease. In some cases of chronic glaucoma he thought the glaucoma was the expression of some very mild cyclitis without the ordinary symptoms of irido-cyclitis. He could have produced notes of two or three cases, in which there were the clinical symptoms similar to sympathetic ophthalmia though the other eye had had no operation done upon it. He had in his mind at least two cases in which some months after cataract extraction, that had gone on perfectly well, without iritis, irido-cyclitis suddenly occurred in the other eye, again of a severity which one usually associated with sympathetic ophthalmia.

Sir WILLIAM LISTER said that one point which Colonel Herbert brought forward, that of incarceration of the iris in cases of trephining, struck very hard at the basal technique of trephining. It was extremely important that the operator should endeavour to get no incarceration of iris whatever. The cases of late infection which occurred, he thought had been associated with some entanglement of iris in the wound. There were three different procedures by the employment of which such incarceration should be avoided. Directly the knuckle of iris appeared through the trephine hole, it was very important to take hold of the posterior portion of the knuckle and pull it up, so as to do irido-dialysis, pulling out sufficient iris so that when once cut the iris spread right away from the opening. It was very important to get away plenty of iris; and where there was a large pupil, one should aim at doing a complete iridectomy right through to the pupillary margin so as to avoid incarceration. Recently he had had the misfortune to see again a case, upon which he had operated for a colleague, in which there was a late infection, and in that case there was certainly entanglement of iris in the wound and a little tucking up of the pupillary margin. It was very important to keep the iris quite free of the wound.

Mr. E. TREACHER COLLINS asked whether Colonel Herbert had made serial sections through the whole length of the trephine hole. Unless he had done so he could not be quite sure there was no entanglement of the iris in the scar.

Colonel Herbert had given his view in regard to this case, but it was open to question whether the infection was not a late infection from without. In one of the sections shown there was a nodule of inflammatory infiltration in the conjunctiva round the trephine hole, and that might have been the source of the infection which had spread into the eye.

Mr. R. AFFLECK GREEVES suggested that one of the most important things in trephining was to split the cornea far forward. Sometimes the trephine was put in too far back, and that resulted in incarceration of the iris. He had cut sections of a series of eyes which had been trephined and which were failures, and the failure in each of those cases was due to incarceration of the iris, caused by the trephine having been put in too far back owing to the cornea not having been split far enough forward.

Mr. A. L. WHITEHEAD (President) agreed with Sir William Lister in regard to the importance of securing freedom of the iris from the wound.

Lieutenant-Colonel HERBERT (in reply) said that possibly he went too far in saying that the average trephine hole contained iris; but he merely accepted Colonel Elliot's admission in his recent treatise on glaucoma. He agreed that the small incarceration of iris present in this case probably had something to do with the development of the disease; but he desired to emphasize the point that the incarceration was only slight.<sup>1</sup> With regard to the nature of the disease, he thought there was no question that it was sympathetic ophthalmia. First there was the history—the operation, followed by congestion which recurred when the other eye became affected. Also the fact that

<sup>1</sup> POSTSCRIPT.—May not the incarceration merely have facilitated direct infection of the iris at the operation? Such infection in accidental impactions is often favoured also by exceptional uveal traumatism. On this account I am averse to the attempts at iris-replacement by a probe that have been mentioned. We have yet to account fully for infections, such as this, which contrast with the steadily accumulating experiences of the safety of properly guarded deliberate iris-inclusion.—H. H.

the condition in the other eye gave way to treatment after the removal of the exciting eye; and the histology was undoubtedly characteristic. The nodular uveitis was distinct from tuberculous or syphilitic uveitis, not only in the character of the cells, but also from the fact that there was no trace of caseation or breaking down of the centres of the nodules. In answer to Mr. Treacher Collins, every particle of the tissue of the trephine hole was made use of; and though the sections were not mounted serially, they were all examined carefully. With regard to the conjunctiva, the infiltration was exactly like that of the uvea, containing numerous plasma cells and their derivatives. It indicated chronicity, rather than a recent infection, and the same condition, though less advanced, was found below the cornea; and there, too, it was connected with the underlying uveal infiltration by means of cells around the perforating scleral vessels.

## The Practical Value of the Slit-Lamp.

By T. HARRISON BUTLER, M.D.

(ABSTRACT.)<sup>1</sup>

THE slit-lamp, in addition to its inherent advantages, enables us to employ our ordinary methods of examination with greater efficiency. This is due to the fact that it is possible to some extent to apply slit-lamp methods to oblique illumination with loup and condensing lens, and when an object has been seen under the high magnification of the corneal microscope it is easier to see it under a lower because we know exactly what to look for. It is explained in the paper how focal illumination can be attained by employing a 100 candle-power, gas-filled, half-watt lamp, at a distance of about eight feet, and focusing the light with an ordinary condensing lens upon the eye. The beam of light so obtained can be used exactly as the beam from the slit-lamp is used, and to a limited degree it confers the advantages of the focal light from this instrument. With such simple apparatus it is possible to make a rough localization of objects in the anterior segment of the eye, to estimate the depth of the anterior chamber, and to detect sclerosis in the lens nuclei. The cornea, iris, and lens, can be examined, not only by ordinary reflected light, but by light from behind—retro-illumination. Under favourable circumstances it is possible to see the blood circulating in the corneal and limbal vessels.

The slit-lamp was demonstrated and described, and the methods of examination mentioned. The works of reference dealing with the subject were noted, the most important being Vogt's atlas.

The appearances seen in inflammatory states of the eye are very important, for it is in this respect that the slit-lamp gives valuable information that cannot otherwise be obtained. Thus, the presence of cells in the aqueous is the first sign of sympathetic ophthalmitis, and their detection with the slit-lamp may give timely warning of the onset of this formidable disease and lead to early, and perhaps, successful treatment. On the other hand the fact that the fellow eye remains absolutely normal even under the high magnification of the corneal microscope may allow us to save an eye that would otherwise have to be removed.

The appearances seen in the cornea, the iris, the lens, and the vitreous were dealt with in turn.

<sup>1</sup> This paper, with the illustrations, was published in full in the *British Medical Journal*, May 31, 1924.



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The cornea can be examined in optical section, and conditions localized along the section by aid of a micrometer eyepiece with close exactitude. The nature of deposits upon the posterior surface can be determined, and in some cases great help obtained in the exact diagnosis of the disease which gives rise to them.

The normal aqueous is optically inactive, but when it contains albumin it lights up the beam; it becomes relucet. Particulate bodies become visible in this beam, and they stream along with the eddy currents of the aqueous. The iris is an interesting object under high magnification. Examined in lens-light by retro-illumination atrophy of the uveal layer shows itself by translucent areas. This method is especially useful in cases of atrophy of the periphery, the "collerette" of the French.

It is in the examination of the lens that the slit-lamp has had its greatest successes. Before its advent the structure of the lens could be deduced only from microscopical examination of the dead lens. So much new knowledge has now been gained that in this chapter of ophthalmology all the text-books are obsolete.

The slit-lamp shows that the lens has a most elaborate structure, and is divided into definite lamellæ. Starting from the anterior capsule, we find an appearance suggesting an internal capsule, the "cleavage-line" of Vogt, perhaps better called the duplication line. Then is reached the layer called by Vogt the cortex, followed by the adult nucleus, and the outer embryonic nucleus. Internally we find two plano-convex bodies, the internal embryonic nucleus. As we proceed backwards we note the reduplication of the anterior lamellæ. These dissociation areas are most distinctly marked off, both in section, and also when viewed in specular light. This method shows up the sutures characteristic of each nucleus, and of the cortex.

The recognition of these definite areas enables us not only to localize opacities in the lens but to a large extent to date them as regards the time they developed. Thus an opacity outside the embryonic nuclei cannot be congenital, for the adult nucleus and cortex had not been formed at birth.

The vitreous can be examined in a manner impossible without the aid of the slit-lamp. It is seen to be full of fibrils or membrane-like structures, in fact its constitution is of the most varied type. In some cases it seems to be almost homogeneous. A full examination of the vitreous can be carried out only with the aid of Koeppe's silver mirror, and a contact glass, which abolishes the corneal curvature. It is better to use a micro-arc lamp, and a special microscope with a single objective. These additions enable us to examine the whole of the vitreous and the retina by focal light, but this aspect of slit-lamp microscopy is almost outside the clinical sphere, and is useful only for research.

The slit-lamp is a valuable addition to our methods of examination, but it cannot take the place of the older instruments. It is essential that before an eye is examined with the slit-lamp it shall have been carefully tested with the ophthalmoscope and by oblique illumination.

Mr. BASIL GRAVES (introduced by Mr. Montague Hine) said that some time ago he heard Mr. Treacher Collins express the view that the slit-lamp would exert an influence on ophthalmology comparable to that of the ophthalmoscope. The uses of this apparatus were manifold. In every-day routine clinical work a case might be examined by means of the slit-lamp for minute clinical details with greater ease and confidence, and with the expenditure of less time and energy. Secondly, so many clinical features were revealed which could be detected or located or explained only by

the use of this apparatus. Vogt had said that the facilities afforded by the apparatus to detect prodromal manifestations of sympathizing ophthalmitis at a stage far earlier than had hitherto been possible was alone a sufficient justification for the possession of the apparatus.

With regard to research work with the slit-lamp he felt that in the present stage of development the methods along which such research work might be pursued fell under the choice of three main headings. There was, first, the indiscriminate slit-lamp examination of patients whose eyes were normal, and of others irrespective of the eye-condition from which they might be known to be suffering. For example, it could be said that the eyes of any old woman examined with the binocular microscope and slit-lamp revealed a museum. During the last week he had examined, at the Westminster Eye Hospital, five cases which had been kindly referred to him for that purpose; and, though he had been doing this work for some time, largely at the Royal London Ophthalmic Hospital, he was able, from the examination of these five cases alone, to derive twelve or fourteen comments on facts new to him, which he thought worth noting for future slit-lamp reports.

There was a second method of clinical investigation besides that of unselected patients, namely, in the study of special and selected patients who were known to be suffering from some regional affection, e.g., of the cornea or iris, or lens or vitreous; and for those who had access to many patients this method of investigation was one which would be very fruitful of results. A third method of investigation—one which would yield comparatively few results—could be useful to those who had access only to a small number of patients. It consisted in the study, over a long period, of the one individual patient known to be suffering from some given alterable condition, and the recording of the minute features seen with such accuracy that it was possible precisely to define, at each succeeding examination, the changes, often minute, which might have occurred since the previous examination of that patient. This method of examination called for laboriously accurate oft-repeated records needing much time for compilation. He did not advocate this study in the case of those who had a large number of patients from whom to make a selection. In the large eye clinics of London and the provincial towns there must be a wonderful field for slit-lamp research. No one person unhelped could do very much in this way, but there was much room for combined eye research with the slit-lamp. Recourse might be had to the team-system, in which the majority of members of any one team might be post-graduate students anxious to acquire experience in the use of the apparatus.

Mr. Harrison Butler's contribution raised the question of illustrating for purposes of publication. Every day he felt that the material was there if only one had the ability to record it pictorially. He had been interested in Dr. Rayner Batten's remarks at the Oxford Ophthalmological Congress in regard to the training of draftsmen for fundus illustrating. Much of what Dr. Batten said then was applicable, the speaker thought, to the training of artists for making slit-lamp illustrations, except that for this the training would take longer. He had tried training one amateur artist and one professional draughtsman, but he had given it up, as he had felt it would require possibly two years for him to train them in order to see the technical details in quite the same perspective as he (the speaker) saw them himself. He was therefore attempting to do his own illustrating. His results would at least be technically accurate, and he hoped to supplement what he was at present able to do by taking lessons in elementary painting; he did not think anything more than elementary painting was needed.

The answer to the criticism often made, that the use of the slit-lamp and binocular microscope took a long time to learn, was that much depended on the aim of the possessor. Detecting many of the important features early diagnostic of disease soon became self-evident to the possessor and user of his own apparatus. Help could be obtained from someone who was acquainted with the apparatus; but if teaching was to be done on a large scale, it required definite organization. When experience had been acquired, an unexplored field lay open to its possessor.

There were one or two precautions to be observed. He was an enthusiast for it, but one occasionally came across indications of the need for restraint or conservatism with regard to the attitude assumed towards certain aspects of the subject. While no limit

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was set by convention to the degree of magnification necessary before the term "microscopy" was justifiable, this term might be responsible for some of the liberal assertions sometimes made as to the possibilities of this work. The subject of the optical section was anatomical, not histological. To only one of the many new manifestations of this work was it, practically speaking, justifiable to apply the term "histology," and that was in observation of the endothelium of the cornea by specular reflection. Not long ago he saw an article by a writer who seemed inclined to regard every white speck seen floating in a pathological aqueous as being a leucocyte, and every pigmented speck as a pigmented cell. He (the speaker) had investigated these aqueous contents.

Another caution to be stated was the following: Enthusiasts for the work, while allotting to it its due share of importance, should be careful that it preserved its proper place and its proportional share in the daily scheme of the routine clinical examination. In a very good article giving a summary of the uses of the apparatus, he saw a statement to the following effect: "Now that we have the slit-lamp we are frequently able to understand, in a manner we could not before, why it is that a given patient, in whom we could see no reason by ordinary methods of examination why he could not see  $\frac{4}{5}$  or  $\frac{1}{2}$ , after correction, can only read  $\frac{1}{2}$ ." He (Mr. Graves) would say that if an ophthalmologist were fully competent in the use of his ordinary appliances, he would be well able, by means of such instruments as the direct electric ophthalmoscope, to detect the presence of minute imperfections causing obstruction to vision in the cornea, lens or vitreous. Having detected them, admittedly he would obtain a far better appreciation of their exact nature and situation if he then, with the slit-lamp, examined these features, of the presence of which he was already aware or of which he had at least some previous indication. For instance, the lens should not be examined by the slit-lamp unless it had previously been subjected to close ophthalmoscopic direct-method examination; otherwise the observer might fail to detect important changes, and his conception of the significance of changes, from the important point of view of their immediate effect on vision, might be erroneous.

## **Section of Ophthalmology.**

President—Mr. A. L. WHITEHEAD, M.B.

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Mr. A. D. GRIFFITH, F.R.C.S., showed a "Case of Tuberculosis arising in the Optic Nerve Head."

### **Case of Traumatic Luxatio Bulbi; Birth Injury, Forceps Delivery.**

By A. D. GRIFFITH, F.R.C.S.

MY case is that of a baby, one year old, who was brought to hospital four hours after birth. It was a forceps delivery for placenta prævia, and the right eye was luxated forwards out of the orbit. The conjunctiva was torn round the limbus, and the internal rectus was ruptured. There was subsequently ulceration of the cornea, but that healed up. The eye is divergent, and although it now adducts a little beyond the middle line, until recently it would not do so. I think there is a weak attachment of the rectus internus to the globe. The optic nerve shows simple atrophy, and the nerve is evidently ruptured.

Mr. MONTAGUE HINE, M.D., F.R.C.S., showed a case of Ethmoidal Mucocoele (for Miss Rosa Ford, M.B.).

### **A Modification of the Usual Method of removing the Lens in the Extraction of Senile Cataract.**

By BASIL LANG, F.R.C.S.

(ABSTRACT.)

IN order to facilitate the delivery of the lens in cataract extraction, I advocate the depression of the posterior (scleral) lip of the incision before pressure is applied to any other part of the globe. I find a lens scoop a very suitable instrument with which to apply the pressure. By pressing the scleral lip towards the centre of the globe, I drag on the iris, thereby pulling it away from the front of the oncoming lens. I suggest that, if pressure be applied in the usual manner at the lower part of the cornea, the iris is squeezed between the posterior surface of the oncoming lens and the sclera, and is damaged. I only apply pressure below at the moment that the lens is being borne through the pupil.

My claim is that the procedure enables simple extractions to be performed with little or no risk of subsequent prolapse of the iris. I attribute this absence of prolapse to the fact that the iris is undamaged. It is not lying, damaged, toneless and flaccid in contact with the wound, waiting to be swept out of the globe on the slightest provocation, should the anterior chamber be lost. It is taut, thus allowing any escaping aqueous to run over its surface.

## A Modification of the Usual Method of "Needling" the Lens Capsule after Cataract Extraction.

By BASIL LANG, F.R.C.S.

(ABSTRACT.)

IN this paper I urge the routine "needling" of the posterior capsule shortly after extraction even in cases with good vision. I suggest that at a later date, the fine capsule becomes not only opaque, impairing the visual acuity, but also tough and difficult to divide.

In "needling" the "after-cataract" great pains must be taken to prevent prolapse of the vitreous into the anterior chamber, as glaucoma may thus be induced. In order to obviate this prolapse it is desirable, in the first place, to avoid allowing the aqueous to escape from the anterior chamber, and, in the second place, to refrain from destroying the normal structure of the vitreous by breaking it up with the needle.

To avoid the risk of the loss of aqueous from the anterior chamber I employ a needle, the diameter of the shaft of which is two-thirds of the width of the blade. By this means the blade cuts a hole in the corneo-sclera which is just filled by the shaft, thus making the loss of aqueous impossible.

In order to avoid damaging the structure of the vitreous I lift the capsule up on the needle and divide it, cutting forwards into the anterior chamber.

As a preventative of possible sepsis, I use a drop of 1 per cent. silver nitrate solution applied to the conjunctiva at the point of entrance of the needle. Further, to obviate the possibility of infection down the needle track I pass the needle subconjunctivally through the limbus into the anterior chamber.

Often when an attempt is made to pass the needle subconjunctivally, the point catches in the episcleral tissue. To obviate this, I hold the needle with the point directed towards the patient's feet and the blade in the antero-posterior, i.e., sagittal, plane. I press the cutting edge of the blade on the conjunctiva some 4 mm. from the limbus, the point, in the case of the right eye, being at the nine o'clock position. In the case of the left eye it is held at the three o'clock position. I then press the edge lightly into the conjunctiva and move the needle bodily towards the limbus. A fold of conjunctiva is thus formed in front of the advancing blade. When the needle has reached the limbus I begin to rotate it on its point, keeping this stationary. Not only do I rotate the needle by rolling it between my finger and thumb so that the blade lies in a plane parallel to and a little in front of the iris, but also at the same time I turn the whole instrument so that—in the case of the right eye—instead of pointing to the patient's feet, it now points to the left, the shaft of the needle lying in a direction radial from the centre of the cornea. I pass the instrument through the base of the fold of conjunctiva, onwards through the limbus just in front of the iris, into the pupillary area. I then dip the point, pick up the

capsule, and running the needle onwards just under the capsule, I pierce this once more near the far side of the pupil. The needle is then twisted so that the cutting edge faces forwards and upwards and cuts forwards and upwards into the anterior chamber. I never attempt to cut a strand but invariably choose some thin piece of capsule. I make no attempt to cut a large hole. I think that so long as the hole is central, only a small one is necessary. I believe that quite possibly it is disadvantageous to make a large hole, for I think that the capsule probably does good in the periphery by holding the vitreous in position. The advantages of this modification are:—

(1) It is easy; (2) it obviates the possibility of sepsis; (3) it obviates the possibility of glaucoma; (4) it eliminates the necessity of stirring up the vitreous.

#### DISCUSSION.

Mr. W. H. BRAILEY said that first, in regard to the method of extraction, he agreed that it was important to depress the sclerotic behind the lens, but by not putting on pressure below, one lost the mechanical advantage of the tilting forward of the lens. It was most important that the lens should be tilted forwards as the extraction was being done, and unless the operator pressed below he did not see how this could be achieved.

With reference to needling, he could not agree that the entrance of the vitreous into the anterior chamber was the most common cause of glaucoma following cataract extraction. He believed that the worst cases of glaucoma were due to the ingrowing of the corneal epithelium through the wound. This was borne out by the fact that in the ten years since he described the operation in the *Ophthalmoscope*<sup>1</sup> he had done very many needlings from the posterior part of the eye, passing a needle through the vitreous and dividing the capsule from behind with a Ziegler's knife, which he considered better than Bowman's needle, for one could easily get the former of the right dimensions to fit the wound; there was also the inestimable advantage that one could get the Ziegler's knife sharp, whereas it was very difficult to get a Bowman's needle with a good edge. Under this operation the vitreous could have a free access to the anterior chamber and he had never seen glaucoma caused in this way.

Sir RICHARD R. CRUISE said it was most important in cataract extraction to produce the forward tilting of the lens to which Mr. Basil Lang referred. During the last thirteen or fourteen years, the technique he (the speaker) had followed was the following: always to do a simple extraction and invariably fully dilate the pupil, definitely paralysing the sphincter before commencing the operation. He made the incision in the ordinary way. He considered it of great importance to lacerate the upper part of the capsule first, just inside the upper portion of the pupillary margin. For that purpose he used a cystotome. He met it with a T-shaped incision coming from below upwards. The lens had then begun to present in the forward position. The next procedure was to produce pressure on the posterior lip of the wound through the eyelid; he did not use an instrument for this. He then produced pressure through the eyelid on the posterior lip of the wound. The lens would already have been rotated so that its upper edge was presenting forwards; indeed it was almost asking to be allowed to escape out of the wound. With a tortoiseshell spoon he induced the lens to come out. The first pressure on the upper part, and not pressing back on the lower part of the cornea, were most important. And it was essential, from the point of view of prolapse, that one should have the iris retracted into as rigid a mass as possible, so that fluid could escape around the border of it, rather than push a fold in front of it and so cause prolapse.

It was now thirteen years since he did his first simple extraction in private, and he had had three cases in which there was slight adhesion of the iris to the lip of the wound, and in no case had there been prolapse. In two cases during the last four years there had been a slight adhesion of the anterior surface of the iris to the wound, and in each of those cases he made a small incision with a Ziegler knife in the lower part, and pulled it off with a repositor.

<sup>1</sup> *Ophthalmoscope*, 1913, xi, p. 600.

### 38 Lang: *Modification of the Usual Method of "Needling"*

Mr. J. GRAY CLEGG said that he would be reluctant to do a discission in these early stages. Most of the cases at his hospital returned with excellent visual acuity without resort having had to be made to that procedure. A cutting in the capsule was more easily made in the first six months than subsequently.

Mr. T. HARRISON BUTLER remarked that Mr. Basil Lang had said a great deal about pressure, but this was what he (Mr. Butler) tried to avoid in extracting a cataract. The operation should be so planned that the lens glided out with a mere touch. He would not open the thorny question of the merits of the simple operation, but if it had a disadvantage it was that it involved more pressure than was needed after iridectomy. Mr. Lang seemed to object to the use of a lens hook. He (Mr. Butler) always employed one and as soon as the edge of the lens appeared he drew it out gently, pressure on the globe thus being avoided.

With regard to the operation of discission: he imagined that Mr. Lang and Dr. Ziegler were in opposite camps. Ever since he had had the opportunity of seeing Dr. Ziegler operate and heard his paper at the Oxford Congress in which he described his V-incision into the capsule, he had followed his methods, and the improvement was so marked that he now regarded the Bowman needle as obsolete. Ziegler insisted that the knife be so made that the shaft was of the correct size and parallel, so that it would slide in the corneal wound without moving the cornea or losing aqueous. A badly-made knife jammed, and impeded the operation. The Ziegler knife required sharpening after an operation, but only once. With the Ziegler knife one could plan and carry out an accurate division of the capsule. After a careful examination of the capsule preferably with the Capski microscope, the incisions could be made so that a gap was formed in the capsule where it was most wanted. If he saw bands he attacked them at once, for they were doing the most mischief, and with a sharp knife were generally severed without difficulty. Many operations for cataract were quite satisfactory without a discission. It did not follow that sepsis was always due to external infection. He felt sure that it might be caused by endogenous infection. Not long ago he had needled the lens of a child and acute panophthalmitis followed. He discovered that the child had whitlows. Some years ago he had had two similar cases, in one the child had boils on its back and in another there were whitlows. In a third case an eye did badly, irido-cyclitis following a discission. Shortly after the infant died of pneumococcal meningitis. In a fourth case he needled the capsule for an elderly man whose lens had been extracted by Mr. Bell Taylor. The result was excellent. Two years later he needled the second eye and the operation was followed by severe cyclitis which fortunately subsided, a useful eye being left. Between the two operations the patient had had a severe attack of double pneumonia and the speaker thought that at the second operation the blood contained pneumococci from the pneumonia. He always took the most advanced aseptic precautions and operated only when he had obtained a sterile culture on blood agar. Under the circumstances he never took the risk of discission if the vision was satisfactory to the patient. To needle in a case where there was  $\frac{2}{3}$  vision seemed to him monstrous.

Mr. H. KIRKPATRICK remarked that a modification of Mr. Lang's method, though not so radical as the one described, had been in use in Madras for about ten years. He (the speaker) had found it very useful. A little pressure was made with a curette on the upper lip of the wound, before an attempt was made to deliver the lens. That produced a slight dislocation of the nucleus downwards. One knew when it happened, because the pillars of the coloboma spread out, whilst in the simple operation the pupil assumed an oval shape. The lens could then be delivered easily, without any counter-pressure being made at all; pressure was simply exerted on the lower part of the cornea. He had found that procedure very valuable, and it simplified the operation. Impaction of the nucleus beneath the scleral lip of the section could not occur if this technique was employed.

Mr. A. L. WHITEHEAD (President) said he gathered that Mr. Lang now did the simple operation entirely, and it seemed that in his last series he had nearly 4 per cent. of prolapses, rather a high percentage. For three or four years he (the President) had tried the simple operation, and for some time he thought he had found it to be the ideal operation.

But, like Mr. Harrison Butler—and perhaps others—after the experience of a series of relapses he had reverted to the combined operation, and with that he now rested satisfied. Mr. Lang's method of extraction was extremely interesting, and perhaps many members would try the preliminary pressure above before exerting the usual pressure from below.

In the matter of needling, too, he agreed with Mr. Harrison Butler; it was a good thing to let well alone, and if one could secure to the patient  $\frac{2}{3}$  or  $\frac{3}{4}$  vision, one should wait, especially in the case of old people, for in their case many things might happen before the transparent capsule subsequently became opaque. He did not advise routine needling within two or three weeks of the original cataract extraction.

Mr. BASIL LANG (in reply) said that he did not think that the lens "tilted" because pressure was applied at any one spot. If the volume of the eye were decreased, the contents of the globe must escape. First the iris came out, then the lens. If pressure were applied from above, in the manner he had described, the iris was pulled out of the way and the lens escaped readily and the iris was undamaged. If pressure was applied from below the lens in escaping pinched the iris and damaged it. His object was to remove the lens with the least possible damage to the iris.

He knew of only one case in which epithelium grew over the inner surface of the anterior chamber. He had never heard of a case in which lens epithelium had grown in this manner.

He remembered that Sir Richard Cruise did press the lid down into the wound. He (the speaker) thought that it was a proceeding not free from risk as the lid was incapable of being sterilized and the anterior of the eye might be infected. It was in order to avoid this very risk that he (the speaker) used a speculum of such form as would keep the lid from contaminating the wound. He did not think that Sir Richard Cruise pressed upon the upper part of the wound to the same extent as he did. He thought that it was not possible to depress the centre of the wound only, and to avoid pressure on the sides of the wound, as was essential to this method, by pressing with the thumb on the wound through the lid. For that reason he thought that pressure with an instrument was essential.

With regard to prolapse, the figures he (Mr. Lang) gave, were those from his patients at the Royal London Ophthalmic Hospital. Of twenty-four cases upon which he had operated at other places since he had been employing this method, he had had one prolapse only and that had occurred on the fourth day, as the result of the patient rubbing his eye violently.

With reference to needling being unnecessary, he had seen cases which would have been far better off if they had been needled shortly after the operation. He always needled his cases, and he would continue to do so, as his father's experience had taught him that it was always advisable to do this.

In reply to Mr. Harrison Butler, at no time was great pressure applied to the eye, and when he spoke of pressure it was in terms of pounds—not tons—per square inch. His great point was to avoid applying pressure which would damage the iris. He wished to employ the undamaged, and therefore unprolapsing, iris to keep the capsule out of the wound. If the capsule became entangled in the scar, the eye often did badly at a later stage.

With regard to the Ziegler knife, he (the speaker) did not use it nor did he counsel its use, because so rarely could a knife be found that had been made so that it cut a hole that was completely filled up by the shaft. Either the blade was too small and the tissues were bruised by the instrument being pushed into the anterior chamber, or the blade was too large when the anterior chamber more or less rapidly emptied itself during the operation.

With regard to sepsis following needling with consequent loss of the eye, he believed that such accidents always followed direct puncture of the cornea with needles the blades of which were too large, in cases where the conjunctival sac contained pathogenic organisms. With the use of a correctly shaped needle and the subconjunctival route, such accidents he believed were unknown. Cultures should be made on blood agar or other nutritious media and should be incubated for forty-eight hours. He had known sepsis follow needling where the operation had been performed in a case in which the culture tube showed a single colony of *Staphylococcus aureus*.



## Classification of Diseases of the Choroid.

By MALCOLM HEPBURN, F.R.C.S.

(ABSTRACT.)

[This paper will be published in full in the *British Journal of Ophthalmology*.]

THE author claimed that, in the present state of our pathological knowledge, the time had come when a classification ought to be attempted on the basis of the pathological changes known to occur in the various choroidal conditions; and that the method based on the ophthalmoscopic picture or on the position was somewhat antiquated and would lead to errors both in diagnosis and treatment.

After criticizing the various terms in common use, he proceeded to divide diseases of the choroid into five groups: (1) Inflammatory; (2) vascular; (3) degenerations; (4) congenital; (5) new growths. In the first two both acute and scarred stages might be seen.

After entering into the details which make up the ophthalmoscopic picture in each group, the author drew the following general conclusions in order to characterize the various types.

(1) When there is a large or small cedematous patch accompanied by sudden lowering of the visual acuity, vitreous opacities, and keratitic precipitates, followed by the formation of fibrous tissue of varying amount, with many coarse masses of gross pigmentation, and sometimes vessels crossing the floor of the scar, the pathological condition producing it is inflammatory in origin.

(2) When the pigmentary changes are of a fine granular type, and the choroidal disturbance, though decided, is of a somewhat indefinite character; accompanied by gradual and variable visual acuity, with no vitreous opacities and no keratitic precipitates, the pathological condition producing it is of vascular origin, either complete or partial cutting off of the blood supply, an old hæmorrhage, &c.

(3) When there occurred white or whitish-yellow areas, never very large and sometimes quite minute, surrounded by a perfectly even, well defined border of pigment which is not excessive, with no choroidal disturbance around it, the condition is one of hyaline degeneration, probably of the membrane of Bruch.

(4) When there is a pearly white patch with absence of vessels crossing the floor of it, and a narrow fringe of pigment round a well defined margin with no choroidal disturbance beyond the actual defect, the probability is that the condition is congenital in origin.

(5) The characteristic feature of a new growth is the raised choroidal swelling, often with a well defined non-cedematous border, and whatever pigment there is, is indefinitely mixed up with the main mass. There is frequently an ordinary detachment, namely, separation between the neuro-epithelial layer and the pigment-epithelial layer, somewhere in the neighbourhood of the growth; and if the case can be safely watched a gradual increase in the size of the swelling is noticed. The presence of new vessels helps the diagnosis.

(The paper concluded with an exhibition of coloured drawings in illustration of the various points referred to.)

Dr. RAYNER D. BATTEN said that Mr. Hepburn had most kindly warned him that he would combat his (Dr. Batten's) view that certain diseases of the macula were entitled to recognition as an entity as primary disease of the macula and to a separate classification; and that he would state his (Mr. Hepburn's) view that they should be considered as localized forms—choroiditis or retinitis, as the case might be—

and brought under the same classification. But even after hearing Mr. Hepburn's most valuable and interesting paper, he (Dr. Batten) still felt justified in claiming a special place and classification for diseases of the macula.

He asserted that anatomically, physiologically and clinically the macula area was singled out as a distinct area from the rest of the fundus. True, it contained both choroid and retina, and therefore was liable to be involved in diseases affecting either of these structures; but nevertheless both choroid and retina showed special reactions to disease in the macula area. The macula itself was avascular and was surrounded by a network of terminal vessels, which brought it into the category of an end-organ, and rendered it liable to special changes. Its nerve supply was also specialized, as shown by its special involvement in certain diseases and its exemption in others. In tobacco amblyopia, migraine, retrobulbar neuritis and lobar disease its function was affected, yet in hemianopsia it was exempted. In glaucoma it might survive to the end. Yet in myopia it was singled out for destruction.

There were other points which he would advance as justifying his claim that macular disease should be treated as an entity and not merged under the general head of choroido-retinitis, but they were not germane to that evening's discussion.

His own work on macular diseases was purely clinical, and it was on these grounds that he would specially wish to base his claim for the recognition of macular diseases. If macular disease, according to Mr. Hepburn, was only part of a general choroido-retinitis, how did Mr. Hepburn explain its comparative exemption from attack in syphilis and tubercle and its peculiar liability to attack in septic and cardio-vascular conditions.

Symmetry in eye disease was of course a marked feature in many eye conditions, but in few conditions was it so marked as in diseases of the macula, so much so that where symmetry was absent he (Dr. Batten) was generally doubtful as to whether the condition should be classed as a pure macular disease.

But the chief support for the macular diseases lay in the establishment of the maculo-cerebral group—and its associated familial macular disease. If these two groups be taken as established he (Dr. Batten) failed to see how it could be held that other macular diseases, such as septic and toxic cardio-vascular groups, might not exist.

## Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

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### Four Cases of Cerebro-macular Degeneration.

By LESLIE PATON, M.S.

THREE of the patients in these four cases of cerebro-macular degeneration are members of one family, and as we have had them under observation for some time, it is of those that I especially wish to speak. The affected members of the family consist of a boy, aged 8, a girl, aged 10, and the eldest boy, aged 11. In all three, the changes began to be manifested at secondary dentition. The whole three have been under observation for some considerable time, the youngest boy having had nothing wrong with him when we first had him under observation. The first sign of any change in his case was last July. Up to that time, his macula and retina generally were quite normal. Then he had a fit, and soon after this we observed the first changes in his macular region, so that we can say that in this boy we have seen the earliest change that can be detected in cerebro-macular degeneration, a small white area at the macula, with a reddish ring round it. So slight is the change that, if he had not been under observation, we might have regarded it as a normal, glancing fovea, of the bull's-eye lamp reflex character. The change is a little more marked in the left eye than in the right.

The sister shows the second stage in the development of the macular disease. The white area, with the reddish ring round it, is very much more defined. The peri-macular retina shows a streaky whitish appearance of degeneration. The disc is atrophic and the vessels diminished in calibre.

The elder brother shows a still further stage in the degenerative process; where the atrophy of the macula is complete, with a much larger white area, speckled over with black pigment, the peri-macular area showing the same streaky white appearance and the whole peripheral retina atrophic, with pigmentary changes resembling those of retinitis pigmentosa. The disc is white and the vessels are reduced to tiny threads.

The cerebral signs show similar progression from the youngest to the eldest patient.

In the youngest boy there is, as yet, very little spasticity, but during the fits spastic conditions are manifest, and he has the slight jerky movements which the others show in greater degree. The elder boy was very spastic in the earlier stages, but the spasticity does not seem to be quite so

<sup>1</sup> Clinical Meeting held at the National Hospital for the Paralysed and Epileptic, Queen Square, March 14, 1924.

marked now. He has no control of his movements in walking and cannot stand up. He is completely incontinent. The girl shows the intermediate stage, with marked spasticity and jactitation. All the deep reflexes are exaggerated.

Clinically, this condition, commencing usually about 6 to 9 years of age, is closely allied to Tay's amaurotic family idiocy. In amaurotic family idiocy, the disease most characteristically affects Jewish children. It commences in infancy, and proceeds rapidly to complete cerebral degeneration, with macular degeneration, and the children usually die before reaching the age of 2 years. It has been met with at least in one case in a child of purely Gentile parentage, reported by Dr. Harris, about the year 1910. In amaurotic family idiocy the whole progress of the disease is very much more rapid than it is in these cerebro-macular cases beginning in later childhood.

There is a third type of case, which I am inclined to associate with the other two, commencing usually about puberty. In this, the macular and retinal changes are almost exactly like those found in the cerebro-macular degeneration type, but the cerebral symptoms are only very slightly marked. I have recently had under observation two or three cases of this kind. The children in whom it occurs are usually intellectually backward, i.e., many of them at the age of 13 or 14, when the blindness commences, have only reached the first standard at school. In some of them, however, there does not seem to be any intellectual deficiency at all, but, with the progress of the retinal degeneration, some signs of cerebral irritation develop. Of one boy, in whom the macular condition was very marked, his parents remarked that he had changed in temperament, had become very moody and irritable, and was liable to explosions of violent temper.

Clinically, therefore, I should say that there are three definite types, one commencing in infancy, very rapid in its progress, with equally rapidly developing degenerative changes in the retina, cerebrum and cerebellum, and central nervous system generally. The second type, the Batten type, develops more slowly after secondary dentition, with a clinical history often extending over seven or eight years, but the final stages show the same complete degeneration of the central nervous system and in the retina as in the infantile type. Then there is the third type, not so definitely allied to the other two, in which the cerebral symptoms are very slightly marked and the retinal changes are similar to those found in the other two types.

The pathological changes found in the cells of the central nervous system seem to consist in lipoid infiltration mainly affecting the vegetative portion, as distinct from the neuro-fibrillar portion of the larger cells. The Nissl granules become crowded into the one portion of the cell as the lipoid material develops, and the neuro-fibrils are pushed to the sides.

Dr. GORDON HOLMES demonstrated the Pathological Changes in Cerebro-macular Degeneration by means of the epidiascope.

### **Case of Spleno-medullary Leukæmia (with Blood Films).**

By LESLIE PATON, M.S.

THIS is a characteristic case of the disease, but one can never rely on the retinal picture being the same from week to week. When I first saw this patient, the veins were not so widely dilated as they are now, but there was

#### 44 Paton: *Spleno-medullary Leukæmia*; Holmes: *Myasthenia*

a large area of retinal changes with very few patches of white fringed with red. These patches are now much more numerous. Three weeks ago there was a little patch of reddening and cedema, and some stippling near the macula. That has been replaced by an atrophic white area with pigment in it, and a bright red patch is seen in its centre. Since then there has been a great distension of veins. I have here a specimen showing all the different types of cells to be found in spleno-medullary leukæmia. This patient began with a red blood cell count of 2,000,000, and a leucocyte count of 500,000; the red-cell count has varied slightly, but that of the white cells has varied a good deal. The visual field shows very little change.

Mr. LESLIE PATON showed a case of Voluntary Nystagmus.

### Two Cases of *Myasthenia Gravis*.

By GORDON HOLMES, C.M.G., M.D.

THE first patient, a girl, aged 14, was referred to me at the Royal London Ophthalmic Hospital, by Mr. Juler. At that time she had an incomplete oculo-motor palsy on the right side, but the pupil reacted normally. I could not then find any other evidence of disease, and nothing in the history suggested a more general affection. Her blood and her mother's blood were both examined, but the Wassermann reaction was negative in both. Some weeks later it was the left eye that was chiefly affected, the levator palpebrarum and the elevators of the bulb being weak, but the pupils still remained normal and reacted well. The palsy has been variable in degree and in distribution; sometimes one eye has been more affected, sometimes the other, and the relative amount of weakness of the different muscles has also varied. The only other symptom of myasthenia at present is a tendency for her voice to tire out when she talks for a long time, especially towards evening.

In the other case, that of a man aged 40 years, there is the history of an apparently isolated diplopia seven years ago, from which he recovered completely. About three years ago he again developed diplopia, which disappeared, after lasting a few weeks. At that time there appeared to be no other symptoms. A year ago, diplopia again developed, with some ptosis of both lids, and his condition since then has been what you see to-day, namely, a paresis of various ocular muscles. It was only on my examining him more carefully that I recognized his case to be one of myasthenia gravis. When I first saw him he did not complain of any other symptoms, and he did not admit that his ocular palsies were more marked in the evening than in the morning. But I found there was some weakness of the triceps muscles on both sides, and that these and other muscles could easily be exhausted and that they yielded a typical myasthenic electrical reaction. His work has been of a monotonously limited character, that of picking up pieces of linen in a laundry and throwing them into a washing machine for several hours each day, by a sudden jerky extension of his elbow, the muscles chiefly employed being the triceps.

The especial interest of this case from the ophthalmological point of view is, that after suffering from diplopia he recovered and was well for three or four years. He had then another attack, from which he also recovered, and only now has the disease developed sufficiently to enable a certain diagnosis of the condition to be made.

## **Paresis of the Internal Recti in Lateral Movement of the Eyes.**

By GORDON HOLMES, C.M.G., M.D.

THIS case was referred to me a few weeks ago at the Royal London Ophthalmic Hospital by Mr. Foster Moore.

The patient stated that about ten days previously he had awakened seeing double. At that time neither internal rectus moved the adducting eye beyond the middle line when he attempted to look to the right or to the left, though both external recti contracted normally. The power of convergence was, however, fully preserved. All the other external ocular movements were unaffected and his pupils were equal, of moderate size and contracted well to light and on accommodation. There was consequently a dissociation in the functions of the internal recti; they acted normally in convergence but failed to contract in lateral movement.

The most obvious explanation of the case is that a lesion involved the fibres which ascend from the pontine centres for conjugate lateral movement of the eyes to that part of the oculomotor nuclei which innervate the internal recti. It is generally assumed that these fibres run through the dorsal longitudinal bundles. The result of such a lesion must be to produce a paralysis of the internal recti in conjugate lateral movement, but the mechanism of convergence would remain intact, provided the lesion did not involve the oculo-motor nuclei.

Three weeks after the onset of the condition the left internal rectus began to contract on lateral movement of the eyes to the right, but it still remains weak in this action. There has been no recovery in the inward movement of the right eye.

In reply to a question put by Mr. AFFLECK GREEVES, Dr. GORDON HOLMES pointed out that the history suggested that the lesion was vascular. The man went to sleep one Sunday afternoon, and when he awoke he was seeing double. He has a high blood-pressure and thickened arteries. Probably there was a thrombosis of one of the small branches of the basilar artery which ascend in the neighbourhood of the raphe of the pons: occlusion of one of those vessels would produce a softening of the dorsal longitudinal bundles.

## **Two Cases of Optic Atrophy in Osteitis Deformans.**

By W. G. WYLLIE, M.D.

*Case I.*—Female, aged 61, showed marked deformities of the skull and of the right humerus and femur. The right optic disc was pale all over with a greater degree of pallor in the temporal than in the nasal half, and the left disc showed pallor of the temporal half only. The visual acuity was  $\frac{5}{12}$  in each eye. In the right visual field there was a large irregular scotoma which practically surrounded the fixation point, and some constriction of the field was also present. No evidence of constriction or of scotomata was discovered in the left visual field. The pupillary reactions were normal. The patient at the age of 53 suffered from diplopia for a period of six months.

*Case II.*—Female, aged 52, developed double vision four years previously, and her sight began to fail gradually. The right external rectus and superior

## 46 Wyllie : *Osteitis Deformans*; Mann : *Orbital Tumour in Infant*

oblique muscles became completely paralysed. She also became increasingly deaf. Evidence of osteitis deformans was afforded by the increase in size and marked deformity of the skull, and rigidity of the spine. As much as six inches was lost in height. Pallor of the temporal halves of both discs was present. The visual acuity of the right eye was  $\frac{6}{24}$  and of the left  $\frac{6}{18}$ . Moderate restriction of both visual fields was present. The pupillary reactions were brisk.

Both these cases were in this hospital under the care of Dr. Gordon Holmes, and they have been examined carefully by means of X-rays. In both of them the skull showed typical osteitis deformans in the advanced stages. Several other bones in the body were also affected by the disease. It is well known that in this condition most of the foramina at the base of the skull become narrowed, and it is suggested that this has produced pressure on the nerves, and optic atrophy has resulted. In examining one or two skulls with osteitis deformans, I found that an instrument which could be passed with ease through the optic foramina in the normal adult skeleton was unable to be passed in those skulls.

In both these cases shown to-day the Wassermann reaction was negative, in one both in the blood and the cerebro-spinal fluid, and in the other in the blood. In neither of the patients was there any reason to suspect syphilis as a primary cause.

Mr. LESLIE PATON said that one of these cases had been under his observation since 1914, and the first occurrence in her case—as also in the other—was diplopia. In both patients the diplopia had lasted for some time. In the case of the patient he had watched, the muscles which seemed involved were entirely those of the right eye. The patient was also the subject of hyperacusis in the right ear with marked tinnitus. The ear condition improved a good deal. She remained fairly well until she came for fresh glasses in January, 1922, and at that time vision was good in both eyes and there was no trace of diplopia. But routine examination showed that there was a white atrophic disc in the right eye. Using a 10 mm. disc he could not find any limitation of fields, and there was nothing pointing to pituitary overgrowth, nor was there a history of syphilis. By November, 1922, however, though visual acuity was still  $\frac{6}{6}$ , she had extraordinary scotomata, which varied. Since then she had steadily lost the field in the right eye, though it still had  $\frac{6}{6}$  vision. Similar changes in the left eye were now becoming evident. When he first looked into her right eye he thought she had developed glaucoma, as there was a white disc and what looked like a cupping of it.

### Orbital Tumour in an Infant, with Metastases in the Skin.

By IDA C. MANN, M.B.

THE infant, aged 15 weeks, was normal at birth, and the mother healthy. The Wassermann reaction of the mother and child is negative. At the age of 5 weeks the mother noticed that the left eye was becoming displaced upwards and inwards, and that there was inability to raise the upper lid completely. A week later nodules appeared in the skin of the back, thighs and arms, and similar nodules have been appearing ever since, though not so quickly, recently. There are now twenty-three of them.

When the child was first seen four weeks ago, the left eye was displaced upwards and inwards and movement was deficient downwards and outwards; a swelling was projecting forwards from the orbit, just beyond the orbital margin below, and it was just visible through the conjunctiva. There was no

chemosis at that time. In both eyes the fundus was normal. The condition of the child has always been good, and it is better now than when it was first seen. Skiagrams showed the left orbit enlarged. The blood-count does not show anything abnormal except a slight increase in the hyaline leucocytes.

Two of the skin nodules have been excised under the idea that they were secondary to the orbital growth, but little clue has in this way been supplied, as the sections only show fibrous tissue and some thickening of epidermis.

The case is shown for discussion of diagnosis and treatment. The orbital tumour is evidently malignant. It is not a chloroma, as it has not the characteristic colour, and the child's condition seems to be good. The blood-picture is not that of chloroma, in which there is either an increase of myeloblastic cells or a greater leucocytic count than this child has. Both the orbital and the skin tumours may be secondary to the condition known as adrenal sarcoma of infants. In the Hutchinson type of this disease the primary growth is often too small to be palpable. The orbital growth may be a lympho-sarcoma, but there are no enlarged lymphatic glands, and nothing abnormal can be felt in the abdomen.

#### DISCUSSION.

Mr. D. L. DAVIES said he had seen a case of the kind in a lady, aged 76, who stated that forty years previously she had seen treated by Sir James Paget for malignant disease of the tongue, and was, as she said, cured by Chian turpentine. When the speaker saw her, she had very small reddish nodular lumps in the skin of the scalp, and later her trunk was involved in their spread, and the limbs also. After death, the whole of her peritoneum and the liver were found to be covered with similar nodules, which proved to be of an endotheliomatous nature.

Mr. A. L. WHITEHEAD (President) said that in his opinion the tumour in this infant was sarcoma. He regarded the case as inoperable, but the effect of deep X-rays might be tried.

*Postscript.*—Since this case was shown exenteration of the orbit has been performed, on account of rapid increase in size of the tumour. The pathologist's report states that the tumour is a lymphosarcoma. The orbital periosteum was not involved, and there has been no local recurrence in four months. The child's general condition is still good, but the skin nodules have increased in size.

#### (1) Case of Cervical Sympathetic Lesion of Central Origin ; (2) Case of Cervical Rib.

By S. A. KINNIER WILSON, M.D., and H. J. MACBRIDE, M.D.

#### REMARKS BY DR. KINNIER WILSON.

I TAKE the two cases together. It is very interesting to contrast them. The girl's case is of central origin, due, no doubt, to syringo-bulbar disease. In the case of the woman, heterochromia iridis is shown. The girl with the cervical rib shows the reverse of what would be expected in a case of heterochromia iris; I think that on the affected side the appearance is darker and thicker, and several members of the Section who have looked at the girl's eyes agree with this view. Mr. Affleck Greeves said it looked shaggier on that side. In the other cases I have seen there is a diminution of pigment on the affected side, and therefore I have no explanation of this case, in which that condition seems to be reversed.

Dr. S. A. KINNIER WILSON and Dr. H. J. MACBRIDE showed a case of Optic Atrophy of unknown origin.



### Case of Benedict's Syndrome.

By J. S. RISIEN RUSSELL, M.D., and H. J. MACBRIDE, M.D.

#### REMARKS BY DR. MACBRIDE.

THIS is a comparatively rare condition; involvement of the third nerve, with hemiplegia on the opposite side, with tremor. It is a very obvious case. The tremor is of the undulating type, not like that of paralysis agitans.

Mr. LESLIE PATON said he showed this patient before the Section two years ago, and then the tremor was not so marked as now. He suggested that the lesion was situate in the red nucleus.

Dr. JAMES S. COLLIER and Dr. E. A. CARMICHAEL showed a case of Jaw-winking Reflex.

### Case of Gumma of Choroid.

By C. M. HINDS HOWELL, M.D., and E. A. CARMICHAEL, M.D.

#### REMARKS BY DR. CARMICHAEL.

THIS case was seen two months ago at the hospital; there was a distinct swelling in the region of the macula, which was yellow and raised; *plus 4 D.* The child has been placed on active treatment for syphilis, and the condition has cleared up without even a scar being left.

### Case of Retro-ocular Tumour.

By C. M. HINDS HOWELL, M.D., and M. CRITCHLEY, M.B.

#### REMARKS BY DR. CRITCHLEY.

THIS girl was admitted to hospital with a five weeks' history of exophthalmos in the right eye; for four or five days there had been headache and vomiting. The right eye was found to be proptosed, but there was no loss of upward movement. There is a very intense papilloedema—5 D of swelling. The skiagram of the skull is negative. The blood Wassermann reaction is negative, and the blood-count excludes chloroma. Is this a primary intra-orbital tumour, or a secondary intra-orbital growth following adrenal sarcoma?

Mr. TREACHER COLLINS pointed out that there was very marked hypermetropia in the eye, besides the papilloedema, this indicating that there was a good deal of pressure on the posterior surface of the globe, with a flattening from before backwards. He considered there was a growth in the cone of muscles behind the globe, which was pressing on the globe. There was a strong probability that it was a tumour of the optic nerve. He therefore advised an exploratory operation, cutting through the outer canthus, dividing the external rectus muscle and exploring with the finger behind the globe.

Mr. MONTAGUE L. HINE, M.D., F.R.C.S., showed a Specimen of an Unusual Form of Opaque Nerve Fibres.

Mr. F. A. JULER, F.R.C.S., showed a Case of Essential Shrinking of the Conjunctiva.

PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

EDITED BY  
SIR JOHN Y. W. MACALISTER  
UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

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**VOLUME THE SEVENTEENTH**

SESSION 1923-24

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SECTION OF ORTHOPÆDICS



LONDON  
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1924

## Section of Orthopædics.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### PRESIDENT'S ADDRESS.

#### Manipulative Surgery.

By R. C. ELMSLIE, O.B.E., M.S.

THE use of manipulative methods in surgery is constantly being brought before the public, and has perhaps been specially prominent during recent years. It is a branch of work frequently taken up by unqualified practitioners, probably because it is easily acquired and can be carried out with comparatively little training or experience, and with very considerable immunity from gross accidents. Formerly such practitioners were called "bone-setters," claiming that they replaced small bones in the course of their manipulations. Wharton Hood states that Hutton really believed that he did this. The term "bone-setter" is a very ancient one, and was formerly applied to both regular and irregular practitioners. In the seventeenth century there was a "bone-setter" and an "assistant bone-setter" on the staff at St. Bartholomew's, both of them presumably surgeons. Recently, however, the irregular practitioner has become known as a "manipulative surgeon" rather than as a bone-setter. The claim to replace small bones is difficult to sustain in view of the knowledge of anatomy now possessed by many of the general public, and of the possibility of confirmation by X-ray photographs. It is now more often made by the comparatively new school of osteopaths, rare still in this country but very numerous in America. These practitioners, in spite of radiography, are able to propound their claims to replace bones in the comparatively complicated osteology of the spine.

Sir James Paget investigated bone setting, and published a clinical essay upon the subject in 1867. His teaching was followed up by Howard Marsh, who lectured almost yearly at St. Bartholomew's upon the subject. The following paragraph from Howard Marsh's writings is worthy of quotation, as it puts many of the facts in a nutshell:—

"Chirurgery, or handicraft, began, as we may well believe, in attempts to pull-in dislocated bones, to straighten distorted joints, and to restore movement to stiff limbs. In this dawn of the art nothing was known of anatomy or pathology; it was only seen that a limb was bent or stiff, and force was employed to overcome the defect, just as it might be used to straighten a crooked bar, or loosen a rusty lock. Soon, however, the primitive operators of those early days became ranged in two groups: the mere empirics, who went straight to the point of trying what force would do; and those who endeavoured to ascertain the nature of particular cases, and the difference between one



case and another—those, in other words, who cultivated pathology and diagnosis in order that they might use force with safety and advantage. The results of practice conducted on these different lines can easily be imagined. The empirics, applying force in all cases alike, and thus involving their patients in a mere game of chance, did good whenever such untempered force as they could use was appropriate, and harm whenever it was unsuitable; those who used force only when they could see a reason for doing so, and when they thought it was safe, while they did little harm, often, as their diagnosis was very rudimentary, missed an opportunity of producing a cure. In these conditions the empirics frequently had the best of it. Regular, but as yet very ill-informed, practitioners were so often beaten in their encounters with disease that they lost credit in the public eye; while the empirics, making the most of their cures, and not seldom laying the blame of their failures on the surgeon whose previous treatment they alleged had done all the mischief, were credited with powers that approached the miraculous. And we can understand their success, for every surgeon now well knows that instances are common enough in which pain and limited motion, resulting from sprains and other injuries, may at once be set right by even rough and unskilled movements, or, indeed, by an accidental wrench."

Wharton Hood, gaining experience from working with the bone-setter, Hutton, showed how some of the methods of treatment of injuries formerly in general use led to the production of stiff joints which could be easily righted by simple manipulation.

More rational methods of treating injuries, together with the evolution of a well-trained and experienced body of masseuses, have greatly diminished the number of cases of gross stiffness of joints after injury, the class of case most often quoted by Paget, Marsh and Wharton Hood. Yet the dramatic successes of the unqualified manipulator continue, and it behoves the medical profession to look to its methods and to find out why so many of these cases pass into the hands of the unqualified.

#### FACTORS IN FAVOUR OF THE UNQUALIFIED PRACTITIONER.

Certain external factors must inevitably work in favour of the unqualified. Of these one of the principal is the inherent tendency of the public to believe that the irregular practitioner must have some peculiar ability, either a special skill in methods of manipulation unknown to the profession, or an almost occult power of seeing what is wrong inside, or what is the effect of his manipulations. A second factor is the unrestricted power of the unqualified to advertise, either directly or through the mouths of their patients. A third is perhaps the most important; it is that the failures of the unqualified are in almost every case hidden. The patient who visits a bone-setter and is cured boasts of the fact widely, but those who are not cured, or who are made worse, are perhaps a little ashamed that they have been gulled; they say nothing of their experience unless perhaps to a medical man, who subsequently treats them and who discreetly maintains silence. For the failures of the unqualified manipulator are numerous, almost as numerous as his successes. The gross cases quoted by Marsh, who mentions two cases of sarcoma and one of spinal caries treated by manipulation, are perhaps uncommon at the present day. Yet I, personally, have seen many failures by commission or omission. Among the former I may mention myositis ossificans around the knee rendered so bad by the manipulations of a bone-setter that the patient had to submit to complete rest of the joint for two and a half years before movement returned; a youth with a slight spinal injury subjected to manipulations of the coccyx, which was loosened, and the patient converted into a confirmed neurasthenic with coccydynia; a young woman con-

verted by osteopathic manipulation into an apparently hopeless neurasthenic, and a woman with a large loose body in the knee which was manipulated into such a position that it blocked full extension of the knee for four years.

I need only mention more gross cases of obvious fraud; the treatment of a joint with bony ankylosis by manipulations repeated over a period of a year; the patient being promised a restoration of mobility; a dislocated thumb treated for three months with improvement of movement *in the direction of excessive hyperextension*; a shoulder stated by a bone-setter to be dislocated, whereas it was normal. In this last case, when confronted with an X-ray showing the joint in place, the bone-setter countered with an X-ray of a dislocation of the shoulder, which he demonstrated as the normal position.

Errors of omission are also not uncommon. The unqualified manipulator refuses to treat a patient on the ground that the condition does not come within his province, and a cure subsequently results from a simple manipulation carried out by a qualified medical man.

In addition to these, cases in which manipulation has failed to cure the original condition or in which there is a recurrence of trouble (for example, displacement of a damaged semilunar cartilage) come within the experience of most of us. I do not suggest that the bone-setter is to be blamed for this last class of failure. Usually in these cases he has carried out a rational method of treatment. If his patient has any complaint to make, it is because he has been promised a certain cure where no certainty was possible. But when the failure is due to insufficient care in diagnosis, or to an inadequate knowledge of pathology, or to the use of irrational methods, then the manipulator should be blamed.

We all have our failures; medicine is not an exact science and no doubt many of us have failed to cure patients who have subsequently been successfully dealt with by the unqualified manipulator, just as some of us fail to cure patients who are afterwards cured by our colleagues, or as the unqualified manipulator has failed to cure patients with whom we afterwards succeed.

The real interest of the profession and particularly of the orthopædic surgeon is to frame such rules of treatment as will leave as small as possible a number of cases for the unqualified manipulator to treat.

#### SUGGESTIONS FOR THE ORTHOPÆDIC SURGEON.

Looking back in my own practice I venture to make these suggestions to you all. First and foremost, in treating all bone, joint and muscle injuries, form as accurate as possible a judgment of the pathology of the condition present. When there is an inflammation or other condition indicating rest and contra-indicating mobilization, say so. If your judgment is accurate, your patient will not be cured by a practitioner who takes up the opposite view. A common error is to assume that the inflammatory reaction to a simple injury necessitates rest. This is not so; a sprain, provided that an important ligament is not ruptured or unduly stretched, recovers most rapidly with normal use, the joint being supported if necessary. An injury to a small portion of muscle requires use as soon as the pain felt will permit it. If the internal semilunar cartilage in the knee has been damaged and displaced, use of the knee as soon as possible after the displacement has been reduced is the best method of treatment. Some years ago I had to treat an oarsman in training for an important boat race who displaced the internal semilunar

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cartilage nine days before the race. The displacement was reduced twelve hours after it occurred and the patient was only kept out of his boat for twenty-four hours. In the end he rowed with an apparently normal knee and has had no trouble since.

This leads me to a second point: Never advise operation for removal of a semilunar cartilage which has once been displaced and successfully reduced; in a large number of cases the accident never recurs.

One often meets with patients who give a history of recurrent trouble with a knee, which sounds superficially like a displaced cartilage, but in which true locking of the knee has been absent. It is my custom to treat all these cases by manipulation, warning the patient that if the cartilage is damaged there may be a recurrence of the trouble, necessitating operation. I believe that many of the so-called cartilage injuries cured by bone-setters come into this class and that there has never been any damage to the cartilage at all. Other cases so cured are analogous to those in which the cartilage is displaced once and once only. In these cases the cartilage is presumably displaced by the manipulation into a position in which it gives no trouble and is not liable to become again displaced. I do not believe that the bone-setter has any special tricks of manipulation. If he has he ought to describe them, instead of keeping his knowledge to himself, just as any of us would put on record any new method we had devised. But, as I say, I do not believe that he has any such methods, and this opinion is borne out by the fact that many of us have treated patients with cartilage injuries which have been manipulated by bone-setters not simply once but many times.

It would take me too long to enumerate the many other conditions that are amenable to treatment by manipulation; they range from joints and muscles which are painful on account of apparently trivial restriction of movement to gross cases of stiff joints, of adherent muscles and of nerves such as the sciatic, which are fibrosed after an old-standing neuritis. I can only make this suggestion, that whenever there is a possibility of improvement by well-judged manipulation, try it. If your pathology is correct, you will do no harm and often you will cure your patient. I do not suggest that you can always promise a cure; how much you can promise must always depend upon individual knowledge and experience. Further, I would say that whenever a joint or muscle is apparently normal, yet is painful, it is worth while to try a manipulation. Disease and mechanical defect having been eliminated no possible harm can result, and in nine cases out of ten some minor adhesions, undetectable by clinical examination, will be felt to give and the patient will be cured.

I believe that we fail to cure by manipulation most often by our failure to give the method a trial. Let us use it freely, restrained only by an adequate knowledge of pathology.

#### Case of Deformity of the Spine of the Tibia.

Shown by R. C. ELMSLIE, O.B.E., M.S.

C. H., A GIRL, aged 10 years, came to the Orthopædic Hospital on February 16, 1923, with a history that she had fallen and injured the left knee nine weeks before and had since then been unable to straighten it; there was no previous accident or illness bearing on the condition.

The knee was then flexed at about  $165^{\circ}$ ; attempts to force extension were painful, flexion was normal in range, there was slight lateral mobility and a little knock-knee deformity. The X-ray photograph showed a keel-shaped prominence in the region of the spine of the tibia. In the antero-posterior view there was a lighter area beneath this projection, which indicated that possibly the central part of the tibia had been lifted up and that new bone had formed between it and its original position.

Owing to infectious disease in the hospital the operation was not performed until May 16. The knee was then opened from the front, the capsule being divided just internal to the patella and patellar tendon. The patella was displaced outwards and a good exposure of the joint secured. The projection of bone was exposed and a wedge-shaped portion removed, leaving the upper surface intact. This upper surface being then forced down in position, the knee came into full extension with ease and was fixed in this position on a back splint. The limb was fixed in plaster a fortnight after the operation and left thus for five weeks, massage and movements being commenced on July 6. At the present time the knee is practically normal in all respects.

### **Case of Infantile Paralysis with Calcaneo-cavus Deformity, showing the Result of Whitman's Operation Ten Years after Operation.**

By E. LAMING EVANS, C.B.E., F.R.C.S.

M. B., FEMALE, aged 23, suffered from an attack of anterior poliomyelitis at age of 3. First seen at the West End Hospital for Nervous Diseases in 1909 at the age of 8. The left lower extremity was typically atrophic, with calcaneo-cavus deformity. Galvanic stimulation, massage and supports were employed until 1913, when Whitman's operation of removal of the astragalus, displacement backwards of the foot and shortening of the peronei with suture to the tendo Achillis was performed after a preliminary section of the plantar fascia.

The case is now shown ten years after operation. The movement at the new joint is smooth, painless, and through  $30^{\circ}$  (from  $90^{\circ}$  to  $120^{\circ}$ ), locomotion is good, the foot passing into slight valgus on weight bearing. There is no cavus, and the external malleolus lies on the outer side of the calcaneo-cuboid joint. Dorsal flexion beyond a right angle is prevented by contact of the scaphoid with the anterior margin of the lower tibial facet.

### **Case of Infantile Paralysis with Calcaneo-cavus Deformity, showing the Result of a Combination of Steindler's and Whitman's Operation.**

By E. LAMING EVANS, C.B.E., F.R.C.S.

E. V., MALE, aged  $12\frac{1}{2}$ , suffered from an attack of anterior poliomyelitis at the age of 1 year 3 months. Was treated at the Royal National Orthopædic Hospital by massage, galvanism and supports, off and on for several years, but with progressive calcaneo-cavus deformity.

The cavus was particularly severe. Steindler's operation was performed

## 6 Laming Evans: *Infantile Paralysis*; Ogilvie: *Myositis*

in February, 1923, for its correction, and in March, 1923, Whitman's operation for the calcaneus was performed. All deformity has been corrected by these two operations and some movement is beginning to take place in the new joint.

It is hoped that the employment of a preliminary Steindler's operation will prevent any tendency to a secondary varus deformity, by increasing the basal support for the new joint.

### Three Cases of Myositis.

By W. H. OGILVIE, M.Ch.

I HAVE brought this evening three cases of myositis, of different types, which have come under my care during the last two years and which may possibly present some points in common.

*Case I*, a boy, aged 19, who in September, 1920, fell, and strained his left hip. There was very little disability at the time, but after much walking the hip became painful. The patient played football for a local club throughout the winter, but after a match he was inclined to limp, and was prevented by pain from swinging the leg right back. There was a gradual diminution of the pain, till by March, 1921, it had practically gone.

In the first week of April, 1921, patient had an attack of "influenza" with pyrexia, headache, and pain behind the eyes, for which he was confined to bed for three or four days. After this the thigh and abdomen began to swell, and became very painful, finally making walking impossible.

When I saw him on April 28, 1921, a hot, tense, and tender swelling occupied the whole of the left iliac fossa, producing a pronounced bulge above Poupart's ligament, and extending well up into the left lumbar region. Fluctuation could be obtained from this swelling to the upper part of Scarpa's triangle, and was also transmitted to a finger placed against the left wall of the rectum. The left thigh was flexed, abducted and externally rotated. With the thigh flexed to a right angle however, rotation at the hip joint was free and painless. The spine showed no limitation of movement.

Urine: normal.

*Operation* same day. Two pints of pus were evacuated by an incision above Poupart's ligament. The first culture from this pus gave *Staphylococcus aureus* only, but later cultures gave a mixed growth of staphylococci and streptococci. No tubercle bacilli were found.

*Progress*.—Five days later the patient had a temperature of 103° and developed a scarlatiniform rash.

The sinus finally healed in October, 1921.

*X-rays*.—Plates taken in April, 1921, and November, 1921, showed no abnormality in the vertebræ, left sacro-iliac, or left hip-joints.

*Present condition*.—General health excellent. Full range of movement in left hip.

The sequence of events in this case appears to have been: First, a strain of the left psoas, causing rupture of some fibres and a hæmatoma inside the sheath; six months later a lowering of resistance, due to an attack of influenza, and consequent infection of the old hæmatoma with streptococci. Bone disease can be definitely excluded.

*Case II*, a boy, aged 9, who in April of this year had an acute febrile attack, diagnosed as influenza. He was in bed for two days. There was no rash or

sore throat, but pain and tenderness to touch of both limbs, the pain being worse at night. There was no swelling of the legs or discoloration of the skin.

*Examination* (May 15, 1923).—The child was brought to my out-patient department a month later for a limp, which had persisted since the febrile attack a month before. He dropped the right hip in walking.

There was wasting of the right lower limb. Measurements: Right calf,  $9\frac{1}{2}$  in.; left calf, 10 in.; right thigh,  $11\frac{3}{4}$  in.; left thigh, 13 in. Hip, knee, and ankle-joints were normal on both sides, apart from the limitation due to shortening of the controlling muscles.

In the right hip extension was limited by  $30^{\circ}$ ; flexion was limited by  $80^{\circ}$  with knee extended; abduction was limited very slightly: rotation and adduction were normal.

On flexing the spine a full degree of extension was obtained at the hip, and on flexing the knee-joint another  $40^{\circ}$  of hip flexion was obtained. The limitation of movement was thus due to contracture of the psoas and hamstring muscles. There was wasting, without shortening, of the quadriceps extensor and of the calf muscles.

On the left side, a slighter contraction of the hamstrings was the only noticeable abnormality.

The child is unable to sit up with legs extended on a couch.

*Reflexes*.—Abdominal reflexes normal; cremasteric reflexes normal; knee-jerks normal and equal; plantar reflexes—flexor on both sides; tendo Achillis jerks—absent on right side.

No sensory or vasomotor changes.

In this case there appears to have been a sudden inflammatory affection of the muscles affecting only the lower limbs, and ending in wasting with fibrosis in the hamstrings, wasting, without any shortening, in the quadriceps extensor and calf muscles.

There is no clue to the causative organism.

*Case III*.—A case of dermato-myositis or pseudotrichinosis in a boy aged 13.

His illness started in January, 1920, at the age of 10. He first noticed a stiffness and weakness in the legs, which only affected him going upstairs, and did not, for some weeks, prevent his going to school. Gradually the weakness increased, and the legs became painful and tender to the touch; apart from this he did not feel ill. The weakness, stiffness and tenderness later affected the muscles of the trunk and upper limbs. By May, 1920, he was "hardly able to crawl," and "his whole body seemed to be getting stiffer every day." He was admitted into the Victoria Hospital, Chelsea, and remained there three months. About this time a bright red rash appeared on the cheeks, neck, eyelids, behind the ears, on the extensor aspect of the arms and forearms, and on the inner aspect of the thighs just above the knees. The rash remained for about a year and then slowly faded leaving an eczematous condition of the cheeks, eyelids and ears.

He was admitted to St. Vincent's Home in May, 1922, with a diagnosis of "chronic arthritis of all joints." At that time the shoulders, elbows, wrists, vertebral joints, hips, and ankles allowed very little movement. The feet were in extreme equinus, so that walking was impossible, the knees and elbows were flexed, the shoulders adducted. At these joints however, the small amount of movement allowed by the contracted muscles was free and painless, and the X-rays showed the articulations themselves to be unaffected.

*Progress*: The heel tendons were lengthened, and the feet put up at right

angles. The other joints were treated by gradual stretching and massage. There has been continuous and steady improvement.

*Present Condition: (a) Skin.*—There is smoothness, hardness, and loss of elasticity of the whole skin of the face, giving it a blank and expressionless appearance—the “masque sclérodermique.” The normal wrinkling during the play of the features is absent. The same change is present over the mastoids, and here there is some eczema. A slighter degree of this fibrotic change is also present in the skin over the backs of the arms and forearms, and on the skin of the inner side of the right thigh just above the knee. Here the change is only perceptible when the skin is plucked up between the finger and thumb.

*(b) Muscles.*—There is wasting with fibrosis of all the muscles of the axillæ, arms, forearms, intrinsics of hand, muscles of erector spinæ group, glutei, adductors of thighs, hamstrings, quadriceps, and muscles of calf and foot. The muscles of the neck, thoracic and abdominal wall, and those of mastication and deglutition appear unimpaired. The shortening is now much less than it was, and is chiefly noticeable in (a) long flexors of fingers; (b) erector spinæ; (c) quadriceps femoris.

*(c) Reflexes.*—Eye reflexes—normal; cutaneous reflexes—normal; tendon reflexes—present, but difficult to elicit.

This boy is undoubtedly an example of dermato-myositis or pseudo-trichinosis, a diagnosis which was first suggested to me by Dr. Symonds. This is an uncommon condition, Osler and McCrae only having found twenty-eight cases, of which seventeen were fatal [1]. Through the kindness of Dr. Herbert French I have also examined the notes and drawings of an unpublished case which was under his care in Guy's Hospital.

The history of the onset in this case is characteristic of the disease: first the weakness in the muscles, succeeded later by pain and tenderness, and spreading from one group to another till finally nearly the whole voluntary muscular system is involved; later the onset of constitutional symptoms, and the appearance of a rash. The rash is usually erysipelatous but may be urticarial, and is accompanied by cedema without renal or vascular changes. In Dr. French's case the rash is described as resembling that of erysipelas superimposed on cedema, and varying from a bright scarlet to a livid purple. The rash lasts from some months to a year, and then slowly fades.

The microscopical pathology has been fully described owing to the fatal nature of the disease. Degenerative changes are found in the muscle fibres, but the chief feature is the infiltration of the intermuscular connective tissue, and of the subcutaneous tissues.

Little is known of the causation. Dr. French excised portions of subcutaneous tissue and of muscle during the acute stage, and recovered a streptococcus from the subcutaneous tissue, but no organism from the muscle. His patient died from broncho-pneumonia three years later, when recovery was well advanced, and post mortem streptococci were cultivated from several excised muscles. This may, however, have been a terminal infection. The evidence, such as it is, seems to point to a streptococcus as the cause. The enlargement of the spleen which accompanies the acute stage suggests a septicæmic state.

No mention is made in the text-books of late results, but the general fibrosis of muscles and subcutaneous tissues seen in this case probably presents a fair picture, and suggests that with appropriate treatment, the final disability is inconsiderable.

The chief differential diagnosis is from trichinosis, due to the ingestion of the *Trichinella spiralis* in infected pork. Trichinosis gives a very similar picture, with stiffness, pain and tenderness in the voluntary muscles, accompanied by œdema similar in distribution. There are, however, acute gastrointestinal symptoms, a rash is uncommon, the blood shows a high eosinophilia averaging 30 per cent, and the parasites may be found in the stools. In the latter stages an X-ray shows the calcified embryos in the muscles.

In my third case, then, we are dealing apparently with a widespread septicæmic condition, affecting nearly the whole voluntary muscular system and the subcutaneous tissues, and possibly due to the streptococcus. At the other extreme we have, in the first case an acute abscess in one muscle due to streptococci and staphylococci, the incidence being determined apparently by a lowering of both general and local resistance. The second case appears intermediate between the two, both as to the rapidity of the course and the extent of the muscular involvement: it may be that here too we are dealing with a blood-borne streptococcal infection.

As well as typical dermatomyositis, there are a fair number of reported cases of scleroderma, entirely chronic in onset, and accompanied by contractures in single muscles or parts of muscles. This association has been described by Nixon [4] who also points out that some cases of rheumatic fever are characterized by infiltrations of tendinous or aponeurotic structures, others by subcutaneous inflammation. In the same paper he describes a case of a girl of 20 who scratched her right arm; the scratch was followed by an attack of erysipelas, and when this subsided an indurated area of skin remained over the deltoid insertion. Two months later she developed articular rheumatism with acute endocarditis.

This case seems to suggest a connecting link, and it appears possible that in many of these patients we are dealing with different manifestations of an infection by a strain of streptococcus.

#### REFERENCES.

- [1] OSLER and MCCRAE, "System of Medicine," vol. v, p. 875. [2] ALLBUTT and ROLLESTON, "System of Medicine," vol. vii, p. 3. [3] LORENZ, *Berlin Clin. Woch.*, 1906, xliii, p. 727. [4] NIXON, *Lancet*, January 12, 1907.

Mr. FAIRBANK said that with regard to Mr. Ogilvie's second case he would suggest that this was a case of anterior poliomyelitis. The present condition would agree with this as a possible diagnosis. While discussing this suggestion Mr. Ogilvie had called attention to the fact that contracture was present one month after the onset. Mr. Fairbank did not think this fact against a diagnosis of infantile paralysis, as he had frequently seen contracture occur as early as one month from the onset in that disease.



## Fracture with Dislocation of the Lower Third of the Right Radius.

By PAUL BERNARD ROTH, F.R.C.S.

C— P—, aged 27, lorry-driver.

Seen on September 12, 1923, one and a half hours after injury; he had been starting up his engine, when it back-fired, and broke his right wrist.

*On examination* of patient, who was in very great pain—unable to sit still, rolling this way and that on his chair—the right forearm and wrist were seen to be deformed, with marked ulnar deviation of the hand. The radiogram already taken showed that an old fracture of the radial shaft, at the junction of its middle and lower thirds, had refractured with dislocation backwards, away from the ulna, of the lower fragment, carrying the hand with it. An anæsthetic was given, and at the third or fourth attempt at reduction



(a)

FIG. 1.

(b)

FIG. 2.

FIG. 1.—Radiogram taken within an hour of the injury.  
(a) Antero-posterior view; (b) lateral view.

FIG. 2.—Radiogram taken on following day, showing reduction of dislocation.

by flexion, extension and rotation, the parts suddenly jerked back into place with an audible "scrunch," and the limb appeared normal. A straight anterior splint reaching from the elbow to the heads of the metacarpals was applied, and the arm placed in a sling. When the patient recovered from the anæsthetic, he remarked that all the pain had gone.

The radiogram taken the day after reduction shows a perfect replacement of the lower fragment in relation both to the upper fragment and to the ulna.

*Note.*—Patient states he broke his right radius in starting his car eleven months ago, and was treated for it at Omar Military Hospital.

**Case of Recurrent Dislocation of the Inner End of the Right Clavicle.**

By P. MAYNARD HEATH, M.S.

M. W., FEMALE, aged 15½, was stooping while at work, on April 18, 1923, and on raising her body struck the point of her right shoulder on the under surface of a machine. She felt pain in her neck, and later on discovered a lump where the clavicle projected.

The inner end of the right clavicle was found dislocated forwards. It could easily be replaced and as easily redislocated. It gave rise to very little pain. The arm was bound to the side and the clavicle kept in position with strapping. Treatment has been without effect.

**Case of Tendon Transplantation.**

By B. WHITCHURCH HOWELL, F.R.C.S.

PATIENT, a male, aged 34, sustained a gunshot wound of the right forearm in 1916, also a gunshot wound of the right foot, for which he underwent Syme's amputation. He suffered from paralysis of extensors of fingers and thumb for which I operated on June 19, 1922: "The flexor carpi ulnaris inserted into extensor communis digitorum. Operation on the extensor longus pollicis was not done."

Present condition, October 2, 1923: Strong action of transplant. Good grip. Working as manual labourer.

**Tendon Fixation for Foot-drop.**

By B. WHITCHURCH HOWELL, F.R.C.S.

M. W. J., MALE, aged 41. Wounded March, 1918, with external popliteal paralysis.

Operation, July 15, 1921: Nerve explored and found to be destroyed at its division into muscular branches. Suture impossible.

Operation, March 24, 1923: Peroneal tendons divided, distal end of peroneus longus being passed along tunnel in tibia and sewn to tendon tibialis anticus, and the distal end of peroneus brevis through a tunnel in the fibula, and sutured back to itself. Tibialis anticus tendon passed through tunnel in tibia. Foot maintained in dorsiflexion by plaster of Paris, followed by special boot, leg iron, and toe-raising spring.

Present condition, October 2, 1923: Slight foot-drop has appeared since operation in March, 1923. Function has been much improved by operation, and the patient is satisfied. Boot and iron have not yet been dispensed with, as the patient is doing heavy work which will not as yet allow the gradual discarding of the appliance.

## 12 Whitchurch Howell: *Foot-drop*; Verrall: *Sacro-iliac Arthritis*

### DISCUSSION.

Mr. W. R. BRISTOW asked Mr. Whitchurch Howell if he considered the patient had been improved by the operation of tendon fixation for slinging up the foot, and pointed out that he still required the toe-elevating spring. In Mr. Bristow's opinion this operation gave unsatisfactory results, and as far as he knew had been given up, and was not favoured by modern orthopædic surgeons. Experience had shown that the ligaments so produced stretched, and failed in their object of permanently holding the foot in the required position. He (Mr. Bristow) considered that if a case of this kind was shown at the Section, the operation should not be allowed to pass uncriticized; but of course this criticism must be regarded as entirely impersonal.

Mr. T. P. NOBLE said that at one time they did quite a number of tendon fixations for foot-drop at Oswestry, but their experience had been similar to that of Mr. Bristow, and recently they had more or less given them up. No matter how far the foot was dorsiflexed at the time of operation, it gradually dropped, in spite of the raising springs or stops.

### A New Type of Bone-graft for Sacro-iliac Arthritis.

By P. JENNER VERRALL, F.R.C.S.

PATIENT, a male, aged 24, sustained a severe strain while bayonet-fighting in 1917. This was followed by continued pain in the hips. He was treated at various hospitals without relief. He first came under my observation in March, 1923, when he had the typical symptoms of double sacro-iliac arthritis. This was confirmed by skiagrams which showed bilateral destructive arthritis but no signs of abscess formation. In spite of the fact that he has a dubious lung, I cannot assert that the lesion is tuberculous, although this is probable. I had intended to perform a double sliding bone-graft, but my assistant, Mr. Fleming, suggested to me that a tibial graft placed between the two posterior superior iliac spines would be useful. This appealed to me as being on the principle of a tie-beam. Accordingly I exposed both spines by turning down a flap convex upwards, cleared both spines, raised the two erectores spinæ from the sacrum, cut a groove in the sacrum between the two spines, and bored holes in the spines sufficiently large to hold a graft. I cut a thick tibial graft of the required length and passed it through one spine, along the groove in the sacrum under the muscles and through the opposite spine. The graft remained firmly in place without added fixation.

The patient was kept on a plaster bed for six months and subsequently allowed up in a pelvic corset.

At present he is entirely free from symptoms. The skiagram shows the graft firmly united in place, and a considerable improvement in the joint condition.

I may be mistaken, but I believe this to be a new method, easy of performance and applicable equally to cases of unilateral disease.

Mr. VERRALL, in reply to Mr. ELMSLIE (President) and Mr. ROTH, who expressed their appreciation of the method he had adopted, reminded them that the primary credit was due to his assistant, Mr. Fleming, who suggested the operation.

## Case of Patchy Gangrene of the Toes due to Vasomotor Injury.

By P. JENNER VERRALL, F.R.C.S.

MALE, aged 23, who in 1918 injured his right leg in an omnibus collision. There was no fracture or skin lesion, but there was great swelling of the limb with immediate paralysis of all muscles below the knee. I first saw him a year ago, when the paralysis remained and there were small black gangrenous patches on the toes. Loss of sensation was confined to the toes and the electrical reactions of the muscles were normal. The whole leg was somewhat blue and cold. I was informed that the patches appeared  $1\frac{1}{2}$  years after the injury.

Under treatment the gangrenous patches healed except for discoloured spots, but in spite of contrast baths, &c., the paralysis remains and the leg still swells if allowed to hang down. In July last I stripped off the coats of the popliteal artery but up till now this has had no effect. The artery is normal in size and there is good pulsation in the posterior tibial artery. I take this to be a case of vasomotor disturbance with associated functional paralysis. I should be glad to hear views as to diagnosis, treatment and prognosis. I may say that the possibility of malingering and dermatitis artefacta have been excluded to my satisfaction.

### DISCUSSION.

Mr. MAYNARD HEATH related his experience in treating a case of gangrene due to thrombo-angiitis obliterans. He had first injected alcohol into the coats of the femoral artery and later resected the coats of that artery following the method described by Sampson Handley. Very little relief of symptoms had resulted. Large doses of citrate of soda were then given intravenously and by the mouth. Great improvement in the circulation and alleviation of the pain ensued. He suggested that citrate of soda might be tried in the present case.

Mr. BINFORD BARNETT said that in the somewhat similar condition of thrombo-angiitis obliterans described by Buerger, the intramuscular injection of iodipin had been found to yield good results.

Mr. W. H. OGILVIE said that a paper by Professor J. B. Stopford upon causalgia, which appeared in the *Lancet* towards the end of the war, had some bearing on the case. In this article it was pointed out that the nerve supply to the coats of an artery did not run down the wall as a periarterial sympathetic plexus, but passed to the vessel from the nerve trunks at different levels all the way down the limb. Stripping the coat of the popliteal artery would therefore affect this vessel only, and would do no good if the vascular lesion was in one of the tibial arteries. Again, Mr. Sampson Handley had shown that periarterial injection of alcohol produced a more lasting vaso-dilatation than stripping off the coat. He (Mr. Ogilvie) therefore suggested, that before having recourse to radical measures, it would be worth trying the effect of injecting alcohol into the coats of the anterior and posterior tibial arteries.

Mr. JENNER VERRALL (in reply) said that the patient had been seen by a neurologist and a physician who had specialized on arterial lesions and these two had concurred with his diagnosis. To the best of his knowledge the patient was not a Jew and did not smoke excessively. He (Mr. Verrall) was well acquainted with thrombo-angiitis obliterans and had in fact operated on such a case quite recently but he would suggest that in this case there was no diminution in size or stenosis of the arteries.

He thanked one speaker who had suggested amputation; this suggestion agreed with his own views, especially as the time factor was of importance.

**Case of Cyst of Humerus with Fracture.**

By H. A. T. FAIRBANK, D.S.O., M.S.

PATIENT, a male, J. M., aged 11 $\frac{3}{4}$ .

*History.*—On August 11, 1923, while riding in a cart, he fell against the side of it. On August 15, he was brought to hospital with a fracture about the junction of the upper and middle thirds of the shaft of the humerus.

*X-ray* examination showed an oblique fracture with a moderate degree of displacement and good alignment. The fracture was seen to pass through the centre of a clear area occupying the shaft of the bone, which was slightly enlarged and the cortex thickened. The clear area has fairly sharply defined margins above and below, and is strongly suggestive of fibrocystic disease.

He was treated for the fracture in the ordinary way by my assistant, Mr. Batten.

*September 19.*—Examination showed that firm union had taken place.

A subsequent X-ray examination has shown good union with ample callus. No very obvious change in the cystic area.

It is proposed to postpone operative treatment for a time in the hope that the fracture may eventually lead to the arrest of the disease.

Mr. BATTEN stated that he had a case of cyst of the humerus when he was dresser and that was put up on a splint and union took place rapidly. He saw the boy some eighteen months subsequently and he was quite well then, and that was the reason why he had treated the present patient, shown by Mr. Fairbank, in that way.

## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### Case of Snapping Knee.

By R. C. ELMSLIE, O.B.E., M.S.

PATIENT, a girl, aged 19, fell and broke her left arm in September, 1922, and she thinks that she hurt the knee at the same time. The joint was stiff for a time afterwards, and she dates the snapping from then.

At present the knee looks normal, but on flexion, when it reaches an angle of about  $135^{\circ}$ , an audible snap occurs, the tibia rotating inwards on the femur and a movement being detectable on the outer side of the joint at the level of the top of the head of the tibia. On extension a similar snap with movement of the tibia in the opposite direction occurs. It appears that there is some rotation movement in which the external semilunar cartilage alters its position.

In four previous cases, with a similar snap, upon which I have operated, removal of the anterior two-thirds of the external semilunar cartilage cured the condition.

### Case of Maternal Obstetrical Paralysis.

By E. LAMING EVANS, C.B.E., F.R.C.S.

H. O., FEMALE, aged 30, was first seen at the Royal National Orthopædic Hospital in September, 1923.

*History.*—Five months previously she was delivered of a living child, weighing 6 lb., by high forceps operation after fourteen hours of labour. Dr. Felce reports that there is a slight degree of general contraction of the pelvis, which is present also in her two sisters. The child was apnœic, and required fifteen minutes' artificial respiration after delivery. Upon recovery from the anæsthetic, she noticed numbness and sense of weight in the whole of the right lower extremity. These gradually diminished during ten days. The puerperium was normal without pyrexia or any sign of infection. On leaving her bed, patient noticed shooting pain along the back of the thigh and outer side of the leg down to the outer side of the foot, also a dropping of the foot at the ankle-joint. The pain lasted four months.

*Examination.*—When patient was first seen, five months after delivery, she showed complete foot-drop; all voluntary power in the muscles supplied by the external popliteal nerve was absent, also some weakness in the flexors of the

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toes. No sensory changes were present: no tenderness on pressure of nerve trunks was elicited.

X-ray examination showed normal lumbar spine and sacro-iliac joints. Wassermann's reaction was negative. Electrical reactions carried out by Dr. Rawlinson showed no response to faradic stimulation through muscles supplied by the external popliteal nerve. Galvanic response was fairly brisk, but not suggestive of a total lower neuron lesion.

*Treatment* has consisted in relief by extension of the paralysed muscles by a toe extension spring during the day, and a right-angled retention shoe at night. Massage and galvanism have been given three times a week.

After six weeks of treatment and six and a half months after the onset of the initial lesion, there is seen to be a definite recovery in the tibialis anticus, as evidenced by voluntary action.

The following points summarize my view of this case: (1) That it belongs to the category of maternal obstetrical paralyses; (2) that the lesion is a rupture or ischæmic necrobiosis of the fibres of the fourth and fifth lumbar nerves constituting the sacro-lumbar cord as they pass over the brim of the pelvis; (3) that those branches which lie directly upon the bone suffer the greater damage and these fibres constitute the external popliteal nerve; (4) that those fibres of the sacro-lumbar cord which lie on the anterior surface of the cord, and constitute the internal popliteal nerve, are less liable to compression, and therefore usually escape permanent damage. In the recorded cases, the external popliteal nerve is involved in by far the larger number of cases. In this case, the flexors of the toes were weak, so that a few fibres of the internal popliteal were involved.

### Case of Congenital Dislocation of the Right Radius.

By E. LAMING EVANS, C.B.E., F.R.C.S.

G. T., MALE, aged 5 years 4 months, was brought to the Royal National Orthopaedic Hospital, in October, 1923, on account of deformity of the right elbow. It was first noticed when he was 3 months old. There is no history of any trauma.

The head of the radius can be felt to be displaced upwards and backwards and this displacement is confirmed by X-ray examination.

The function of the elbow is incomplete. Full flexion is possible, but extension is diminished by 30°. Pronation is complete, but supination is checked in the mid-position.

The usual treatment by removal of the head of the radius and forcible correction, followed by active and passive movements, has not always been attended with success, and although this deformity is comparatively uncommon, I show this case more with a view to soliciting the opinions of the Members of this Section as to their experience of the results of the treatment indicated, and as to any alternative method of treatment.

### **Transplant of the Palmaris Longus into the First Metacarpal.**

By D. McCRAE AITKEN, F.R.C.S.

PATIENT, a male, aged 16. An old case of infantile paralysis with complete loss of power of opposing the thumb.

*Operation.*—The tendon of the palmaris longus is exposed in front of the annular ligament and a strip of its extension into the palmar fascia is isolated. If this strip is well developed it will be found long enough to reach the proximal part of the shaft of the first metacarpal in the interval between the abductor and the opponens and flexor brevis pollicis. The palmaris tendon is sutured to these muscles and to the periosteum. In this instance there was a definite aponeurotic streak in the short muscles which helped in getting good fixation.

If the palmaris tendon is not long enough the extensor brevis tendon is brought round and the operation described by Ney<sup>1</sup> is performed. The result, in the case shown, is satisfactory.

### **Case of Congenital Subluxation of Both Hips.**

By P. JENNER VERRALL, F.R.C.S.

PATIENT, a girl, aged 10. No symptoms till age of 6. Then slight limp noticed in right leg. No pain until quite recently. When seen: left hip, free and painless movement in all directions; right hip shows slight limitation in abduction only. Pain on movement slight, if any. Space between thighs increased. Trendelenburg test doubtful. X-ray shows subluxation of both hips upwards, heads of femora possibly flattened but to a very slight degree, if at all—quite unlike a coxa plana. Upper edge of acetabulum slightly fluffy on both sides.

The case is shown on account of the interest of the condition and to obtain views as to treatment required, if any.

The only possible treatment would be by extension caliper splints. These are expensive and the child walks fairly well. My own opinion is against any treatment or use of appliance, but I should like the opinions of others.

### **Two Cases of Absence of the Fibula.**

By K. J. ACTON DAVIS, M.Ch.

*Case I.*—Child one of twin boys; other twin normal. Breech presentation. Seen four hours after birth. The right tibia was bent at right angles in the centre with a dimple over the angle. Fibula, two outer metatarsals and corresponding phalanges absent. Foot in calcaneo-valgus position.

Skiagram shows a fracture of the right femur united in good position, and a fracture of the right tibia in position of angle. Both firmly united.

*Case II.*—Case of Mr. R. A. Ramsay. Child 6 weeks old. Angular deformity of tibia with dimpling of skin and absence of fibula.

<sup>1</sup> *Surg., Gyn. and Obst.*, 1921, xxxiii, p. 342.



**Case of Rheumatic Fever affecting a Paralysed Limb.**

By K. J. ACTON DAVIS, M.Ch.

L. M., GIRL, aged 11. Suffered from infantile paralysis affecting the right lower limb which is completely flail. Pain and swelling appeared in the right knee. Chorea afterwards developed and patient has left a mitral lesion.

**Case of Spastic Paraplegia.**

By G. PERKINS, M.Ch.

PATIENT, a boy, aged 11, says that eight years ago a cart wheel ran over his right ankle; since then he has had difficulty in walking. Some years ago the right tendo Achillis was divided in a country hospital.

On examination the boy appears to be of normal mentality. There is marked spasm of the whole of the right leg. The hip is held in flexion and adduction, and the muscular spasm cannot be overcome by passive force. At the knee there is full active range: and at the ankle full passive range is possible, but only about three-quarters active range. The deep reflexes are increased, and the plantar reflex is extensor. On the left leg the plantar response is also extensor, but there is very little spasm of muscles, and full active range is possible at all joints. The arms are normal. Athetosis is absent. He walks with a stiff hip, flexed and adducted, and with a valgoid foot.

This is presumably a case of spastic paraplegia, in no way related to the injury of eight years ago.

Mr. H. A. T. FAIRBANK (in discussing treatment) said he would advise operating on the obturator nerve. The valgus of foot could be corrected by a tendon or a nerve operation, preferably the latter.

## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### DISCUSSION ON MYOSITIS OSSIFICANS TRAUMATICA.

Sir GEORGE MAKINS.

THE brief remarks which I shall make in opening this discussion will not, I fear, materially add to our knowledge of the affection known as traumatic myositis ossificans, but I propose to make certain dogmatic statements, and also an attempt to define more sharply the various conditions which have been from time to time included under the designation.

First as to the name itself: its appropriateness has often been questioned, and it is an obvious misnomer, since the process is not to be regarded as inflammatory but rather as an instance of redundant and misdirected regeneration or repair, while the muscle itself plays a purely passive part.

Various names have been substituted: thus, traumatic osteoma, myosteoma, intramuscular osteoma, and lastly, and perhaps least satisfactorily, periosteal osteoma. These names, although corresponding with a certain stage of the development of the masses of bone also seem inappropriate, in that they suggest that the tumours are of the nature of a new growth, while we know that the ordinary history is that of the development of a more or less evanescent bony mass, in no sense corresponding with a true tumour of the bone. The history and development of the masses of bone correspond indeed exactly with that of the provisional callus which takes part in the repair of a fracture of the bone.

#### CAUSATION AND DISTRIBUTION.

It is unnecessary to dwell upon the direct causation of the condition; we know that one essential requirement is a lesion of the muscle due either to a blow, a strain, or over-action, further that the actual injury may not be severe in degree.

#### LOCALIZATION.

The most common situations are either within the sheath of the quadriceps extensor of the thigh, or the brachialis anticus muscle. Less common situations are the pectoralis major, the deltoid, the triceps, the biceps cubiti, the subclavius, the adductor magnus, the iliacus, the soleus, the masseter, &c. In fact almost any muscle attached to bone may be affected, and during the War I saw at least two cases in which as a result of general bruising from the explosion of a shell, considerable masses of bone developed in the muscles of the trunk—a site for the most part confined to the ossifications taking place in myositis ossificans progressiva.

At this stage it is advisable briefly to consider the several conditions which have been from time to time included under the heading of traumatic myositis ossificans, and to see how far this classification is justifiable.

(1) We may first turn our attention to the ossification of the occupational class, spoken of under the names of "rider's bone" (adductor longus), "fencer's bone" (brachialis anticus), "dancer's bone" (soleus). This class of ossification in muscles should be sharply distinguished. The development of the bone starts at the point of origin of the muscle and is excited and encouraged by over-use or irritation of the muscle concerned. It may properly be regarded as an abnormal and exaggerated increase of the process by which the normal muscular ridges and eminences are gradually developed up to the period of maturity of growth of the body. It is a gradual process in no sense comparable or identical with a rapidly developing ossification in the quadriceps or brachialis anticus consequent on an injury to these muscles and the bone to which they are attached, and the bony outgrowths show little tendency to re-absorption.

(2) The metaplastic ossifications which invade the muscles in certain forms of disease, notably tabes, with or without a history of injury, the relation of which to myositis ossificans progressiva would appear to be much nearer than to the traumatic form.

(3) It may be borne in mind that masses of callus in the connective and fibrous tissues surrounding the articulations, frequently increase and complicate the limitation of movement which may in part depend on traumatic myositis ossificans of a torn muscle, e.g., brachialis anticus; and there can be little doubt that the cause of the superabundant callus in the fibrous tissue is identical with that leading to ossification within the substance of the muscle.

In my opinion the name traumatic myositis ossificans (or an appropriate substitute for it) should be limited to a condition consequent upon an injury in which the following three factors are associated:—

(a) An injury to the periosteum or bone, of sufficient severity to allow of the escape of bone-cells into the neighbouring muscle sheath.

(b) Hæmorrhage from the bone and surrounding tissues.

(c) An injury to the muscle sufficient to open its sheath.

Clinical observation has demonstrated the regular presence of these three factors, and their capacity to produce the condition has been experimentally proved by Mr. John Morley and others.

We may now briefly consider the part taken by each factor in the development of the condition.

(1) The injury to the periosteum allows of the escape of bone-cells, the escape being facilitated by bleeding from the Haversian canals.

(2) The extravasated blood affords a suitable medium for the preservation and multiplication of the bone-cells. It further acts mechanically by opening up tracks in the substance of the muscle and carrying the bone-cells with it into these tracks. Acting as a foreign body it excites an immediate reaction in the surrounding muscular tissue favouring the active development of the cicatricial connective tissue necessary to form the groundwork of the future bony mass. Lastly, the formation of a hæmatoma is occasionally responsible for a cystic character of the tumour.

(3) The injured muscle affords a richly vascularized bed favourable to the rapid development of the foreign tissue.

The sheath of the muscle forms a limiting boundary to the tumour, while the arrangement of the muscular bundles determines the architecture of the

new bony formation. Thus both the shape and structure of the tumour are due to its location within the muscle.

#### MORBID ANATOMY.

With the gross anatomy of the tumours we are well acquainted. Within the sheath and the substance of the muscle we find in the fully-developed condition a mass of newly-formed connective tissue surrounding cancellous bone; the mass is intimately connected with the surrounding muscle, and is usually continuous with the shaft of a long bone. In some instances a synovia-like fluid is found in bursal spaces, in the surrounding tissue, and in others the tumour may consist of a large cyst containing fluid. The latter form is of special interest, since the walls of the cyst are formed of connective tissue and bone, and correspond exactly—except in not being connected with the artery—with the form of traumatic aneurysm sometimes met with when the original arterial injury is complicated by the presence of a neighbouring fracture of a bone. The identity of the two conditions well illustrates the minor part taken by the muscle in the so-called traumatic myositis ossificans.

Microscopical examination of the tissues reveals in the early stage mainly connective tissue in the condition of active proliferation. Scattered amongst this tissue may be found chondroblasts and osteoblasts suggestive of metaplastic bone formation, and numerous bone-cells no doubt derived from the existing injured diaphysis. The surrounding muscular fibres are in varying stages of degeneration, while signs of regenerative changes are seen in the multiplication of the nuclei. Later, a well formed mass of cancellous bone is enclosed in fibrous tissue undergoing cicatricial changes.

So far, then, as the morbid anatomy and the clinical history of the condition are concerned, we are in possession of sufficient practical knowledge, but we are still ignorant as to the pathological conditions which favour the development of the tumours in minor injuries to the bone and muscle in one patient, while in the majority of instances the repair of the injury proceeds on normal lines.

Help in this direction might have been hoped for from the occurrence of a spontaneous disease in which ossification of the muscles is the main feature, but in spite of the fact that this disease has long been recognized, we are still in a position of complete ignorance as to its true pathology.

Over-production of callus in connexion with fractures of the bones has been attributed to irritation due to want of fixation of the fragments, or to the occurrence of infection; while deficiency of callus has been attributed to the absence of extravasated blood and consequent failure of reaction in the surrounding tissues, to defective fixation, or to the co-existence of some constitutional disease. The development of large masses of callus in connexion with comparatively trivial injuries to the bones, in only a strictly limited proportion of such injuries, points to the existence of some special constitutional condition or idiosyncrasy.

Such a constitutional condition was suggested by Virchow as "*Diathesis ossificans sive ossea*." The assumption of such a diathesis carries us, however, little further. The suggestion has been made that the calcium content of the blood is in excess in these conditions, but I am unaware that the suggestion has been proved. Some recent observations by M. Cohn have led him to suggest that the calcium salts deposited in provisional callus are not derived from the blood stream, but are conveyed by the lymphatic system directly from the distal fragment of the fracture, and he states from observations with the

X-rays that the deposition corresponds in amount with the loss of salts accompanying the rarefaction of the distal fragment which usually occurs.

It is noteworthy in this connexion to recall the large masses of bone which occasionally develop in the muscles in tabes dorsalis, because in this condition the ossification in the muscle is usually accompanied by atrophic and rarefying changes in the bones. The fact that tabes is a late manifestation of syphilis, hardly helps us towards the causation of the condition we are considering, but the intramuscular ossifications are of interest since we are aware that injury to a muscle, even of such insignificance as the prick of an injection needle introduced for treatment by salvarsan, may be an exciting cause of their formation.

It would appear that further information as to the actual pathology of the condition is to be hoped for either in the direction which has been demonstrated of the influence of certain glands in controlling the growth of the body (e.g., pituitary gland), or of the effect of the association of certain tissue elements in influencing the definite arrangement of specialized cells, as shown in Drew's culture of renal cells in association with connective tissue.

#### DIAGNOSIS.

In the light of our present knowledge, and the means at our disposal, the diagnosis of this condition can hardly be held to be one of difficulty. The uniform history of primary injury, and the regular course taken by subsequent events are in themselves suggestive, while the characteristic structure presented by an X-ray plate is sufficient to rule out any suspicion of sarcoma in either of its varieties.

#### TREATMENT.

The primary treatment in all cases should be complete rest and avoidance of anything liable to increase the vascularity of the affected part, since we are well aware that the general tendency of the tumours is to undergo spontaneous absorption, and to leave no serious trace behind them. The bony masses may become absorbed less rapidly, but difficulty chiefly arises in instances where the ossification in the muscle is associated with ossification in the fibrous tissue structures around a joint, as in the elbow, when operative treatment may become necessary at a later stage.

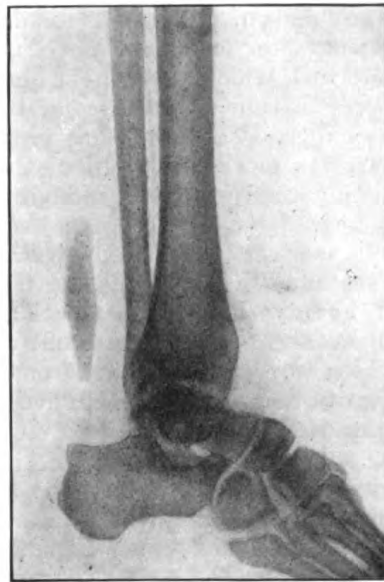
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#### Mr. R. C. ELMSLIE (President).

Experience has shown that traumatic myositis ossificans occurs in a very large variety of situations and in many different muscles. Around the knee it is common in different sections of the quadriceps and occurs also in the capsule of the knee-joint. At the elbow it occurs in the triceps and supinator brevis, as well as in the brachialis anticus. Recent cases seen show ossification

in the supraspinatus, subscapularis and teres minor around the shoulder, and in the insertion of the psoas as the result of a forcible hyperextension of the hip-joint. Two slides, which have been kindly sent for exhibition at this meeting by Mr. S. T. Irwin, of Belfast, show an ossification in the tendo Achillis of a man aged 40, following a tenotomy of the tendon in childhood; the ossification was discovered accidentally in a radiograph taken for an injury to the ankle (*see fig.*).



As to the cause, there is a general agreement that forcible stretching carried out in the course of treatment after an injury to a joint or muscle is an important factor, either in the original production of the ossification or in exacerbating the condition. It is probable that in some cases partial avulsion of muscle from bone without any general or joint injury may lead to subsequent ossification in the muscle attachment. It is possible also that in certain cases an ossification really occurs in a hæmatoma. Sepsis perhaps is an occasional cause. In a case operated upon during the war the hip-joint in an amputation stump was completely fixed by an ossification of the entire gluteus medius muscle, which followed a small pyæmic abscess in the buttock.

Sir George Makins has mentioned the possibility that metaplasia of fibrous into osseous tissue constitutes a factor in the pathology. The occasional occurrence of cases in which there is no clear evidence of injury even to the surface of the bone would point to this, and we must remember the essential unity in nature of the osteoblast and the fibroblast.

In treatment, everyone is now agreed upon the necessity for a prolonged period of rest, and the only question is, if an operation has to be undertaken, at what period it should be carried out. Early operations are almost invariably followed by recurrence, and an operation should not be undertaken until the bone structure has ceased to show any variation in consecutive radiographs,

and then only in order to remove a mechanical blocking of movement of the joint. The time from the occurrence of the bone until an operation is carried out must necessarily be long; only very seldom will an operation be required before the end of a year.

Mr. W. ROWLEY BRISTOW

showed slides illustrating myositis ossificans in the vastus internus muscle, from a patient in whom the condition had arisen with no history of trauma. The onset had been characterized by severe pain in the thigh, which had come on suddenly for no apparent reason, and then quietened down. Two years later he had a similar attack, after which a mass of new bone formation was discovered in the vastus internus, which was tender on pressure.

The condition at the present time—that is, some thirteen years since the onset, showed very little change. The new bone in the vastus internus was palpable and tender. There was marked atrophy of the muscles of the thigh, but the patient suffered little inconvenience, although flexion of the knee was limited at about 90°.

Mr. Bristow showed a further set of slides illustrating the case of a patient—a child of 5—who had sustained a supracondylar fracture of the humerus, which had been followed by myositis ossificans. This condition had been treated by keeping the elbow at rest in flexion, and very gradually allowing extension by dropping the sling progressively from week to week. The series illustrated absorption of the new bone until, five months after the onset, a thin linear bone-scar alone remained, and the recovery of function was complete.

He (Mr. Bristow) emphasized the point that, in his opinion, it was very necessary that information as to the correct immediate treatment of fractures and dislocations about the elbow should be ventilated, as it was clear that the profession as a whole did not realize the necessity of rest for these conditions. Myositis ossificans was a complication which could probably be prevented in the majority of instances, if rest, rather than movement, was insisted upon. He doubted the value of operative interference for the removal of the mass of new bone at any stage. They were all agreed that early operation was definitely contra-indicated, but even in the later stages, in his experience, operation was unsatisfactory. Whilst it was perfectly simple to remove the bone and obtain a movable elbow-joint at the time of operation, there was a grave tendency for the condition to recur even after the most complete operation. Whilst the removal of an isolated osteophyte was likely to be successful, the removal of the mass in true traumatic myositis ossificans yielded disappointing results.

Mr. H. A. T. FAIRBANK

said that with regard to the shoulder cases mentioned by the President, he had seen a few of these cases, and although the calcareous deposits, often two or three separate nodules, seemed to be in the supraspinatus he did not consider them as really cases of myositis ossificans. In one case (that of a surgeon) he had advised the patient to leave things alone and not have an operation. The surgeon shortly afterwards went to Vienna and was there shown a number of radiograms similar to that of his own shoulder and was assured that the opacities would disappear. Recently the surgeon had sent him a radiogram showing that the opacities had disappeared from his shoulder. Mr. Bristow had touched on the question of the teaching of the treatment of cases of injury

about the elbow. He (Mr. Fairbank) considered the knowledge of the general practitioner on this point was not good. He had seen many cases in which unduly violent efforts to obtain movements in the joint after a supracondylar fracture of the humerus had produced myositis ossificans. He regarded every case of injury in this region as a potential case of myositis ossificans, and insisted on return of movement in the joint being obtained by *active* movements only. Massage did no harm, but passive movements were better avoided. The only case, of those he had shown, which seemed worthy of special mention was one in which the myositis followed an operation.

A boy of 11 years had a rounded lump, the size of half an orange, on the inner side of one knee, with two or three other nodules in the quadriceps just above the limits of the suprapatellar pouch. All the tumours turned out to be nævi containing shot-like calcareous nodules. Two in the region of the head of the fibula were still visible in the radiograms. The gaps in the muscle left by excision of the small nævi were closed with catgut stitches. About three weeks later, on getting up, he slipped and flexed his knee fully before the muscles had completely recovered their elasticity. Two weeks later he reported because the knee was still rather stiff. A plaque of bone was felt closely surrounding the anterior and outer aspect of the femur just above the knee-joint. Rest on a back splint for seven weeks produced absorption of most of the bone, a radiogram then showing a typical bone-scar. Perfect movement eventually resulted.

He (Mr. Fairbank) was struck by the fact that the radiographic shadow in the earlier stages always showed a clear space between the mass and the shaft of the parent bone, while in the later stages a typical subperiosteal thickening of the parent bone was seen—the so-called bone-scar. He had been induced to operate on one quadriceps case, the result of a kick on the thigh. The periosteum appeared to be intact over the femur though the bony lump was closely applied to it. In this case the mass was re-forming within a fortnight, though the flexion of the knee was much improved. He would not again operate on such a case.

#### MR. GORDON PUGH

referred to a case of myositis ossificans affecting the triceps, brachialis anticus and supinator brevis which he had brought before the Section (then the Sub-section) in December, 1919.<sup>1</sup> A few days later the elbow was placed in plaster in the rectangular position. After eleven months in plaster, that is, thirteen months after the accident, the movement at the elbow-joint was still limited to a few degrees, and a skiagram showed there had been but little absorption of the bone. The bone filling up the olecranon and coronoid fossæ was therefore freely removed, as was also a spur running between the radius and humerus. Four months later, when the boy was last seen, flexion movement amounted to 40°, and some rotation was possible.

#### MR. ALAN H. TODD

described a case of ossification upon the front of the femur, situated apparently in the lowermost part of the origin of the crureus. The patient was a woman of 25, who had an oval loose body lying just beneath the middle of the patella; the knee was opened and the loose body found to be attached by a slender thread, apparently not vascular, to the synovial membrane of the intercondylar space. The loose body was symmetrical and oval, composed of intermingled cartilage and bone; nothing was seen that suggested that it had originated in

<sup>1</sup> *Proceedings*, 1919-20, xiii (Sect. Surg., Sub-sect. Orth.), p. 110.



any injury of articular cartilage or bone. An unusual amount of stiffness of the knee followed the operation, and it was found that a mass of new bone had formed upon the front of the femur, probably in the origin of the crureus. The limb was completely immobilized, and subsequent X-ray examinations revealed the gradual increase in amount, density and sharpness of delimitation of bone. A year after operation a full range of movement had been restored. Mr. Todd also showed a second series of radiograms, those of a man of 43, injured in the elbow region, without fracture, in a motoring smash. The ossification that had followed mapped out with remarkable accuracy the precise attachments of the anterior and posterior ligaments of the elbow-joint and the orbicular and other ligaments of the radio-humeral and radio-ulnar joints. Mr. Todd observed that though these ossifications following upon injury had been spoken of as "callus," it appeared to him that this term was used upon totally inadequate grounds. It seemed to him to be preferable to term these formations simply "post-traumatic ossifications." Again, why was there such an extraordinary preponderance of these ossifications in the region of the elbow? Elbow injuries were very common in growing children, but that was not enough to explain the great frequency of post-traumatic ossification in this situation as compared with all others. It seemed to him that an anatomical peculiarity of the elbow-region was probably the true explanation. The islets of cartilage-cells and sometimes of bone-cells found widely distributed through the synovial membrane and even the capsule of the elbow- and radio-ulnar joints were not confined to growing children; they were to be found, also, in adults, just as many other "rests" of embryonic tissue were to be found in persons in whom growth had long ceased. Post-traumatic ossifications, apart from gross bone-injury, in adults, were probably examples of activation of rests of chondroblasts or osteoblasts.

#### Mr. C. LAMBRINUDI

showed and described the following case for Mr. W. H. TRETHOWAN.

Miss S., aged 22.

*January 1, 1923.*—"Sprained" her knee in jumping off a chair. Leg was kept at rest in bed without massage or exercise for four weeks. First seen four weeks after the accident. The knee looked normal, but was fixed and painful on attempts at movement. Adhesions diagnosed. Under anæsthesia the knee was mobilized with the very minimum amount of force. Lifting the limb with the hand behind the knee was sufficient to flex the leg. Active and passive movements were begun the next day. Within three to four days the knee became swollen and "boggy" and the characteristic painless and increasing stiffness on attempts at movement suggested myositis ossificans, which was confirmed by X-ray.

*Points of Interest.*—(1) When first seen the amount of fixation after a simple sprain in a patient otherwise normal; (2) the rapidity with which the myositis ossificans appeared after the manipulation, which was of the gentlest kind; (3) the bone appeared in both heads of the gastrocnemius and at the insertion of the ligamentum patellæ into the tibia.

Mr. Lambrinudi said that Mr. Trethowan would like to know if the meeting agreed with him in recognizing a type where the physical signs, symptoms and responses to treatment suggested the diagnosis of myositis ossificans, but where there was no X-ray evidence of new bone formation.

#### Mr. P. JENNER VERRALL

showed a case (with skiagrams) of a girl aged 21, seen three weeks after a minor fracture of the internal epicondyle of the humerus. In the front of

the elbow there was a hard mobile tumour, sharply defined and of the size and shape of a walnut and a filbert placed end to end. Elbow movement was free except for the loss of  $20^{\circ}$  of extension. The lump was unsightly and growing rapidly. It was therefore excised and found to be composed of spongy bone, encapsuled and lying in the substance of the brachialis anticus, but not attached to bone. Subsequently the elbow was immobilized in plaster for two months and afterwards treated by massage, &c. Elbow movement was now good and skiagrams showed only a very small bony flake remaining, in which there were no signs of further growth. He considered this a case of rapid bony growth in a hæmatoma.

Mr. ACTON DAVIS

showed a man, S. J., aged 19, who received a kick in the left thigh while playing football in January, 1923. He was seen six weeks after the injury with a large mass of bone formation in the vastus externus.

He played football again in the season 1923-24. No disability was felt as the result of the pathological condition. He had been kicked in the right thigh ten days previously to being seen again, and had a large hæmatoma, which was now very tender and showed bone formation again in the vastus externus. The condition in the left thigh was unaltered and there was no difference visible in X-rays taken in March, 1923, and December, 1923, respectively.

Mr. W. E. TANNER

showed a bony plate which had developed in a median epigastric laparotomy scar. The bony plate measured 4 in. by  $1\frac{1}{2}$  in. The anterior surface was smooth, the posterior surface was attached to the rectus muscle. The anterior surface was attached to the anterior sheath of the rectus and had a layer of cartilage beneath a sheath of fibrous tissue.

Similar specimens had been described growing in scars following suprapubic incisions for exploration of the bladder.

It appeared probable that at the operation cartilage cells and bone cells had escaped into the soft tissues, from division of the ensiform cartilage or pubic bone. These cells growing in the lines of stress of the young fibrous tissue formed longitudinal tension lamellæ downwards from the ensiform or upwards from the pubes.

On the front of the elbow the osteoblasts grew in a semicircular direction corresponding to the line of stress in the muscles when the elbow was flexed and extended.

Mr. W. B. FOLEY

described a case of myositis associated with new bone formation. The patient received a bomb wound in the right upper arm, in May, 1921. A piece of metal was removed a short time later. A tourniquet was not used. Three days later the whole arm became acutely painful and swollen and incisions were made under the impression that suppuration had taken place in the muscles. No pus was found. Repeated attacks of this nature, usually preceded by slight injury, had occurred. Several incisions had been made by other surgeons but no pus had been evacuated. In September, 1921, there was a formation of new bone in front of the humerus. It produced limitation of flexion of the elbow and was excised. The piece of bone was said to have

measured  $5\frac{3}{4}$  in. in length. The attacks of inflammation continued and in 1922 the X-ray showed a recurrence of bone formation.

The Wassermann reaction in November, 1922, was strongly positive. An intensive course of N.A.B. was given.

Limitation of movement in the elbow increased and in August, 1923, a bony mass was removed. It was found to be submuscular and attached to the humerus. A specimen of muscle also removed showed a fibrotic condition with calcareous deposits. As a result of the operation range of movement was much improved.

A recent X-ray picture showed slight growth of new bone. There had been one slight inflammatory attack since the last operation.

## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### The Principles of Early Active Movement as Applied to the Treatment of Fractures of the Upper Extremity.

By J. W. DOWDEN, F.R.C.S.Ed.

(Introduced by Mr. R. C. ELMSLIE (President).)

THE principle of early active movement in the treatment of practically all injuries and most inflammatory conditions will assuredly come to be the method adopted in the not distant future.

The anatomical study of a limb reveals that it is constructed entirely for active use.

The muscles clothed in the fascia terminate in the silvery tendons which glide where needed in special sheaths. The joints with their smooth, resilient cartilage, synovial membrane and lubricating fluid, all ask for active movement. The bones, which reflect the musculature of the individual, are all evidence of their need and use. In the strong, the bone is heavy and ridged for the attachment of muscle, and where there is disuse the bone is light and featureless, and atrophy from want of use sets in rapidly. The vascularity of the limb depends on the amount of use, and the attuning of the nerves is at its best in a muscular limb; they carry the messages to the muscles from the brain, and to the brain is conveyed the sense of pain which is the great guide in the treatment by active movement, which should always be short of pain.

When a fracture takes place there is more or less displacement from either direct or indirect violence, and that displacement is due to the force which causes the fracture and not to the muscles. In addition there is more or less damage to the soft parts and blood is effused widely. Clotting soon occurs and repair is begun, and ultimately fibrous tissue forms and binds the soft parts together as well as the bone in the position in which the limb is most constantly kept. This repair goes on day and night, and the longer the limb is kept in one position the longer it takes to restore it to use, and then only after the lapse of time and occurrence of pain.

#### TREATMENT.

A great advance was made by Lucas-Championnière, who rightly said that movement was life; but in the treatment of fractures he utilized another individual's brain and muscles, namely, the masseur's; for all that, however, it was a move in the right direction.

What does the average patient want? The most useful limb in the shortest time. As a rule the shape will not matter so long as it is useful, and that is the

### 30 Dowden: *Treatment of Fractures of the Upper Extremity*

great object in a wage-earner, not only for himself and those dependent on him, but for the State. Early active movement will give this result. The better the alignment of the bones, the quicker the results, and therefore an effort may be made to get the bones into good alignment, though as a rule this is not very successful. Splints applied to a limb swollen with effused blood will very rarely keep the bones in position, as the pressure needed will be too damaging. Operation is urged by some, but in the hands of the general practitioner operations will not be associated with the same good results as active movements. Operators are too apt to keep the limb immobilized, but operation in the hands of a skilful surgeon, together with active movement, might open out a great advance in getting early good results. The results of active movement are not only excellent and rapid, but the best that I have seen. Many most deplorable results from immobilization have come under my care both before, during and after the war. In septic cases active movement "pumps" the pus out from the sinuses, and most of the results are extraordinarily good.

As to the actual method: the arm is put in a sling and heroin given for the first few nights. Within twenty-four hours active movement, short of producing pain, is begun, such as flexion of the fingers and movements of the arm so far as it is possible. It must be frequently carried out, and the degree of movement should steadily increase in all directions, the important rule being borne in mind that it should do so short of pain, but "following on the heels of pain." All the joints should be exercised as far as possible, and the patient should have the arm out of the sling and should lower it and raise it with the other hand, exercise abduction and adduction, and otherwise employ passive, personal, intelligent use of the limb.

Certainly in children marked deformity of the bone can be ignored, as time will remedy that and restore the appearance of the bone practically to its normal state. The main thought should be of the muscles and joints, and not of the fractured bones. The fracture is the only one lesion that is obvious in an X-ray picture.

In one case only during more than twenty years has there been the occurrence of a false joint, and that was in a very bad fracture of the clavicle. The man rows, however, in a racing boat, and plays most games and suffers practically no inconvenience; and he does not wish an operation to fix the fragments. There were only two cases of delayed union in the humerus, but these were cured by bumping the elbow on the padded arm of a chair.

Splints may be useful at night at the beginning, when the patient may roll over in sleep and hurt the arm, also in a few odd cases to protect the patients from injury when walking about among many people; occasionally in the case of a sagging fracture of both bones of the forearm; and at times in that of a bad Colles' fracture as well.

Massage and passive movements are helpful in fractures about the elbow-joint, especially in the later stages.

#### INDIVIDUAL FRACTURES.

##### *Fracture of the Clavicle.*

Here there is overlapping. Most methods of treatment by bandages demonstrate that the triple deformity—downwards, forwards and inwards—can be overcome by such means. The experiment is most convincing when carried out on a sound limb, but in a fractured limb it results in still further

overlapping. A sling and early movement will usually result in excellent movement in a few days, and a clerk can go back to his work in about a week.

*Fracture of the Scapula.*

This is a rare fracture, but when treated on the same lines as the last the results are excellent.

*Fracture of the Neck of the Humerus.*

This is a comparatively common fracture, and the treatment for it is the same, though in adults an abduction splint at night might be useful in order to ensure movement at the shoulder-joint. Not only are the results excellent, but in the young the restoration of apparently impossible bone lesions to a normal appearance by X-rays is almost miraculous; and most advocates for operation would be proud not only of the early functional result but of the X-ray appearance as seen later.

*Fracture of the Shaft of the Humerus.*

This bone lends itself well to the type of treatment. Overlapping fractures may be helped by a sling supporting the wrist and extension weight just in front of the elbow. Young people do best, and the important fact is that in their case not only is the movement good in about three weeks' time, but in a few years the restoration of the bone is such that there is no evidence that a fracture had ever taken place.

In the early stages a posterior stiff splint projecting beyond the elbow is useful at night in order to prevent the arm being lain upon, with resultant pain. Delayed union is treated by "bumping" the elbow on the padded arm of a chair frequently during the day. If there is paralysis of the musculospiral nerve (radial), operation should be done at once.

*Fracture in the Region of the Elbow-joint.*

The treatment is the same. Personally, such cases are admitted to my ward for treatment, as among the working classes a constant eye must be kept on the patients, and this cannot be done by busy parents. Jones's method of full flexion causes bending forwards at the fracture. In these cases massage and passive movement are useful so long as pain is not induced, and in the later stages, gentle, continuous, forcible extension and flexion, short of pain, are distinctly helpful. Elastic traction to a posterior splint for extension may also be of use. If there is any elastic "give" in passive movement it means that working at it will overcome a great deal.

*Fracture of the Olecranon.*

The treatment is the same, and even with a large gap the ultimate result is generally complete restoration of function. Bony union may not be complete by any means, but this does not matter if the functional result is perfect. The gap always shortens, and it would be interesting to know what the action of synovial fluid has to do in inhibiting the formation of callus, as is seen in fracture of the narrow part of the neck of the femur.

*Fracture of the Shaft of the Ulna.*

Treatment on the same lines is associated with excellent results.

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### *Fracture of the Head of the Radius.*

This is a fracture not uncommonly missed when only in the nature of a split, but the main feature is pain over the head of the bone on pressure and limited pronation and supination with pain. The treatment is on the same principle, and in time the results are excellent. Possibly removal of the head or of the loose piece with early active movement might be productive of an earlier and better result.

### *Colles's Fracture.*

Here reduction should always be tried, and Sir Robert Jones's method is the best, and then active movement. Splints are occasionally needed, but do not act in retaining the fragments quite as well as would be expected. By the employment of early active movement wonderfully good results may be obtained in what appear to be hopeless fractures.

### *Fracture of both Bones of the Forearm.*

The worst fracture, so far as functional results are concerned, in the upper extremity is where there has been a fracture of the olecranon, or just in front of it, and of the radial head as well. This is produced by direct violence. Here, possibly removal of the radial head and early active movement may produce a better result. I have only seen two cases, and the results, compared with the other types of fractures, were most disappointing.

### *Fractures of the Shaft of both Bones of the Forearm.*

These are usually seen in children, and active movement results in excellent function, which occurs sometimes too early, as the bones are apt to get re-fractured from playing games. If there is a sagging fracture a short gutter splint not interfering with active movement is helpful, but the functional result, which is the important one, should always be kept in mind.

## DISCUSSION.

Mr. R. C. ELMSLIE (President) said that in the first place he would like to thank Mr. Dowden for coming all the way from Edinburgh to demonstrate his methods. He congratulated him on the results he had obtained and upon the very great care he had taken in preserving his records and in showing them to the Section. With one of Mr. Dowden's first statements he agreed heartily, that was, the statement that in most fractures, particularly in the upper limb, the displacement of the fracture was due to the original violence which produced the fracture, and not to the action of the muscles. The significance of this was that such a displacement when once reduced showed little tendency to recur. There were points upon which Mr. Dowden's methods were evidently open to criticism, and he was sure that Mr. Dowden would have to be prepared to hear considerable criticism at that meeting. Many of the cases shown were of fractures in children in whom the growth of bone had corrected in course of time the original faulty alignment, and so done away with the deformity. Mr. Dowden had not convinced him (the President) that his method of treatment was an advance. He (Mr. Elmslie) had always maintained and would continue to maintain that the correction of displacement in a fracture and the attainment of a good alignment was the first principle in the treatment of fractures, and that fixation of the fragments for as long as was necessary to prevent risk of displacement was essential. At the present time they were all using active movements in the treatment of fractures, and their chief difficulty was to persuade the massage profession to keep to active movements instead of using passive or forced movements. In the treatment of supra-condylar fractures of the humerus they had been shown photographs of elbows in which the range of movement was good at the

end of periods amounting to many months or perhaps one or two years. Treating such fractures by reduction of the displacement, rest in a flexed position for three weeks and then active movements, in his (Mr. Elmslie's) experience, led to a normal or almost normal range of use at the end of two to two and a half months. In this type of fracture, therefore, a more classical method of treatment would seem to lead to a quicker restoration of function than did Mr. Dowden's method. Although it was clear from what they had seen that day that treatment without reduction of displacement and without splints might lead to good eventual results in the hands of a careful surgeon such as Mr. Dowden himself, it would certainly seem that to preach the doctrine of carrying out this treatment was very dangerous, if it was to be practised by the entire profession.

Mr. W. R. BRISTOW said that with much of what Mr. Dowden had said there would be common agreement. The value of active movements was beyond question, and would be the usual practice of the Members of the Section; but there were certain quite definite criticisms which he (Mr. Bristow) would make.

First, the results shown on the screen were practically all cases of fracture in children, and it was well known that these did well by almost any method of treatment, short of too prolonged immobilization. Re-formation of bone and absorption of unnecessary bone took place in children in a very regular and fortunate way, but he considered it was wrong and contrary to experience to draw the analogy that the bones in adults would behave in the same manner.

Certain of the pictures showed impaction, and with these, if disimpaction was unnecessary, he agreed that the earlier the movement was started the better. Certain fractures of the upper extremity on the other hand—for example, fracture of the scaphoid—were, in his opinion, better treated by more prolonged immobilization, and, with this particular bone, fixation for six weeks in dorsiflexion usually gave a better functional result and tended to obviate the arthritis which was so frequently seen with old ununited fractures of this carpal bone.

With reference to the involvement of the musculo-spiral nerve with fractures of the humerus, he did not agree that immediate operation was necessary. The complication was a rare one with simple fractures. In some cases the function of the nerve was suspended temporarily, owing to bruising, and perfectly good recovery took place in the course of a few weeks. He considered that when there was evidence of involvement of the nerve the patient should be examined by a competent neurologist, and exploration should be undertaken, not as a routine, but in those cases in which recovery showed no signs of commencing early, or in which it was obvious from the commencement that there was complete division of the nerve.

Lastly, he considered that the use of this method of simply supporting the limb in a sling and leaving the fracture to take care of itself was a very dangerous one to enjoin either to the student or to the practitioner. Physical methods, which included active use of the part had an important place in the treatment of fractures, but the two great principles must not be lost sight of, viz., reduction of deformity with correct alignment and prevention of recurrence of deformity by correct splintage.

Mr. C. MAX PAGE said that Mr. Dowden had demonstrated in a very clear way the manner in which fractures in children gave satisfactory final results despite the failure to procure anatomical re-position of the fragments. He (Mr. Page) felt however that the experience of good results obtained in childhood should not be applied to the treatment of similar injuries in the adult. He suggested that if Mr. Dowden's methods were generally adopted in the latter type of case the outcome would frequently be far from the best procurable.

He disagreed with the author's statement of certain general principles. In his (Mr. Page's) opinion muscle pull must be accepted as a factor productive of deformity in the direction of overlap in all fractures of long bones, and also in the case of small fragments of bone into which strong muscles were inserted, as was obvious in the case of the olecranon and the patella. He also thought that Mr. Dowden's views in regard to the value of movement in the presence of infection in the neighbourhood of the fracture were open to criticism.



### 34 Page: *Deformity of Vertebrae* ; Elmslie: *Dislocated Ankle-joint*

He considered that the general method of treatment of upper extremity fractures advocated by Mr. Dowden had much to commend it, and that with a certain discrimination it was widely used in practice by those having an extensive experience of these injuries.

#### **Case of Congenital Deformity of Dorsal and Cervical Vertebrae.**

By C. MAX PAGE, D.S.O., M.S.

N. M., AGED 5 years.

Brought to out-patient department with the complaint that the head had been held towards the left side since birth. There had never been any pain or disability, but during the past year the deformity had become worse, the head sinking a little more towards the left side.

Labour was normal, head presenting; no delay or difficulty. No previous illnesses; always good health. Two other children, normal and in good health. No family history of any similar conditions, nor of tuberculosis.

*On examination*: General development good. Well nourished. Head held over towards left side, face and chin turned slightly to the right. No facial asymmetry. Neck very short. Muscles on each side well defined; not definitely shortened on one side more than the other. First dorsal vertebra: spine large and prominent and slightly to right of the midline. One cervical spine felt with difficulty; the other cervical spines not palpable, being deep in a marked suboccipital depression. No pain or tenderness. Slight limitation of movement in all directions. Small discrete movable glands on both sides of the neck. Slight postural scoliosis.

*X-ray*: First dorsal vertebra wedge-shaped; the edge of the wedge to the left. Cervical vertebrae not well defined. Difficult to obtain a good plate.

The case appears to be one of congenital deformity of the spinal column in rather an unusual area, viz., the upper dorsal and cervical regions. It has not been possible to obtain an X-ray plate which clearly defines the whole of the cervical spine, but the plate shown suggests that there is a partial absence of cervical vertebrae, and this confirms the clinical appearance.

I do not think that any form of treatment will materially benefit the patient.

#### **Case of Forward Dislocation of the Ankle-joint.**

By R. C. ELMSLIE, O.B.E., M.S. (President).

PATIENT, a woman aged 24, was thrown off the carrier of a motor-bicycle on August 4, 1923. She was unconscious immediately afterwards and therefore did not know the exact way in which the ankle was injured. She was treated at first for a fracture of the fibula and afterwards by massage and manipulations. In the course of this treatment she was seen by Mr. Higgs at the Orthopædic Clinic at Watford and sent to St. Bartholomew's Hospital on December 13, 1923, for an X-ray photograph.

The left foot is displaced forward at the ankle-joint, the malleoli appearing intact with perfect alignment of the shafts of the tibia and fibula; there is a small scar over the internal malleolus. The X-ray photograph shows complete forward dislocation of the foot at the ankle-joint, the only bony injury being a tiny fragment off the anterior surface of the tibia, immediately above the

articular margin. There is no evidence of any injury to either malleolus. The patient is able to walk short distances but with considerable discomfort.

The treatment suggested is an attempt at reduction of the dislocation, incisions being made on the inner and outer aspects and the lateral ligaments being divided, if necessary. In the event of reduction of the dislocation being impossible an astragalectomy with backward displacement of the foot (Whitman's operation) would appear to be the only possible line of treatment.



Forward Dislocation of the Ankle-joint.

### **An Unusual Case of Snapping Hip.**

By ST. J. D. BUXTON, F.R.C.S.

J. F., a TRAM-CONDUCTOR, aged 25, fell against the back of a tram early in December, 1923. He hit his right hip with some force against the metal protection at the back of the footboard. He massaged the part with oils and first presented himself for examination on January 2, 1924. He was then walking with a limp and had pain over the tuber ischii. He had full movements at the hips and no wasting of muscles. There was pain on full extension of the right thigh. On fully flexing the right thigh and then slowly extending it, there was a sharp snap. He can produce the same noise by performing this movement himself. The snap appeared to be in the front of the hip-joint or in it. There never has been a snap on the back of the great trochanter.

Skiagrams do not show any disease or abnormality of the right hip-joint.

### 36 Buxton: *Snapping Hip*; Anderson: *Case of Abnormal Gait*

The case is shown as the snap did not appear to be due to slipping of the gluteus maximus over the great trochanter, nor to the ilio-psoas tendon slipping. It was suggested that if it was a pathological condition, it might be due to the edge of the femoral head slipping on the edge of the cotyloid ligament. It is probable that the snap would not be so constant if it were due to a loose body in the hip-joint or in the ilio-psoas bursa.

#### Case of Abnormal Gait Apparently Functional.

By JAMES C. ANDERSON, M.B., Ch.B. (for H. A. T. FAIRBANK, D.S.O., M.S.)

I. M., FEMALE, aged 9. Patient is a twin and youngest of four. The two eldest children had heart trouble. She was a normal child until  $5\frac{1}{2}$ , but was never so intelligent as her twin brother. At  $5\frac{1}{2}$  years she developed a peculiar gait which has persisted ever since in varying degrees of severity. Her mother believes that the child cultivated the gait because she liked to be carried; she also stated that the condition is worse in winter than in summer. First seen when aged  $6\frac{1}{2}$ . No organic lesion discovered. The gait was regarded as functional. Two years later, i.e., January, 1923, she returned and was admitted to hospital. Again no organic lesion was found. Her gait at that time was described thus: "Walks with peculiar gait, keeping back left side of body, hip and shoulder, when she takes a stride with the left leg. This action varies in degree but her walk is never normal and the peculiarity becomes much more marked on attempting to make her run." In hospital three months; treated in various ways, viz., by training, isolation, starvation, &c. Ultimately discharged slightly improved. In December, 1923, she was admitted in as bad a state as ever. Dr. W. G. Wyllie made a thorough neurological examination and reported "no organic lesion."

The gait is not definitely of one type: the arms are held stiffly by the sides: the right side of the body is constantly held in advance of the left; the right leg is swung forward with very little movement at the hip or knee-joint; the left leg is brought forward either normally or it may be jerked violently from the ground and be planted down again in a violent manner—this last feature is not a constant one. The child's mentality is interesting: she takes a keen delight in her disability; she derives no greater pleasure than when she is displaying it to strangers or to other children in the ward; she is intelligent and cunning. Great improvement has taken place recently under careful drill and training.

## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### A Condition of Right-sided Hemiplegia in each of Similar Twins.

By E. LAMING EVANS, C.B.E., F.R.C.S.

M. D. and J. D., similar twins, aged 3 years 5 months, are under my care at the Royal National Orthopædic Hospital.

They were full-term children, born after five years of married life, of healthy parents. The father was aged 31 and the mother 30. No miscarriages had occurred. There is no family history of deformity or of multiple births.

The elder child was born after difficult extraction, by forceps, which lacerated both sides of the face and which produced a temporary left facial paralysis. The younger child was delivered without difficulty and without forceps.

Spastic paresis of the flexor type was first noticed in infancy, when the children began to use arms and legs. Both children show spasticity of the flexor muscles of the right upper and lower extremity, similar in degree except that the spasm of the triceps suris is greater in the younger twin, J.

Wassermann reactions in the case of J. were negative. (An attempt to obtain blood from M. failed.)

Tendon reflexes in right upper and lower extremities in each were exaggerated, and the right plantar reflexes were extensor and the left flexor in each case.

The attitude and gait are typical of infantile hemiplegia.

A careful research into the literature has failed to disclose any published case of congenital hemiplegia in each of similar twins. On the other hand, spastic paraplegia has been often observed in twins.

The delivery of the second child, J., without forceps and without difficulty, points to a non-traumatic origin of the cerebral lesion.

I regard them as cases of primary neuronc degeneration of the familial type.

Dr. F. PARKES WEBER suggested that—from the terminological point of view—it was better to speak of "primary neuronc hypoplasia" than of "primary neuronc degeneration" as the cause of the congenital hemiplegia in these twins. There was clearly no degeneration in progress; possibly there was some amount of development going on in the neuronc tracts, even though only "compensatory development."

**Bony Ankylosis of Elbow at 125°, and with Forearm Pronated.**

By PAUL BERNARD ROTH, F.R.C.S.

M. T., AGED 12, first seen on February 15, 1924, because of *stiffness of left elbow*, rendering left arm almost useless. Her mother stated that between five and six years ago she had an accident to her left elbow and was treated at a hospital, where an X-ray was taken, and where the mother was told that the elbow was "quite all right." It has been stiff ever since.

*On examination* left elbow ankylosed at 125°, and forearm rigidly pronated at 60°. Wrist, hand and fingers normal.

*X-ray* of elbow, forearm and wrist show that there is sound *bony ankylosis* of the upper ends of the radius and ulna to each other and to the lower end of the humerus; there is no line indicating the level of the joint. The lower radio-ulnar articulation is normal, except for pronated position.

*Remarks.*—This case presents a typical example of how *not* to treat a damaged elbow: it should have been treated with the elbow flexed and the forearm in the "mid-position" between pronation and supination. Evidently there must have been a suppurative arthritis set up as a result of the accident.

I propose to excise the elbow, flex the forearm to 60°, and place the forearm in the "mid-position." I should be glad of opinions as to the best method of getting movable articulations.

**Treatment of Fractured Clavicle by Sayre's Method Resulting in Paresis.**

By PAUL BERNARD ROTH, F.R.C.S.

F. M., AGED 15, broke her right collar bone on December 25, 1923, was brought at once to hospital, and the injury treated by Sayre's method, fixing the hand flat against the chest with adhesive strapping. I did not see the patient until a month later, when she was still in the strapping, and a radiogram showed an oblique and comminuted fracture of the middle third of the clavicle with gross overlapping and displacement. On removal of the strapping, the forearm, wrist and hand seemed withered and almost lifeless, and patient could only perform very feeble movements of the fingers.

Operation on the clavicle was done five days later, on January 29, 1924, but owing to the comminuted nature of the fracture it was found impossible to keep the main fragments in an improved position.

Treatment of the forearm, wrist and hand has consisted in applying a dorsi-flexed wrist splint, and daily massage and active and passive movement. Power is slowly returning, but there is still considerable shortening of the flexor tendons, and the grasp is so feeble as to be useless.

*Remarks.*—I wish to discuss two points which have arisen in this case, the paresis, and the difficulty of improving the position of the fragments by operation.

I regard the paresis as being ischæmic in origin, and strongly deprecate the use of Sayre's method in the treatment of these cases. I think the only satisfactory method of treating the fragment in a bad fracture of the clavicle

is the introduction of a Lane's plate with four screws, the plate being used, however, with the clear understanding that it would have to be removed subsequently. I have never seen a case of fracture of the clavicle treated with a plate in which the plate has not had to be removed later, owing to its irritating effect upon the cutaneous scar. In the present case I have failed to apply a plate because there were no suitable fragments to which to fix it, and the wire used instead tore its way out.

### Specimen of Congenital Absence of the Sacrum.

By S. L. HIGGS, F.R.C.S.

THE specimen is from a case shown by the President (Mr. Elmslie), and reported in the *Proceedings* of December, 1921.<sup>1</sup>

The subsequent history of the case is the following: The child was admitted to hospital and four operations performed for correction of the feet and reduction of the left hip. The operations were well borne and the general nutrition of the child was good, but he had rectal incontinence. The electrical reactions of the lower limbs were tested and all muscles responded normally. The child was about to be discharged when he suddenly died, presumably from heart failure. A post-mortem examination was made and the specimen of the lower part of the spine, pelvis and femora removed. The heart showed a patent foramen ovale.

The specimen is chiefly remarkable in the following respects: (1) Complete absence of the sacrum and close approximation of the two iliac bones. (2) Four lumbar vertebræ only, the fourth having a wedged body and bifid spine. (3) As a consequence the breadth of the pelvic cavity is greatly diminished. (4) The posterior sacro-iliac, great and small sacro-sciatic ligaments appear to be fused to form a single strong ligament. (5) The right hip shows a strong band extending from the great trochanter to the posterior inferior spine, not corresponding with any normal structure and limiting adduction of the hip-joint. (6) The left hip is firmly in position and cannot be easily re-dislocated by manipulation. X-rays show absence of ossification in the head of the femur on that side.

### Case of Multiple Congenital Deformities (shown a year previously, February, 1923).

By D. McCRAE AITKEN, F.R.C.S.

WHEN the patient, a boy, was shown before the Section in February, 1923, his elbows were extended, his forearms pronated and he was crawling on the dorsum of his hands. The knees and hips were flexed and there were club feet.

Treatment was begun in May, 1923. The knees were gradually extended after elongation of the hamstrings; the feet were wrenched and put in plaster. The left hand was gradually corrected in plaster.

Function of the hand is returning. Pronation and supination have been completely recovered and movements with the elbow are improving, and the boy is now walking in calliper splints.

<sup>1</sup> *Proceedings*, 1921-22, xv (Sect. Surg., Sub-Sect. Orth.), p. 8.

### Case of Polio-myelitis.

By D. McCRAE AITKEN, F.R.C.S.

PATIENT, a boy, aged 12 years, has suffered from polio-myelitis since 1919. When seen in 1921 he was unable to hold up his head, the left arm was a total flail and there was no power worth mentioning in the right hand.

He was placed in a recumbent position on a frame. Massage and re-education of the weak muscles were begun, and later a collar was applied to support the head. The right hand was very weak with no power of opposing the thumb, but a small palmaris longus muscle was acting. In October, 1923, the palmaris longus tendon was transplanted into the base of the first metacarpal bone. There has been rapid improvement in the function of the hand since opposition of the thumb was restored.

### Case of Pseudo-coxalgia following Traumatic Dislocation in a Boy.

By H. A. T. FAIRBANK, D.S.O., M.S.

F. T., BOY, aged 10½ years.

*History.*—Seven months ago patient was run over by a motor bicycle. Two days later he was X-rayed and traumatic dislocation of right hip-joint diagnosed. Hip reduced next day at Hanwell Hospital. Was in bed seven weeks; on getting up he walked with slight limp which is said to have gradually increased. Pain in hip on exertion, but not when at rest. Said to have lost weight since the accident.

*Examination.*—Walks with a limp on right leg with apparent stiffness of right hip. There is some general wasting of the leg, particularly of the thigh. There is some slight fixed flexion. Movements of the hip show that extension is impossible and abduction practically obliterated. Flexion is free to a point beyond a right angle but is not quite full. Internal and external rotation definitely limited. Some prominence of great trochanter, which seems a trifle high. Jarring the trochanter causes pain. No obvious thickening of the trochanter, but some fullness over front of joint. Shortening = ½ inch.

*X-Rays* shows slight asymmetry of the pelvis. Distinct irregularity of the outline of the acetabulum with mottling of the bone. Femoral head is distinctly flattened and greatly increased in density. Neck thickened with a little mottling of the bone close to the epiphyseal line. Joint space slightly increased above.

For the last week the boy has been in hospital with a weight extension applied to the limb, abduction being gradually increased. The case is shown on account of the rarity of traumatic dislocation in children and the interesting condition now present. A case with a similar sequence of events was reported by the President (Mr. Elmslie) at a meeting of the British Orthopædic Association in 1917.

X-ray taken two days after the accident, and kindly sent me by Dr. Spain, shows dislocation of the hip without any definite injury to the bone, and certainly no crushing of the femoral head. There is a slight suggestion of damage to the neck, close to the outer limit of the epiphyseal line.

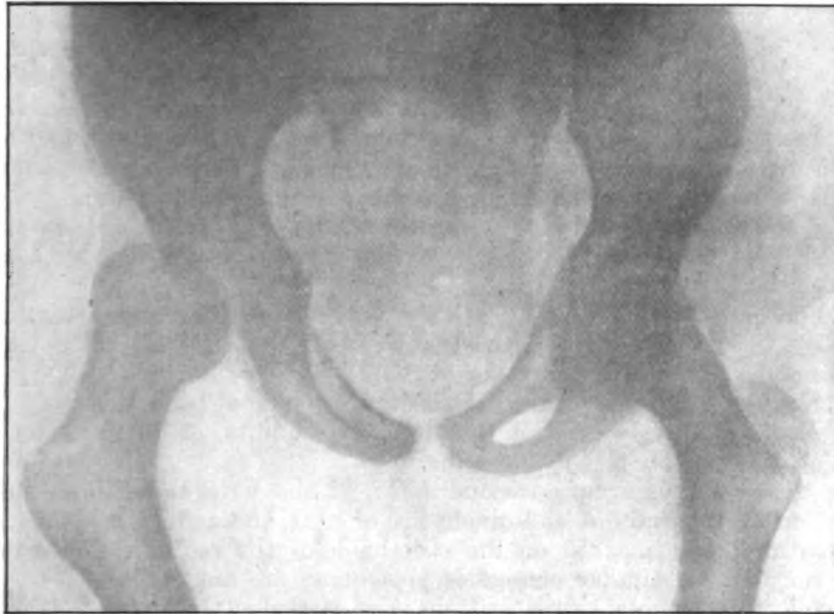


FIG. 1.—Radiogram taken June, 1923, showing a posterior traumatic dislocation of the right hip-joint.



FIG. 2.—Radiogram taken February, 1924, showing pseudo-coxalgia of right hip-joint. The flattening and density of the femoral epiphysis, with broadening of neck are well seen.



### Case of Renal Dwarfism.

By G. PERKINS, M.Ch.

PATIENT, a girl, aged 12½, was first seen in July, 1923: she had a right genu varum, and a left genu valgum. No mention at this time was made by either parents or surgeon of any deformity of the feet or hands. She was admitted to the ward, and a femoral osteotomy was performed on the left side to correct the genu valgum. The operation was followed by a normal convalescence.

In February of this year (1924) she was again seen, and attention was drawn by the parents to the wrist and ankle deformities. The following appearances were then noted:—

(a) Marked valgus deformity at the ankle-joints of both sides.

(b) Marked ulnar deviation at the wrist-joints of both sides, with limitation of rotation of the forearms.

The X-ray shows a curious condition: At the wrist there is an irregular ossification of the ends of the diaphyses of ulna and radius, most marked in the ulna, and least marked on the radial side of the radius. The epiphyses appear normal. A similar change is present at the ankle-joints.

The following chemical investigations were carried out by Dr. de Wesselow:—

|                                                        |     |                        |
|--------------------------------------------------------|-----|------------------------|
| Blood urea                                             | =   | 232 mgr. per 100 c.c.  |
| Plasma phosphates (as phosphorus)                      | =   | 6.8 mgr. per 100 c.c.  |
| Urea concentration:—                                   |     |                        |
| First hour                                             | ... | 0.9 per cent. 75 c.c.  |
| Second "                                               | ... | 0.95 per cent. 60 c.c. |
| Fourth "                                               | ... | 1.1 per cent. 60 c.c.  |
| Urine: Protein small amount; occasional granular cast. |     |                        |

Dr. de Wesselow reports: "This is a case of azotæmic nephritis. In cases of renal dwarfism the urea retention is usually considerable, but when it reaches such a figure as that found in this case, the end is usually a matter of a year or so."

The Wassermann reaction has not been carried out in the case of this patient. The mother is suffering from interstitial keratitis, and her Wassermann reaction is positive.

The points of special interest are: (1) The age of the patient—rather older than usual; (2) the curious symmetrical changes at the ends of the diaphyses; (3) the successful performance of a femoral osteotomy.

### A Curious Case of Congenital Spastic Hemiplegia.

By G. PERKINS, M.Ch.

WHEN this patient, a boy, aged 5, attempts to walk, there is a tic-like spasm of the muscles of the left leg, which causes him to stumble and often to fall. The spasm does not appear to be under control. The mother says the condition has been present since the age of eighteen months.

On examination.—Hydrocephalus is noticed. The right arm and leg are normal; the left arm is rather spastic, but full active movements are possible

at all joints. In the left leg active dorsiflexion at the ankle is limited; all other movements are full. The plantar response is extensor. The deep reflexes are increased.

Are the curious mannerisms of organic origin, or do they represent a functional element superimposed on the hemiplegic condition?

What treatment would Members advise? He has already had a course of educational exercises without deriving any benefit from them.

## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### DISCUSSION ON THE TREATMENT AND FUNCTIONAL RESULTS OF TUBERCULOUS DISEASE OF THE HIP-JOINT.

Mr. GORDON PUGH

said he would like to emphasize the importance of constitutional treatment, viz., an open-air life, with heliotherapy in the warmer months and exposure to artificial light, now upon its trial, during the months when the sunlight was not available. In the patient's contest with the bacillus, every influence tending to improve his vitality must be applied, for the greater the reaction of his tissues the greater the success that would attend the surgeon's efforts. It would be impossible to discuss hip disease without mentioning the name of Hugh Owen Thomas, and it was interesting to note that next year half a century would have elapsed since Thomas first described his hip and knee splints. Thomas had said that in hip disease early diagnosis seldom benefited the patient, because of the erroneous policy which directed the treatment and the inefficiency of the appliances used. To-day, one found that although the defects in the mechanical treatment to which Thomas referred had been to a great extent corrected, still in many instances early diagnosis did not give the patient the benefit of the early constitutional treatment which legislation had made available for all such cases in London. It was a remarkable fact that the majority of patients admitted to the country hospitals of the Metropolitan Asylums Board had been kept under treatment in town institutions or at their homes for months or sometimes years after the diagnosis had been arrived at. There had been some change in this respect in the last year or two, but even of recent admissions only one-fifth had been received within three months of the onset of the disease. The urgency of constitutional treatment in the very earliest stage had not received the attention it deserved.

According to the accepted pathology, tuberculous disease of the hip-joint usually began in the red marrow, where the circulation was most active and the newly-formed bone was least resistant. If the local resistance could be sufficiently increased the disease might be confined to the interior of the bone. Otherwise it found its way into the joint, and the granulation tissue spread under and over the cartilage and partially destroyed it. While the disease was still limited to the interior of the bone, the joint showed evidence of sympathetic inflammation, the synovial membrane became congested and the synovia was increased in quantity, and limp and slight limitation of movement resulted. This was the stage at which intensive constitutional treatment was urgently

[April 1, 1924.]

called for. The speaker showed photographs and skiagrams of cases in which early constitutional and local treatment had been followed by absorption of deposit and complete restoration of articular function, and contrasted them with others in which a few weeks' delay in providing the former had been associated with extension of disease to the joint. He suggested that any child in whom the symptoms of pain or limitation of movement persisted in spite of rest in bed for four weeks merited the treatment advocated for tuberculous disease. Deformities resulting from ineffective treatment were then dealt with. Genu recurvatum could be avoided if the knees were kept slightly bent throughout recumbency; genu valgum was sometimes secondary to adduction of the thigh, but not infrequently was the result of the weight extension not being made in the line of the femur. Severe scoliosis was likely to occur in female adolescents if the hip was fixed in adduction with insufficient flexion. The best position for ankylosis was moderate flexion; if, with the patient recumbent, the lumbar hollow was obliterated when the limbs were raised  $35^{\circ}$ , comfortable sitting and standing were both possible. If the angle was less, there was a tendency to scoliosis; if greater, the patient had to compensate either by excessive lordosis or by bending the knee of the affected limb, which increased the shortening. Adduction persisting to the stage of convalescence required correction by operation. If the joint was not fixed, treatment by tenotomy of the adductors, followed by fixation for several months in plaster, was often sufficient. If there was fibrous ankylosis with only moderate deformity Brackett's osteotomy was useful, but if the adduction was marked the deformity was apt to recur owing to instability of the ankylosis. In marked adduction, therefore, Lorenz's bifurcation osteotomy was advisable and so far very satisfactory results had been obtained. Whichever operation was done the pelvis should be held up on the normal side by a perineal band attached to studs incorporated in the plaster. Abscesses were treated by immobilization and aspiration, but the injection of resolving fluids had not been found of practical value; if aspiration failed to empty the abscess and it threatened to come to the surface the skin should be excised wide of the thinned area, the thick tuberculous material in the cavity expressed, and the wound sutured; the bone should never be scraped. Septic tracts should be slit up and allowed to granulate; sequestra if present should be removed; later, the small sinus leading to the joint should be excised, treated with B.I.P.P., and sewn up. Immobilization in the active stage was now universal. Ambulatory treatment was bad, not only on account of the difficulty of fixing the joint and preventing deformity, but also because it encouraged growth in the normal limb in excess of that of the affected one, a considerable difference in the length of the tibiæ being not uncommon after ambulatory, but quite rare after recumbent, treatment. It was most important that there should be no pressure on the partially decalcified and softened bones, hence imperfect fixation causing reflex spasm of the muscles was most harmful. Even absolute fixation, however, did not remove a tonic contraction of the muscles around the joint, and therefore weight extension was needed in addition. It would be an advantage if this could be applied transversely, to overcome the action of the adductors, abductors, and rotators, as well as in the length of the limb, but there were mechanical difficulties in the way. Some, in despair of obtaining a satisfactory movable joint used a plaster spica and allowed the patient to walk on the affected limb in the hope of securing bony ankylosis. Bony ankylosis, however, was difficult to secure in a child, and even when it did occur the condition often proved unstable. With early and adequate treat-

### Pugh—McMurray: *Tuberculous Disease of the Hip-joint*

ment useful movement of 20° or more might be obtained in many cases and this should be the aim of treatment. The deformity should be corrected by steady traction, under an anæsthetic if necessary, and weight extension applied to the thigh by moleskin strapping. If there was much thickening with considerable deformity it was better to correct gradually by weight extension. The joint should at the same time be immobilized by a plaster spica, the front part of which should be removed to allow of the detection of abscess formation. A perineal band for the opposite groin should be attached to studs fixed in the plaster; the latter, while it did not interfere with traction, prevented jarring of the joint and consequent muscular spasm when the bed-pan was given. The use of a frame and carriage, as at Carshalton, had the advantages that both knees could be kept slightly flexed, rotation could be prevented by the use of a sandal to which a wire support was attached, and exercise of the sound limb became possible without disturbing the affected joint. When the disease was quiescent, as shown by X-ray examination and the fact that even slight tonic contraction had been completely absent for months, the weight extension which had been gradually reduced should be omitted, and the movements which the patient usually tried to make at this stage encouraged under careful supervision; no attempt at passive movement, however, should be permitted. The longer this stage was prolonged the greater was the mobility likely to be, but the position of the limb should be carefully observed and the progress of healing checked by X-ray examination. When the patient got up he should wear a short celluloid spica or similar splint and be allowed to walk on patten and crutches. When he was in bed the spica need not be worn and he should be allowed to move the joint if any movement was possible. Especially when the joint was fixed, was it important that he should be seen every week for a considerable period after the spica had been omitted, lest flexion deformity should occur.

#### Mr. T. P. McMURRAY (Liverpool)

said that after Mr. Pugh's remarks it might be forgotten that the disease was not confined to the young, but could occur at any age. This fact must not be overlooked. The diagnosis was usually made easily from the presence of rigidity in every direction, and should be followed by immediate and complete fixation. He employed a frame with fixed extensions in a recumbent position for a period, which varied according to the nature of the case. In the absence of a sinus, fixation lasted from fifteen to twenty-four months. The period of recumbency came to an end when pain had been absent for six months. He considered a frame to be better than plaster as a means of fixation in the early stages since it permitted the adoption of heliotherapy. Too great a degree of abduction should not be aimed at on account of the scoliosis and pain which might be produced. As regarded the operative treatment, he considered that the mortality was so high that operation was never justifiable in uncomplicated cases of this disease which, as Ford remarked in 1792, was rarely fatal, unless a sinus and secondary infection occurred. With regard to abscesses, he found that the progress of many was checked by mere fixation of the joint, and only rarely was he forced to aspirate. In the latter event he never made injections of any of the many fluids which were recommended, and he considered these exercised no beneficial effect. In the presence of a sinus leading to a detached and carious femoral head he operated and removed the head, but did not interfere with the tuberculous granulation. Other factors in the treatment were

open air, and sunlight or artificial light in winter. The end-results that might be obtained were either bony ankylosis from complete destruction of the joint surfaces, or fibrous ankylosis either fixed or permitting a very limited range of movement which was frequently painful. Of these he considered bony ankylosis to be the better result. In his opinion cases which recovered with a complete restoration and perfect functional mobility of the joint had been erroneously diagnosed as tuberculous.

**Mr. R. C. ELMSLIE (President).**

The Section is very greatly indebted to Mr. Pugh and Mr. McMurray for their excellent opening addresses. Mr. Pugh has discussed diagnosis, and I should like to enter a plea for early accurate diagnosis, particularly as to the nature and site of the lesion, by careful radiographic methods. I have recently treated a cyst in the neck of the femur, which was diagnosed beforehand as tubercle, and I would differ from Mr. Pugh in that I consider that these cysts require operative measures—opening and crushing of the wall—as in children they tend steadily to enlarge until a fracture takes place. In another recent case a large sequestrum in the neck of the femur was shown in an X-ray of a case in which there was an abscess in the thigh. If such a sequestrum is left to itself, presumably prolonged suppuration will take place until the sequestrum has gradually disintegrated. Removal of the sequestrum with complete removal of the sac of the abscess and closure of the wound has so far been quite successful in this case.

I am glad to hear from Mr. Pugh that he considers there is an operative side of the treatment for tuberculous disease of the hip. Too often recently it has been assumed that constitutional treatment, sun treatment and mechanical methods are all that can be necessary. My experience of the results of the old-fashioned variety of excision differs from that of Mr. Pugh. I think that very often cases in which there has been excision have undergone an early cure, probably because the excision results in a dorsal dislocation of the stump of the neck of the femur, thus removing the diseased surfaces from contact with each other and eliminating the elements of friction and pressure to which Mr. Pugh has alluded. I would not, however, advocate excision, as in my experience the functional results are very bad. In examining boys in the London Schools for Physically Defective Children I have found that those who have bony ankylosis can often walk ten or fifteen miles a day; with a mobile joint, i.e., after excision, they cannot walk more than two miles.

The most difficult problem in tuberculous disease of the hip is presented by cases which end with a fibrous ankylosis and a little movement which is of no functional value. In such cases, in adolescence or in adult life, I think there is a place for arthrodesing operations with a view to securing a bony ankylosis in a good position. Such operations must be done with the greatest possible attention to asepsis and with removal at the time of the operation of as much as possible of the tuberculous disease. Carefully carried out I think they are safe, but I would not advocate them in the early stages of the disease or in young children.

## Sir HENRY GAUVAIN (Alton)

said that Mr. Pugh's address was an admirable and lucid statement. With much that Mr. McMurray had said he must express disagreement. It was not possible in the time available fully to discuss the addresses delivered; he would confine himself to one or two points which had specially aroused his interest. He referred to difficulties that arose in the diagnosis of early tuberculous disease in the hip-joint. There were a number of conditions which might simulate tuberculous disease, as for example pseudocoxalgia. It was of the utmost importance to endeavour to fix the diagnosis, but where after all investigation had failed to confirm the diagnosis absolutely, he agreed with Mr. Pugh that the patient should be regarded as tuberculous and treated accordingly. In this way only could possible subsequent disappointment be guarded against, but in some cases without doubt the patient was not tuberculous. Mr. Pugh had referred to "genu recurvatum" developing after extension improperly applied in hip-disease. With the measures suggested for its prevention he agreed and emphasized the importance of traction on the femur with slight flexion at the knee-joint. He pointed out that in many cases, especially in girls, not only did genu recurvatum occur but also a condition which he had noted though never seen recorded—one which, unfortunately, was not uncommon, namely, atrophy of the tubercle of the tibia, which exaggerated the deformity. Not only did the tubercle sometimes disappear but at the same time there was absorption of the underlying bone as well. This condition was well recognized at Alton, and he had given to it the name of "dimpled tibia." He had taken numerous skiagrams of this condition and was of opinion that it was associated with disuse of the quadriceps extensor muscle and could be prevented if the quadriceps was allowed to function during the period extension was applied. Such deformities as genu valgum arising during extension could be prevented by observing the rule that extension should always be in the long axis of the femur, and they would be avoided so long as there was no undue haste exhibited in producing abduction in adducted hips. He referred to the "condylar clamp extension" apparatus employed at Alton which ensured traction on the femur alone, prevented any harm being done to the knee-joint, prevented foot-drop or heel-sores, reduced the risk of eversion or inversion, permitted the application of heliotherapy to the whole of the limb, and by reducing friction to the minimum rendered the smallest possible weight efficient during extension. When deformity had been corrected by extension or other means, simultaneous immobilization of the hip-joint with extension could be continued, if desired, by his combined plaster extension and immobilization apparatus, which he briefly described.

While not discussing the relative advantages of extension and immobilization he was of opinion that return of movement was much commoner if extension was employed than if immobilization was alone used. He disagreed with the view expressed that full function at the hip-joint was unobtainable after genuine tuberculous disease of the hip-joint and said he could prove this statement to be incorrect by referring to cases which had been complicated by abscess formation and where tubercle bacilli had been recovered from aspirated pus. He fully disagreed with the view expressed that abscesses should not be interfered with unless likely to result in sinus formation. He advocated aspiration as soon as the skill of the surgeon permitted this procedure to be undertaken. It undoubtedly saved time and prevented abscesses burrowing for long distances between muscles and through fascia. With regard to the

use of modifying fluids he was not surprised at the results which followed Mr. McMurray's experiment with controls. The value of modifying fluids was restricted to particular circumstances for which there were special indications and their indiscriminate employment, or their use in ignorance of the special indications which should govern their employment, would result in inevitable disappointment and an entirely false conception of their value. He also disagreed with the statement that fixation of a hip abolished muscular spasm and was equally emphatic that to suggest a particular period for the cure of active tubercle in the hip-joint by any method of treatment was entirely unsound and unscientific. As long as there was activity of disease in the hip-joint so long could reflex muscular spasm be elicited, and he referred to the last recognizable sign of activity by reflex spasm on rotation of the hip described by him some years ago as a certain test of great value when doubt existed on this point.<sup>1</sup> X-ray signs always lagged behind clinical signs and frequently it was impossible to tell whether disease remained active by mere radiographic examination.

Mr. H. A. T. FAIRBANK

said that it was apt to be forgotten that at first in many cases the disease was really outside the joint, namely in the femur or in sub-acetabular bone; the early diagnosis was therefore particularly important. In these cases and in cases of synovial disease it was quite possible to get a movable joint. As regards the amount of flexion which was advisable in an ankylosed hip he agreed that 35° was an admirable compromise, but he thought it should be made clear that in treating those cases which might result in ankylosis much less flexion should be maintained, as it was difficult to prevent flexion increasing to too great an extent later on when ankylosis was imperfect. In his experience true bony ankylosis was a very rare result in this disease. Adduction often recurred after correction. In a few obstinate cases he had performed exsection of the obturator nerve by the extra-peritoneal route in order to reduce this tendency to adduction. He had a series of radiograms which showed that adduction might take place at the epiphyseal line when bony ankylosis had occurred between the femoral head and the acetabulum.

Mr. ACTON DAVIS

said he had seen a tuberculous hip recover with complete mobility. As he then regarded the case as not tuberculous the usual scheme of treatment was not carried out, and the child reappeared some time later with recurrence of the disease. He emphasized the fact that the functional results of treatment might be very good in cases exhibiting flexion and adduction, which, anatomically, could only be described as bad.

Mr. PAUL BERNARD ROTH

said that in his experience the hip-joint was the joint of all others which reacted best to conservative treatment. As a test of cure, he preferred a return of the normal density of the head of the bone, as shown by the X-rays, to the Gauvain spasm test. He agreed with speakers other than Mr. McMurray as regards the treatment of abscesses and the desirability of a small angle of flexion. In his opinion the weight of the limb produced the necessary traction when Thomas's hip splint was used in the upright position.

<sup>1</sup> "Tuberculous Disease of the Hip-joint: a sign of Pathological Activity," *Lancet*, 1918, ii, p. 666.



## Mr. McCRAE AITKEN

said he thought that with a movable joint there was often recovery from synovial tubercle, but such cases were apt to suffer bone infection later. He had now come to the conclusion that some form of arthrodesis was the best kind of treatment for old painful fibrous ankylosis or unstable joints with destruction of the head.

## Mr. GORDON PUGH and Mr. McMURRAY

briefly replied.

## Section of Orthopædics.

President—Mr. R. C. ELMSLIE, O.B.E., M.S.

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### Case of Polyarthrititis with Ankylosis of all Joints except Elbows and Shoulders.

By H. TYRRELL GRAY, M.C.

PATIENT, a young girl, aged 17; all the joints except elbows and shoulders were ankylosed. The ankylosis is bony in all the joints except the left knee and the right ankle.

I should be glad of advice as to the method of treatment, and for suggestions as to the pathology of the case. In the course of a routine examination *Bacillus coli* bacilluria was found, and there is a history of this having been present for a considerable time. A large stone was found in the right kidney and has been removed. The urine is now clear.

Mr. W. ROWLEY BRISTOW suggested that the girl be given one sound leg in good position as a stable prop on which she would be able to rely. He said he would take the left leg and perform trans-trochanteric osteotomy in order to correct the hip deformity; he would divide the muscles and posterior part of the capsule to straighten the knee, and would do a wedge excision to bring the foot up to a right angle. He would not interfere with the wrists, at any rate until everything else had been done. The girl could use her hands, and, personally, he would not run the risk of interfering with this, her only useful movement. Elbow crutches could be arranged to meet the case and fit the deformity. With regard to the right leg, which should be corrected when she was able to stand on the left, he thought an attempt should be made to get a movable hip by means of an arthroplasty. The foot could be corrected by removal of a bone wedge, as a fixed ankle in good position was a comparatively slight disability. With regard to the knee, he suggested that, again, arthroplasty was the operation of choice. If the operation failed, it would result in fixation, and so nothing would be lost by making the attempt to give her a movable knee. The state of the muscles was very fair, and although arthroplasty of the knee was very much on its trial, he thought that in this case it was justifiable to attempt the operation. He thought it was safe to begin treatment straight away; and if the result in the right leg was sufficiently good, and the hip arthroplasty yielded a stable and a movable hip, the question of restoring movement in the opposite hip could then be considered.

### **Paget's Disease — Osteitis Deformans.**

By E. MUIRHEAD LITTLE, F.R.C.S.

PATIENT, a female, aged 72, was first seen in 1909; she was then suffering from deformity after a Pott's fracture of the right fibula.

She was seen again from time to time, until in February of 1924 she came to me with a complaint of deformity and pain in the right leg. She stated that except for the old deformity there had been nothing wrong with the leg until about one and a half years ago—when she was aged 70. She then noticed an enlarged vein in the leg, which became painful. The present deformity then gradually appeared.

Besides the deformity in the right leg-bones, the right femur is thickened, but is not otherwise deformed. There is no evident change in the skull or other bones. Radiographs, for which I am indebted to Dr. W. H. Coldwell, show enlargement of the Haversian canals in the tibiae.

The chief point of interest in this case is the late age of onset, viz., 70. The latest date of onset recorded, as far as my knowledge goes, is 72 years.

No treatment is proposed excepting instrumental support.

### **Bilateral Cervical Ribs in a Child.**

By G. PERKINS, M.Ch.

THIS child, aged 4 years 11 months, was brought to St. Thomas's Hospital one month ago. The mother had noticed that for the past three months the boy had been using his hands in a peculiar way. In reply to questions, the mother stated that the hands swelled occasionally, but were never blue; and that she thought the condition was getting worse.

On examination there is seen to be bilateral paresis of the hand intrinsics, also weakness of the long flexors of the fingers. No sensory loss can be made out, but testing is not easy. The radial pulse is palpable on both sides. No swelling or abnormality of colour has been seen. Lumps can be felt in the neck on both sides and the radiogram shows bilateral cervical ribs.

In the discussion considerable doubt was expressed as to whether this was really a case of cervical ribs.

### **Bilateral Subluxation of the Temporo-maxillary Joints.**

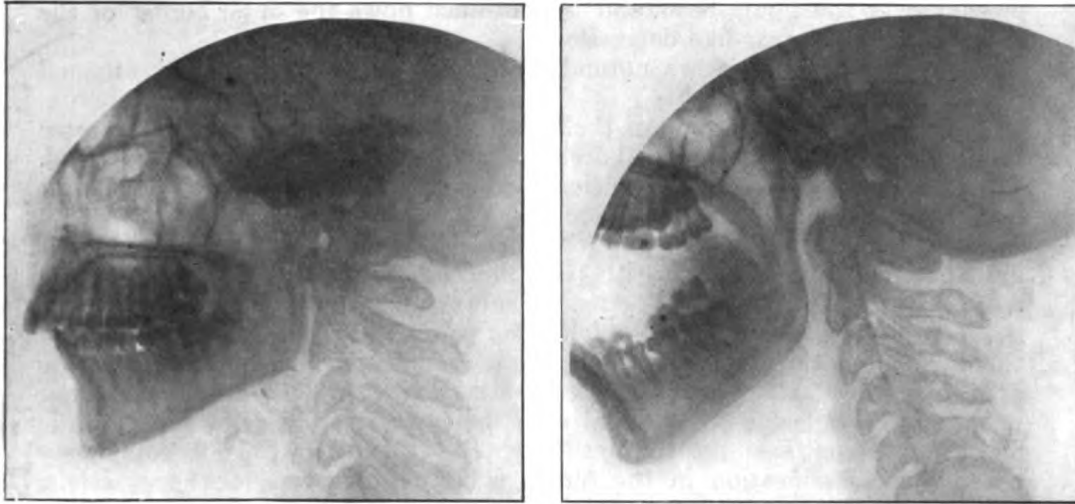
By PAUL BERNARD ROTH, F.R.C.S.

IVY C., aged 17, shorthand typist.

Last July her jaws began to hurt, and the pain has become worse and worse, so that now she can hardly eat and is in constant pain.

On examination: On opening the mouth each condyle dislocates forwards and on shutting the mouth slips back into place.

The radiogram of the left joint taken with the mouth open shows that the condyle has slipped forward over the eminentia articularis. The displacement



Radiograms of left temporo-maxillary joint.

FIG. 1.—Mouth closed.

FIG. 2.—Showing subluxation.

occurs on yawning, eating, or biting: on two or three occasions patient has not been able to reduce the displacement for several minutes.

Mr. ROTH said that many methods of treatment had been practised on cases similar to this, ranging from excision of the condyles, plication of the external lateral ligaments, division of the external pterygoid muscles, to prolonged rest, or "masterly inactivity!" but all had got well in the end.

### Case of Left Unilateral complete Absence of the Tibia.

By E. LAMING EVANS, C.B.E., F.R.C.S.

PATIENT, male, aged 1 year 3 months; well-developed, healthy child.

*Family History.*—Father, aged 27; mother, 21. Both without deformity. They have a younger child, aged 3 weeks, without deformity. No known deformities have occurred in father's or mother's families.

*Present Condition.*—*Head, neck and spine* normal. *Upper extremities*, normal. *Abdomen*: left testicle in a high scrotal position. *Thighs*: the left thigh is  $\frac{1}{4}$  to  $\frac{1}{2}$  in. shorter than the right. Circumference of thigh: upper part, left 10 in., right 10½ in.; lower part, left 8 in., right 9 in. *Patellæ*: right well developed; left impalpable.

The leg is shortened and flexed in the region of the knee: flexion is complete until checked by impact of heel against posterior surface of thigh: extension is short by 45°.

The upper end of the fibula reaches to the level of the upper margin of the external condyle, and has a loose connexion with its outer surface, constituting a pseud-arthritis. The fibula is palpable through its whole length: is curved with convexity outwards, and feels thicker than normal; it is 4½ in. long, measured along its convexity; the right fibula measures 5½ in.

## 54 Laming Evans: *Left Unilateral complete Absence of the Tibia*

The curvature of the fibula in no way resembles the normal curves.

The external malleolus is unduly prominent, and the foot lies in complete supination, along the inner border of the leg. A deeply recessed dimple is present over the fibula head, and is continued down the outer border of the fibula as a linear scar-like depression.

The four outer toes are normal, but the great toe is bifid in its ungual phalanx.

The general conformation of the foot is normal, except for a slight increase in the height of the longitudinal arch and shortening of the palmar structures. There is complete absence of the elongated convex outer border usually present in congenital equino-varus.

All the muscles of the leg and foot appear to act, and the functional results are abnormal, corresponding with the abnormal leverages. As far as one can tell, sensation is normal. The circulation is good.

*X-ray examination* shows the following points: (1) The pubic segment of the os innominatum is still without sign of ossification: normally this centre appears during the fourth month of intra-uterine life. The ilio-ischiatic segment is normally ossified. (2) The centre for the capital epiphysis of the femur is absent on the left side, present on the right. (3) The left tibia is completely absent. (4) Ossification in the tarsus is normal: centres for the os calcis, astragalus, cuboid and external cuneiform are present. (5) The accessory toe lies on the medial side and is fixed with the ungual phalanx of the great toe: this ungual phalanx has one centre of ossification whereas the accessory toe has two centres, thus resembling the four outer toes.

Cases of absence of the tibia are nearly equally divided into partial and complete. In 1920, Nuzzi<sup>1</sup> collected eighty-one recorded cases. Of the partial, there is only one recorded instance of the defect involving the upper end: they, therefore, lend themselves more readily to conservative treatment by bone-grafts.

Cases of complete absence have been treated by attempts at arthrodesis of the fibula with the femur: but, in almost all cases, the results have been poor, union failing to take place, and a prosthetic apparatus being required.

In the present case, even if the fibula could be united to the femur and the foot rendered plantigrade, the present shortening would be increased by the amount of the operative overlaps, and the intrinsic deficiency in growth impulse, with the result that the patient would have to carry a gradually increasing cork on an extended and fragile limb.

I think amputation through the knee-joint is preferable, with an end-bearing stump in a bucket on an extensible peg leg.

<sup>1</sup> "Chirurgia degli Organi Movimento," 1920, v, p. 164.

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## Section of Otology.

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## Section of Otology.<sup>1</sup>

President—Sir CHARLES A. BALLANCE, K.C.M.G., C.B., M.V.O., M.S.

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### The Ear as Morphologically an Apparatus for Perceiving Depth below Sea-level: An Explanation of the close Anatomical Association of Cochlea and Vestibule.<sup>2</sup>

By H. HARTRIDGE, Sc.D., M.D.

(From the Physiological Laboratory, Cambridge.)

THE reason for the close anatomical association of structures so different in their physiological purposes as on the one hand the semicircular canals and otolith-organs, and on the other, the cochlea, has long been a matter for conjecture. The explanation here advanced is that the mammalian organ of hearing subserved in the ancestors of mammals the function of perceiving depth below the water surface, and was, therefore, associated with the semicircular canals and otolith organs in the identification of position and direction of travel.

This hypothesis is supported:—

(a) By the close mechanical analogy between the auditory apparatus of mammalia and the depth-controlling gear in the naval torpedo, and

(b) By the fact that in certain fish (Siluridæ) a mechanism is found consisting of ossicles, which connects the internal ear and the swim bladder. There is strong evidence that this is used for the perception of depth and not for audition.

*Analogy with Naval Torpedo.*—In the naval torpedo there are two mechanisms which operate together to control the depth at which the projectile shall travel beneath the surface of the water (1) a pendulum, (2) a hydrostatic membrane.

The former mechanism under the influence of gravity records any deflection that the long axis of the torpedo may make with the horizontal, and if such a deflection exists it turns the horizontal rudders in such a direction that the deflection tends to be corrected.

The hydrostatic membrane is a sheet rubber diaphragm situated in the side of the torpedo, being in contact with the water on the outside and on the inside with the air-filled interior. If the torpedo is at a depth below the surface the hydrostatic pressure tends to press the diaphragm inwards, but this motion is resisted by a spring. If the torpedo is at the correct depth at which it is required to travel, the hydrostatic pressure produces a force on the diaphragm that is just balanced by the force exerted in the opposite direction

<sup>1</sup> Meeting held at Gonville and Caius College, Cambridge, June 29 and 30, 1923.

<sup>2</sup> This communication was originally published in the *Journal of Physiology*, 1920, liv.

by the spring; but if the torpedo gets too deep or too shallow the forces no longer balance and the diaphragm is pressed inwards or allowed to move outwards respectively, and this causes appropriate deflection of the rudders. Experiment shows that either mechanism by itself causes an alternate deflection of the torpedo from the straight line, because of back-lash and friction. But it is found that if both pendulum and hydrostatic membrane operate together, the one eliminates the tendency to deflection introduced by the other, and the torpedo is thus caused to travel along a straight path at a given distance below the surface.

Applying these conclusions to the fish, it would seem clear that for the proper direction of path in three dimensions of space, at least two mechanisms would be required, namely, one to give information concerning depth below surface, and one to be influenced by gravity, and therefore to show deflection from the horizontal axis. Now the latter mechanism has long been identified as the otolith organ; the hypothesis advanced is that the tympanum, middle ear, and cochlea, are to be identified as the depth-recording mechanism.

The tympanum is on this view the counterpart of the rubber hydrostatic membrane of the torpedo. The ossicles are the counterpart of the levers which convey the motion of the diaphragm under varying hydrostatic pressure to the rudder-control mechanism, the stapedius muscle acting as the spring which in the torpedo balances the force produced on the diaphragm by the hydrostatic pressure. In fish, the homologue of the cochlea, with its basilar membrane, hair cells, and rods of Corti, is, on the above view, the mechanism by which the changes in position of the hydrostatic membrane (caused by changes in depth) are perceived by the sending of corresponding stimuli up the cochlear branch of the eighth nerve.

*Mechanism of the Ear in Fish.*—It is an essential feature of any mechanism for recording changes in hydrostatic pressure that one side of the diaphragm should be exposed to the fluid pressure (in this case water) while on the other there should be a readily compressible fluid such as a gas or air. And, further, since this gas or air is liable to be absorbed, there must be means provided of renewing the supply. In the case of shallow water fish and most reptilia, the supply of air in the middle ear may be readily renewed by the fish coming to the surface of the water and causing air to enter by way of the mouth and Eustachian tubes. (In some fish the air in the swim bladder is renewed in a similar way [1].) In the case of fish living below the surface, a different method of renewing the air has to be provided, namely, by forming a special connexion with the gas-secreting gland (the swim bladder). This may be effected by either connecting the swim bladder with the middle ear by a tube or duct (Owen [2] says this is the case in many osseous species of fish) or by making the swim bladder itself form the chamber, the change in volume of which (with change of depth below surface) brings about movements of the mechanism which connects with the internal ear. This is the arrangement found in carp, loach, and sleat-fish. About these fish Weber [3] wrote (quoting the abstract given by Owen):

A canal is sent from the sac of each vestibule to a common sinus impar . . . which communicates on each side by a small orifice with two sub-spherical atria . . . which atria are supplied externally by the ossicles *l* and *m*, and by means of the large ossicle *o* are brought into communication with the fore part of the air bladder *p*. Both the atria and common sinus are filled with endolymph . . .

Weber actually named these three ossicles the malleus, incus and stapes, and held that they existed chiefly in subserviency to the organ of hearing.

Müller [4] concluded that the air bladder in fishes, in addition to other uses, serves the purpose of increasing by resonance the intensity of the sonorous undulations communicated from water to the body of the fish. This conclusion is not accepted by Bridge and Haddon [5], who examined the anatomy of the swim bladder in many groups of fish. They state that the ossicles connecting the labyrinth with the swim bladder are less adapted to the conduction of fine sound vibrations than they are to the indications of gross changes in the capacity of the air bladder such as would be brought about either rapidly by external changes in the hydrostatic pressure or slowly by alteration in the volume of gas in that organ by secretion or re-adsorption. They advance the following points against Weber's theory (that the mechanism has an auditory function): (1) Sound vibrations would be passed from water to the air in the swim bladder with great loss of intensity. (2) In many Siluridæ, the walls of the air bladder are very thick, and are therefore ill-adapted for conducting sound. (3) The ossicles have considerable inertia, have no useful lever action, and are not firmly connected (as they are in the ears of mammalia), so that they can vibrate rapidly under the action of sound waves as one concrete mechanism. (4) There is no evidence that the Siluridæ, which possess this mechanism, have exceptional powers of hearing.

None of the above objections apply to the alternative view that the mechanism is used for measuring air-bladder volume under changes of hydrostatic pressure. It would seem clear, therefore, at all events so far as this type of mechanism is concerned, that the perception of depth and corresponding control of direction is the function performed, and not that of audition.

The intermediate type of mechanism described early in this paper, in which the water exerts its pressure directly on an externally placed tympanum (as it does in the naval torpedo) now comes up for examination. It is at once found that so far as the anatomy and mechanical features of the apparatus are concerned, the function performed could be either that of audition or that of depth perception. And it would seem that both functions could be efficiently performed without modification. If any differentiation were possible, one would say that those fish in which the mechanism appears to be well designed (so far as freedom from friction, inertia losses and back-lash are concerned) are more likely to employ the organ for audition than for depth perception, but this is probably a purely artificial criterion. It is probable, then, that in this type we see the direct connecting link between the depth-perceiving mechanism used by fish, and the apparatus for audition found in mammalia.

#### SUMMARY.

The view is advanced that the auditory apparatus of mammalia which is represented by the cochlea, fenestræ, ossicles and tympanum, once formed in their ancestors an apparatus for perceiving not sound vibrations, but the depth below sea-level. If this view be correct it would explain why the cochlea, the semicircular canals and the otolith organs are associated together anatomically and have a common nerve, since if the cochlea perceives depth below surface, all three would be directly concerned with the perception of position and the control of direction.

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## The Work of Sherrington on the Physiology of Posture.

By F. M. R. WALSHE, M.D.

To those unfamiliar with modern notions on the physiology of the nervous system, perhaps the main difficulty in understanding the beautiful researches of Magnus and de Kleijn on labyrinthine functions, will be the prevailing vagueness as to what is to be understood when we speak of muscle tone.

The definition which still too often passes muster, to the effect that tone is the state of active tension characteristic of living muscle in functional connexion with the nervous system, is now realized to be profoundly inadequate in the light of modern knowledge.

It is a matter of pride that we owe to the English physiologist Sherrington the fundamental facts in this connexion. In a series of now classic researches carried out during the past thirty years, he has revealed to us the physiological significance of muscle tone and the factors governing its maintenance. As Sir Charles Sherrington himself is unable to be present at this meeting to give a *résumé* of what he has done, I have undertaken to present you, in the simplest possible terms, with a concise summary of his work.

Before Sherrington's time, experimental physiology had yielded very contradictory results, and even the existence of muscle tone was in question. The fundamental experiment, from which all subsequent investigations have arisen, was that in which, after transection of the brain-stem of the cat in the region of the tentorium, Sherrington found that certain muscles had entered into a state of increased tone—hypertonus—which provides an excellent medium for the examination and analysis of the phenomenon. The animal preparation, thus produced, is known as the decerebrate animal, and the transection is found to pass through the pons, cutting off the cerebral hemispheres and midbrain.

The increased tone is selective in its incidence, being confined to those muscles which maintain the animal in its natural standing posture, namely, the extensors of the limbs, neck, and back, and the elevators of the jaw and tail. This great physiological group Sherrington speaks of as the "anti-gravity" muscles, and its antagonists, the limb flexors, the flexors of the neck and back and the depressors of the jaw and tail are found to be reciprocally inhibited and atonic. At once the meaning of muscle tone becomes clear; it is nothing else than the basis of posture, and decerebrate rigidity, as the phenomenon is called, constitute reflex standing. We may, therefore, define muscle tone as the basis, the raw material of posture.

The second fundamental observation lay in the discovery that muscle tone is a true reflex, dependent upon the integrity of the afferent nerve supply of the tonic muscle. If, by posterior root section, we deprive the muscle of its afferent nerve supply at once, it loses its tone. The reflex is, therefore, a deep or proprioceptive one, arising in the muscle itself.

If the brain-stem of the decerebrate animal be again divided at any level below that of the calamus scriptorius, the hypertonus at once disappears from the musculature, an observation which shows that the reflex arc concerned in the maintenance of tone is a long one, having its "centre" in the brain-stem, that is, in the pons and upper part of the medulla.

Sherrington found that if the decerebrate animal be planted on its legs

it can stand, and further, that its attitude can be modified by passive manipulation of the limbs, which tend to retain the new posture imposed upon them in this way. In other words, the tonically contracted muscle can maintain its tone at different lengths. If it be stretched, it keeps its new length, and, similarly, if its ends be approximated by movement at a joint, it remains shortened. These two reactions, the "shortening" and the "lengthening" reactions also occur when the limb actively takes up a new attitude under appropriate reflex stimulation, and they give to muscle tone the quality known as "plasticity." From another point of view they may be said to confirm the conclusion that muscle tone is a postural reflex.

Although the maintenance of tone demands the integrity of the muscle's afferent nerve supply, Sherrington made the interesting observation, out of which Magnus and de Kleijn's subsequent work has arisen, that the tone could be influenced, or modified, by impulses arising elsewhere in deep structures, muscles, tendons, and joints. Thus rotation of the head to one side increased the extensor hypertonus of the limb muscles on the side to which the animal's snout was turned, and diminished that of the opposite limbs. Other tone-regulating influences took their origin in the labyrinths and in the muscles of the limbs.

We see, then, that there are two reflex mechanisms to be considered, that concerned in the maintenance and that in the regulation of muscle tone. The work of Magnus and de Kleijn has been devoted to the analysis of the latter and may be said to have solved the main physiological problems of postural regulation, because the tonic reflexes arising in neck, limbs and labyrinths are the reflex processes by means of which the animal normally takes up and maintains all those attitudes which are natural to it during life.

There are two elements in the co-ordination of movement—the phasic or movement element and the tonic or postural element. Muscle tone is the basis of the latter, and its loss results in the grossest disorders of voluntary movement. All those disorders with which we are familiar as components of cerebellar ataxy may be regarded as the inevitable result of loss or diminution of postural tone. It is, therefore, clear that a precise conception of what the physiologist understands by muscle tone is essential, not alone to the comprehension of Magnus and de Kleijn's beautiful researches, but also to the study of all disorders of movement.

Interesting as these purely experimental observations are, they become increasingly important to us if we can show that they can be applied to man. There are reasons for supposing that the so-called spasticity of hemiplegia and the extended form of paraplegia are physiologically, that is qualitatively identical with Sherrington's decerebrate rigidity, and we should naturally expect to find associated with it some of these tonic reflexes, these regulating reactions, which Magnus and de Kleijn have described. They are, in fact, to be found in nearly every case of spastic residual hemiplegia. Thus the associated movements of the paralysed limbs are identical in character with the tonic reflexes found by Sherrington to be produced by passive movements of the limbs of the decerebrate animal. In man, also, rotation of the head may give rise to variations in the attitude and in the tone of the paralysed arm. The associated reactions mentioned may be reversed in form by head rotation, or by variations in the position of the head in relation to space. Further, phasic reflexes, that is, reflex movements as opposed to reflex postures, may be modified in form by variations in the position of the head. Certain changes in the Babinski type of plantar response, to take but a single example, may be produced by this means.

In conclusion, we see how far knowledge has advanced from the simple definition of muscle tone as merely "a state of active tension" in a muscle, and, for our present purpose, we may speak of it appropriately as the basis of posture.

## Experimental Physiology of the Labyrinth.

By A. DE KLEIJN (Utrecht).

I WISH to begin by thanking the members of this Society for the opportunity they have given me to speak about the work conducted in the Pharmacological Institute at Utrecht during the last ten years. My thanks are also due to Mr. A. R. Tweedie and Dr. F. M. R. Walshe for the excellent summaries of this work they have given in the English journals.

As early as in 1875 Breuer, the most famous otological physiologist still living, far outstripping the ideas of his age, gave it as his opinion that: "labyrinthine reflexes must be divided into two different groups:—

(I) Reflexes responding to movements (reflexes of the semicircular canals).

(II) Reflexes resulting from position (reflexes of the otoliths).

This division is in entire accord with anatomical researches; on the one hand the cristæ and cupulæ of the semicircular canals freely moving in the endolymph are adapted to react to movements of the head; on the other hand the maculæ covered by the otolithic membranes with their greater specific weight are specifically adapted to react to changes in pressure of these membranes produced by alteration in position of the head in space.

That this quite new division of Breuer was not accepted by his contemporaries can easily be understood. Though, after the ingenious experiments of Breuer himself it was not possible to deny that the reflexes corresponding to movements must arise in the semicircular canals, his deduction of the existence of a new group of labyrinthine reflexes resulting from position was not sufficiently supported by experiments to be generally accepted.

His experiments concerning the rotatory reactions and after-reactions on the head and on eyes were so ingenious that up to the present time it has only been possible to complete his work by finding new details. Except for the past-pointing tests of Bárány, all our rotation-tests are only clinical applications of Breuer's experiments.

As otolithic reflexes Breuer considered:—

(a) Progression reactions.

(b) Compensatory eye positions.

(c) Rotation of the head after unilateral labyrinth extirpation.

As to progression reactions, known in 1875 only as subjective sensations, I hope to show you later on that these reactions cannot be considered as pure otolithic reflexes.

The knowledge of the compensatory eye positions was in 1875 quite incomplete; sufficient investigations had not been made.

Most of the investigators of Breuer's time considered the rotation of the head after unilateral labyrinth extirpation to be caused by post-operative cerebral lesions. No method was known of examining separately the functions of the semicircular canals and of the otoliths.

Briefly, the opposition to Breuer's classification of the labyrinthine reflexes must be ascribed to the insufficient knowledge of the *tonic* labyrinthine reflexes in his time.

Forty years later, thanks to the famous investigations of Sherrington on decerebrate animals, it has been possible to find other tonic labyrinthine reflexes. Before passing on to the description of the different labyrinthine reflexes I shall first discuss, how it is possible to examine separately the functions of the semicircular canals and of the otoliths. It could evidently be done in the following way: either by extirpation of the semicircular canals alone, or of the otoliths alone. Then the reflexes arising in the extirpated part of the labyrinth would be lacking and the reflexes arising in the remaining part would be present.

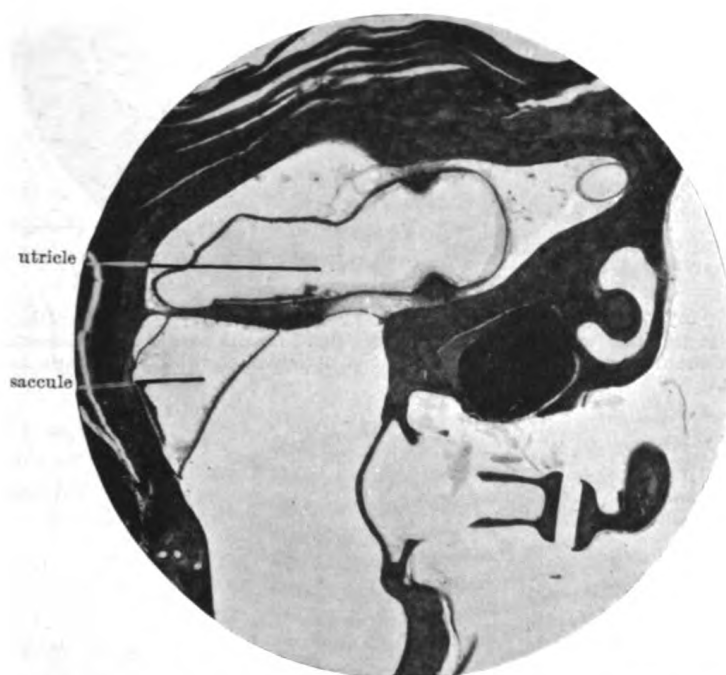


FIG. 1.—Labyrinth of a normal guinea-pig. Sensory epithelium of saccule and utricle covered by the otolithic membranes.

Such experiments, possible on fishes, as is known from the investigations of Kubo, Benjamins, Maxwell and others, have so far not been carried out on mammals.

Wittmaack showed that it is possible to detach separately the otolithic membranes by centrifugalizing normal guinea-pigs under ether narcosis at a rate of 1,000 metres per minute. By this method only the otolithic membranes are detached, the *sensory epithelium* of the maculae remaining *quite intact* (figs. 1, 2, 3). Hence the behaviour of an animal with detached otolithic membranes on the one hand and that of an animal with paralysed extirpated maculae on the other hand is not quite the same.

Special experiments have shown that the sensory epithelium of the maculae is in a permanent condition of stimulation. In normal animals with intact

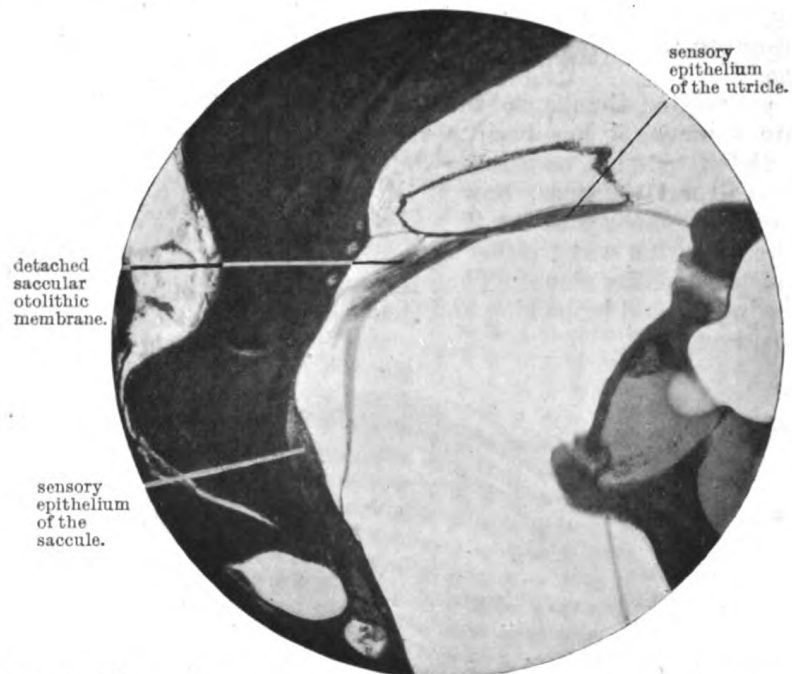


FIG. 2.—Labyrinth of a guinea-pig after centrifugalizing. Sensory epithelium of saccule and utricle without otolithic membrane. In the corner of the saccule is the detached otolithic membrane. The detached utricular otolithic membrane is shown in fig. 3.

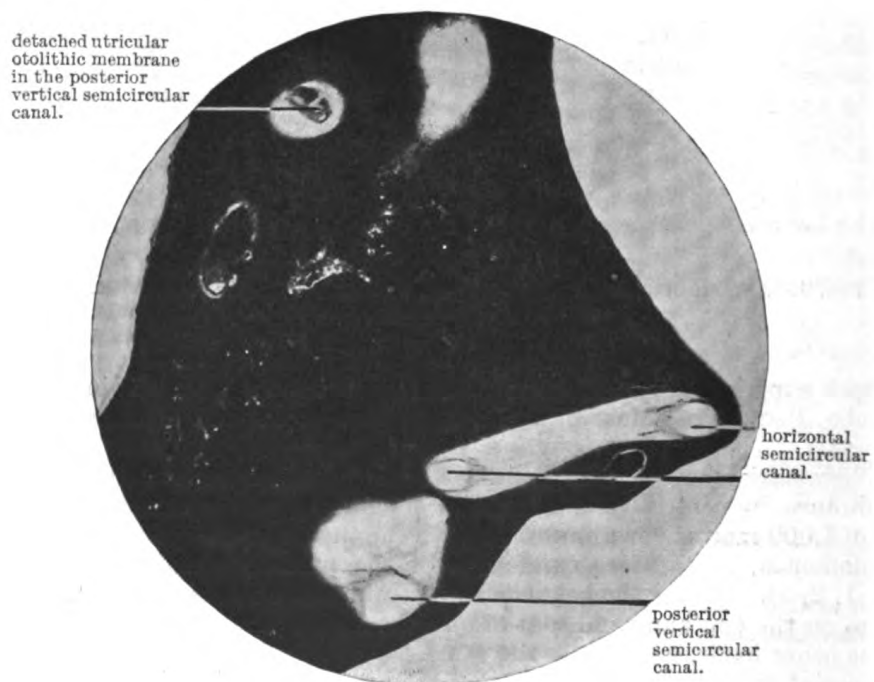


FIG. 3.

otolithic membranes this condition of stimulation varies in different positions of the head in space in proportion to the changes in pull or pressure of the otolithic membranes.

In an animal with detached otolithic membranes there is a permanent condition of stimulation of the maculæ; change of position of the head in space does not, however, elicit a *change* of this stimulation as in normal animals, because the otolithic membranes are absent and cannot pull or press. On the contrary in an animal with paralysed, extirpated maculæ there is no condition of stimulation at all.

As we have seen, up to the present time it has not been possible in mammals to extirpate the different maculæ separately. However, during the past year de Burlet and Hofman have succeeded at the Anatomical Institute at Utrecht in preparing the different maculæ of several mammals by using for this purpose Zeiss's operating microscope. Applying this method, it has recently been found possible at the Pharmacological Institute to extirpate separately the saccular maculæ from living rabbits. This method can only be applied to the saccular maculæ, because the utricular maculæ are so closely connected with the semi-circular canals that a separate extirpation is not possible without injuring the latter. Perhaps it may be possible to succeed in the following way: Some months ago, in operating on a living rabbit, it was intended to make a partial resection of the labyrinth. From the microscopical investigation it was found that only the cochlea was removed, and that the saccular nerve was cut through. Therefore we tried to see if it were possible to cut through the different nerves (nerve from the saccule, nerve from the utricle, &c.), without opening the labyrinth at all. Experiments on skulls showed that this is really possible, and in the near future such experiments on living rabbits will be undertaken.

We have seen that up to the present time the different labyrinthine reflexes could be examined separately by the method of Wittmaack, but we hope in the near future to be able to divide, with the aid of the operating microscope, the different nerves of the labyrinth without injuring the labyrinth itself.

Returning to the description of the different labyrinthine reflexes I classify them in the following way:—

(I) Reflexes responding to movement (reflexes of the semicircular canals).

(1) Rotatory reactions and after reactions (with nystagmus): (a) on head; (b) on eyes.

(2) Progression reactions.

(II) Reflexes resulting from position (tonic reflexes, reflexes of the otoliths).

(1) Tonic labyrinthine reflexes on the body musculature.

(2) Labyrinthine "righting" reflexes.

(3) Compensatory eye positions.

This division is in complete agreement with Breuer's classification except that Breuer considered the progression reactions to be reflexes of the otoliths. To this difference I shall revert later on.

(I) REFLEXES RESPONDING TO MOVEMENT (REFLEXES OF THE SEMICIRCULAR CANALS).

(1) *Rotatory Reactions and After-reactions (with Nystagmus) on Head and on Eyes.*

As I mentioned above, Breuer himself described these reactions very exactly, and we all use them so much in our clinical work that it is unnecessary to describe them here.

(2) *Progression Reactions.*

The chief progression reactions are the following:

(a) *Lift reaction.*—If, for instance, you place a normal animal upon a board and move the board slowly upwards, the limbs are flexed, whilst at the termination of the movement they are extended. The reverse occurs with a downward movement.

(b) *Toe-spreading reflex.*—If we hold a guinea-pig in the air vertically, grasping it with one hand about the axillæ, and the toes of the hind limbs are stroked together, then during any slight movement in a vertical direction either upwards or downwards a spreading out of the toes will occur.

(c) *Springing reflex.*—If any animal is held horizontally with one hand around the neck and the other around the loins, a sudden passive movement of the animal forwards and downwards will induce extension forwards of the limbs. This reflex is of great practical value to animals, as it enables them when jumping down to the floor to catch the weight of the body by the extended forelimbs.

All these reflexes are labyrinthine reflexes; they disappear after bilateral labyrinth extirpation. They are reflexes arising in the semicircular canals, because they persist when the otolithic membranes are detached.

As you know, Breuer and Mach thought that the semicircular canals could not react upon progression, on the ground that they formed a system comparable to a closed vessel with rigid walls. However, the walls are not rigid; the endolymph is connected with the endolymphatic sacculus, and the perilymph is enclosed in a wall with two elastic fenestræ (fenestra rotunda and fenestra ovalis). By means of a model made by Ornstein and Burger at Utrecht it was demonstrated that, under the circumstances above mentioned, the semicircular canals really can react upon progressive movements.

It may be possible, however, that progression reactions also arise from the otoliths. I use the word *may* because we cannot make a control experiment. Removal of the semicircular canals separately, without injuring the otoliths, has not been possible, as mentioned before, up to the present time. How the otolithic membranes *might* produce progression reactions, I will show by an example. At the end of the lift reaction upwards, when the movement is stopped, the utricular otolithic membranes will pull on the maculæ in consequence of their inertia and will produce by that means an extension of the limbs, as will be shown later on. This is actually seen in the lift reaction at the termination of the movement upwards. Therefore I believe that the progression reactions occur through a combination of reflexes arising both in the semicircular canals and in the otolithic organs.

## (II) REFLEXES RESULTING FROM POSITION (TONIC LABYRINTH-REFLEXES, REFLEXES OF THE OTOLITHS).

In speaking about *tonic otolithic labyrinthine* reflexes one must be assured of the following points:—

(a) That they are really *labyrinthine* reflexes and therefore disappear after bilateral labyrinth extirpation.

(b) That they are *otolithic* reflexes, that is to say they disappear when the otolithic membranes are detached.

(c) That they are *tonic*, that is to say their influence persists as long as a certain position of the head in space is maintained.

In these investigations we must pay attention to the following points:—

(i) In order to exclude tonic neck reflexes arising from the neck muscles the position of the head in relation to the neck and trunk must remain fixed. The most simple way to exclude these neck reflexes is to fix the head and trunk in a plaster cast. A second method consists in cutting through the dorsal cervical nerve roots. After section of these nerves all tonic neck reflexes disappear.

(ii) It is necessary to exclude reflexes in response to movement (reflexes of the semicircular canals). These reflexes are transitory; the reflexes of the otoliths are tonic. Therefore after any change in the position of the head in space it is necessary to wait a few minutes before examining the tonic labyrinthine reflexes.

(1) *Tonic Labyrinthine Reflexes on the Body Musculature.*

Independently of each other Sherrington and Magnus discovered, that after rotation of the head of a decerebrated animal the extensor tone in the limbs is changed. Now if we bring the head of an animal from one position in space into another, we do two things: first, we change the position of the head in space and secondly we change the position of the head in relation to the trunk. Special experiments have shown that if the position of the head in space is altered, but its position in relation to the trunk remains the same, *only* labyrinthine reflexes arise (from stimulation of the otolithic organs) to influence the extensor tone in the body musculature.

There is only *one* position of the head in space in which the extensor tone is maximal, namely when the animal is on its back with the line of the mouth about  $45^\circ$  above the horizontal plane. There is only *one* position of the head in space, in which the extensor tone is minimal, namely when the animal is in the standing position with the line of the mouth about  $45^\circ$  below the horizontal plane (fig. 4). In all other positions the degree of extensor tone is between the maximum and minimum.

If we wish to solve the problem "in which maculae do these reflexes arise?" we can use a model of a labyrinth which shows that the position of the different maculae completely agrees with the exact measurements of de Burlet and Koster (fig. 5).

If we place such a model in the position for maximal extensor tone it will then be seen, that while the saccular maculae have no special position, the utricular maculae have a very typical position: they lie horizontally, the otolithic membranes pulling at the sensory epithelium. If we then place the model in the position for minimal extensor tone, it will be seen that the



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utricular otoliths again occupy a very typical position whereas this is not the case with the saccular otoliths. The utricular otoliths lie horizontally, the otolithic membranes pressing on the sensory epithelium.

The utricular maculæ also influence the *flexor* tone of the limbs: in the position for maximal extensor tone the influence on the flexor tone is minimal; in the minimal extensor position the influence on flexor tone is maximal. If one labyrinth only is extirpated, it is seen that the maximum and minimum positions and their influence on extensor and flexor tone remain the same; this showing that each labyrinth is connected with the muscles



FIG. 4.—Decerebrate cat. Spinal cord cut through, therefore only reflexes on the forelimbs. The head and trunk are fixed in a plaster cast. Cinema photographs. (a) Minimal position for the labyrinthine reflexes. Flexion of the forelimbs. (b) Animal in maximal position for the labyrinthine reflexes. Extension of the forelimbs. The extension is very strong; it takes place against the pulling of an elastic cord.



FIG. 5.—Model of the maculæ (not in typical position).

of both sides. The utricular maculæ also influence the tone of the neck muscles, but the maximum and minimum positions are the same as for the limbs. There is only one great difference; each utricular macula is only connected with the neck muscles of *one* side; this is one of the reasons why after unilateral labyrinth extirpation the head turns to the operated side.

With regard to the influence of *tonic neck reflexes on the body musculature*. Tonic neck reflexes arise when the position of the head in relation to the trunk is changed; they disappear after section of the dorsal cervical nerve-roots and can be investigated free from complications after bilateral labyrinth extirpation or by changing the position of the head only in the horizontal plane.

(For instance, changing the position of the head by rotation when the animal is in the vertical position, by ventral flexion and dorsal flexion when the animal is in the lateral position, or e.g., by lateral flexion of the head to one shoulder in animals in the normal standing position.)

The tonic neck reflexes influence the tone of the muscles of the limbs nearly always in an asymmetrical manner; rotation of the head or lateral flexion of the head causes increase of extensor tone in the limbs on the side to

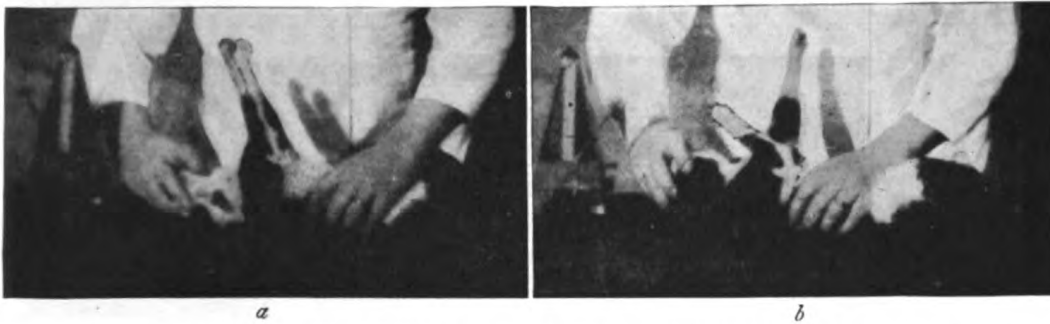


FIG. 6.—Decerebrate cat. Spinal cord cut through. (a) Animal on its back: strong extension of the two forelimbs. (b) Rotation of the head. Flexion of the right forelimb (on the side to which the vertex is directed); extension of the left forelimb (on the side to which the snout is directed).

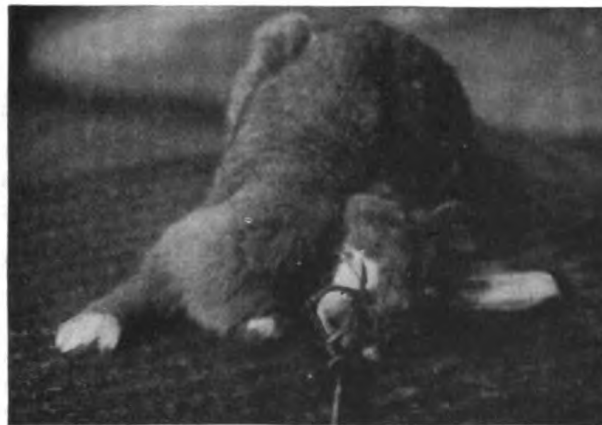


FIG. 7.—Rabbit after labyrinth extirpation on the left side.

which the snout is directed and diminution on the side to which the vertex is directed (fig. 6). Dorsal flexion of the head causes in most animals extension of the fore-limbs and flexion of the hind-limbs; ventral flexion of the head on the contrary causes flexion of the fore-limbs and extension of the hind-limbs. Tonic neck reflexes therefore (and this is also true of tonic labyrinthine reflexes) cause in many cases such postures as are frequently assumed for various purposes by normal animals.

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Inadequate knowledge of these tonic neck reflexes has resulted in many wrong conclusions. I shall only give you one example. After unilateral labyrinth extirpation you see a rotation and lateral flexion of the head to the operated side; the limbs on this side are flexed and adducted, the limbs on the other side extended and abducted (fig. 7). This was understood to be a direct consequence of the unilateral labyrinth extirpation; in reality it is only the result of tonic neck reflexes. If you bring the head into the normal position the extension and flexion, the abduction and adduction of the limbs disappear and the animal sits quite normally (fig. 8).

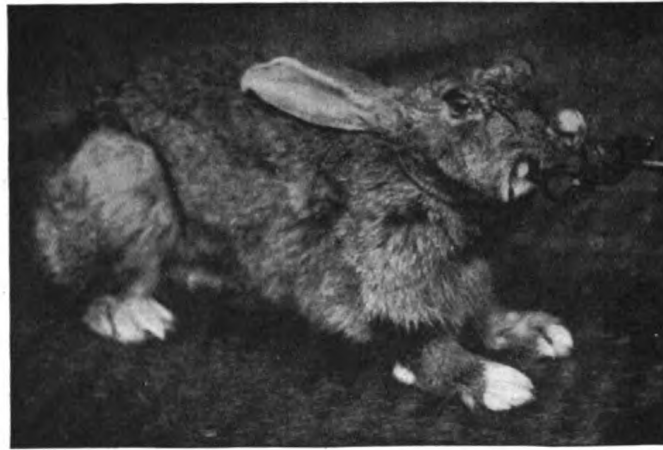


FIG. 8.

### *Combination of Tonic Labyrinthine and Tonic Neck Reflexes.*

We have spoken about tonic labyrinthine and tonic neck reflexes separately. If you bring the head of an animal into a new position in space, you nearly always change the position of the head both in relation to the horizontal plane and in relation to the trunk. Speaking quite generally we may say that, in all positions, the result of the combined influences is always the algebraic sum of the influences caused by the tonic labyrinthine reflexes and by the tonic neck reflexes.

Again, suppose the animal to be in the lateral position (i.e., on its side), head symmetrical to trunk. If we now turn the head so that the snout is directed upwards, we have the following situation: Tonic labyrinthine reflexes increase the extensor tone in all four limbs, because the utricular maculae are brought into a position nearer the maximal for extensor tone; tonic neck reflexes influence the extensor tone in such a way that, in the limbs which lie uppermost, it is increased (the limbs on the side to which the snout is directed) while in the under limbs it is diminished (the limbs on the side to which the vertex is directed). The result will be the following: The limbs lying uppermost gain in extensor tone from both the labyrinths and the neck, and they will show very strong extension; the under limbs get an increase of extension from the labyrinths, and diminution of extension from the neck. Now, if the labyrinthine influence is stronger, they will become more extended, whereas if the neck influence is stronger they will become less extended, and if the labyrinthine and neck reflexes are equally strong nothing will occur. Tonic

labyrinthine and tonic neck reflexes can be very well studied in all decerebrated animals. They can also be studied in normal animals with few voluntary movements, like rabbits, guinea-pigs, &c., or in animals with many voluntary movements, like cats, dogs, monkeys, &c., if they are under narcosis.



FIG. 9.



FIG. 10.

FIG. 9.—Child with amaurotic family idiocy. Cinema photograph. Child in "maximal" position for the labyrinthine reflexes: extension of all limbs.

FIG. 10.—Same child as in fig. 9 in "minimal" position for the labyrinthine reflexes; disappearance of the extensor tone.



FIG. 11.—Child with idiocy. Rotation of the head to the left. Flexion of the right limbs (on the side to which the vertex is directed); extension of the left limbs (on the side to which the mouth is directed).

#### *Clinical Observations on Tonic Labyrinthine Reflexes.*

A complete investigation of these reflexes in patients is very difficult, because to exclude tonic neck reflexes it is necessary to fix the head and trunk in a bandage. Therefore it is really only possible in children. Up to now

four cases have been observed in the neurological clinic at Utrecht in children with severe cerebral disturbances. In two cases with amaurotic family idiocy the maximum and minimum positions were practically identical with those found in animals (figs. 9 and 10); in two other cases the maximum and minimum positions were found at the same places, but reversed, the maximum position for the extensor tone being with the line of the mouth about  $45^{\circ}$  below the horizontal plane, and the minimum position with the line of the mouth about  $45^{\circ}$  above the horizontal plane.

*Tonic neck reflexes* have been found by different examiners in many patients with severe cerebral disturbances. The reflexes are practically identical with those found in animals (fig. 11).

During recent months two very interesting papers by Simons and Walshe have appeared relating to tonic neck reflexes in the so-called "associated movements" of hemiplegia. The best way to elicit these "associated movements" in a paralysed leg is to cause the patient to make a forcible contraction of the normal arm. Tonic neck reflexes can then be produced in the paralysed



FIG. 12.—Labyrinthine "righting" reflexes. Guinea-pigs held by their loins free in the air. Normal guinea-pig (right): the head is brought to the normal position. Guinea-pig lacking righting reflexes (left): head in lateral position.

legs by changing the position of the head in relation to the trunk. By a very careful analysis of the different reflexes observed in his patients during these "associated movements" Walshe was also able to demonstrate tonic *labyrinthine* reflexes and a combination of tonic labyrinthine and tonic neck reflexes; all practically identical with those found in animals. These investigations of Simons and especially those of Walshe are important because they have shown us how to study these reflexes in conscious patients.

## (2) *Labyrinthine "righting" reflexes.*

You are all aware of the following experiment: If you hold a normal guinea-pig in the air by its loins free and you bring the trunk into different positions in space, the head always tends to remain in the normal position and does not conform to the different positions of the trunk. Breuer showed that this phenomenon must be ascribed to labyrinthine reflexes; after bilateral labyrinth extirpation it disappears (fig. 12). These labyrinthine "righting" reflexes acting on the head can be studied in normal guinea-pigs and rabbits; in all other animals the cerebral hemispheres must first be removed, but the

mid-brain, especially the nucleus ruber, must be left quite intact because the centres of these reflexes are situated there (Magnus), in the nucleus ruber especially (Rademaker). These animals without hemispheres are incapable of voluntary movements, but otherwise their behaviour is that of normal animals and what is most important, the muscle-tone is not changed.

If we wish to discover in which part of the labyrinth these reflexes arise, it is useful to investigate them after unilateral labyrinth extirpation.

After unilateral labyrinth extirpation the righting reflexes cause the animal to turn the head into such a lateral position that the intact labyrinth is uppermost. In this position the animal is quiet, the righting reflexes being minimal. If the intact labyrinth is underneath, then the righting reflexes are at their maximum and the animal does not rest until it has succeeded in bringing the intact labyrinth uppermost into the minimum position again. If we now bring the model of the labyrinth into the maximal position you can see that the macula of the saccule has a "typical" position, whereas the utricular macula has not a typical position at all. The macula of the saccule lies horizontally; the otolithic membrane *pulls* at the sensory epithelium; in the minimal position the macula of the saccule also lies horizontally but the otolithic membrane *presses* on the sensory epithelium. *With regard to the labyrinthine righting reflexes it is therefore proved with certainty that the strongest stimuli occur when the otolithic membrane is pulling at the sensory epithelium.*

When both labyrinths are intact, the head is always brought back from an asymmetrical to a symmetrical position; that is to say the head only comes to rest in such a position, in which the stimuli arising from both saccular maculae are equally strong. There is only one difficulty; the head comes to rest not only in a symmetrical position, but also in a very typical position, the vertex above, the lower jaw below, and the occipito-incisor axis of the head about 35° below the horizontal plane. Whether the saccular or the utricular maculae are the cause of this "typical" position we do not know; perhaps we may say the head comes to rest in such a position, that both the saccular maculae have a symmetrical position, and both the utricular maculae occupy a minimal position (the otolithic membranes pressing on the sensory epithelium).

After bilateral labyrinth extirpation all mid-brain animals, and such normal animals as have few voluntary eye movements, lose their labyrinthine righting reflexes. Normal animals however, with a wide range of voluntary eye movements (dogs, cats, monkeys, &c.) held free in the air after bilateral labyrinth extirpation, right their head by using their eyes. These *optical* righting reflexes disappear when the animals are blindfolded.

Magnus has described other groups of righting reflexes:—

(a) If a guinea-pig is held free in the air in the lateral position by its loins, the head is brought into the normal position, and not only the head, but also the trunk as far as possible. This is caused by *righting reflexes arising in the neck and acting on the trunk.*

(b) A further group of righting reflexes are those *arising in the body walls and acting on the head.* These can be examined after bilateral labyrinth extirpation. In these circumstances a guinea-pig does not bring its head into the normal position when it is held free in the air in the lateral position. But when the same animal is laid on its side on a table it does bring its head immediately into the normal position. That these reflexes arise from an asymmetrical stimulation of the body walls is demonstrated by the following experiment: if a board is laid upon the free upper body wall these reflexes disappear because the asymmetry of the stimulation of the body walls has ceased



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(c) *Righting reflexes arising in the body walls and acting on the trunk and limbs.* If a guinea-pig is laid on its side and the head is fixed in this position, the animal rights its trunk and limbs immediately and brings these into the normal position. The experiment with a board shows that these reflexes also arise from asymmetrical stimulation of the body walls.

We have thus five different groups of righting reflexes: Three acting on the head (labyrinthine reflexes, optical reflexes, and reflexes arising in the body walls) and two acting on the trunk and limbs (neck reflexes and reflexes arising in the body walls).

With these different groups of righting reflexes the animal is able to bring its head, trunk and limbs from any abnormal position back to the normal again.

### *Clinical Observations.*

Till now labyrinthine righting reflexes have been observed in one case only, that of a child with anencephaly (Neurological Clinic, Utrecht). This child,

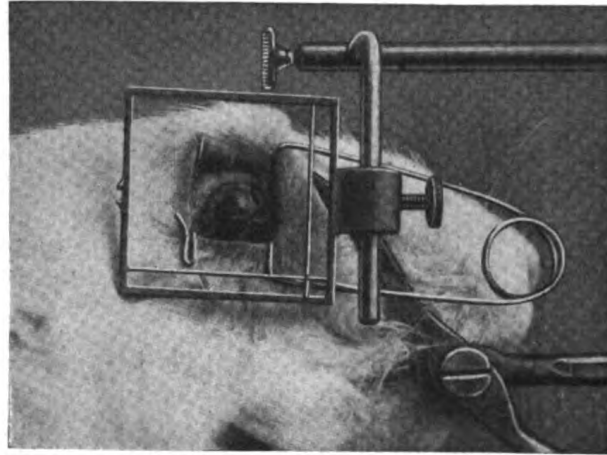


FIG. 13.

held by its trunk free in the air in the lateral position, immediately brought its head into the normal position again.

### (III) COMPENSATORY EYE POSITIONS (TONIC LABYRINTHINE REFLEXES ON THE EYE MUSCLES).

The study of the compensatory eye positions dates from about a century ago. Interesting investigations have been made by Hunter, von Graefe, Hoyer, and others; however all these observers made the same mistake. They did not distinguish the compensatory eye *movements* responding to movements of the head and arising in the semicircular canals from the compensatory eye *positions* resulting from the special position of the head in space and arising in the otoliths.

Breuer, however, took the right view of this question in his classification of the labyrinthine reflexes already mentioned. The compensatory eye positions can be studied in normal animals with two separate visual fields (and, in consequence of this, with few voluntary eye movements) such as rabbits, guinea-

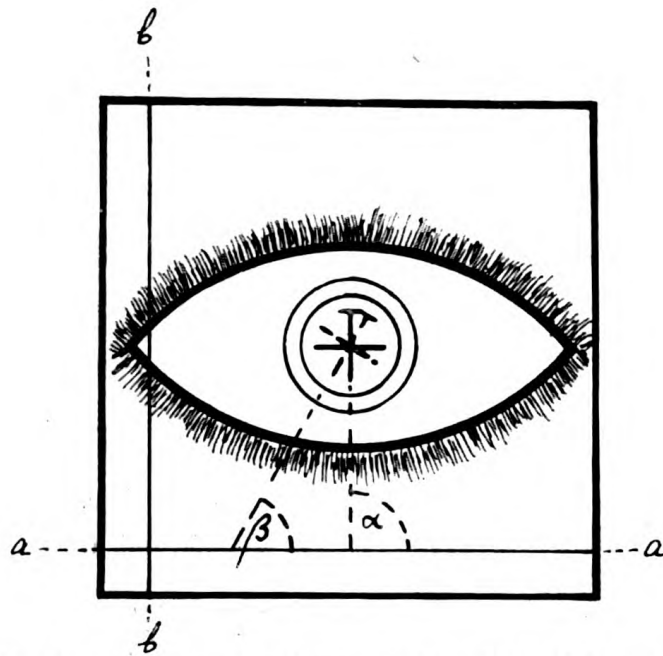


FIG. 14.—Cross on the eye in two different positions by two different positions of the head in space. The rotatory displacement is the difference of angle  $\alpha$  and angle  $\beta$ .  $aa$  and  $bb$  = thin wires of the frame.



FIG. 15.



pigs, &c. For all other normal animals with binocular visual fields and many voluntary eye movements (cats, dogs, monkeys, &c.), narcosis is necessary. These reflexes are important, because they are the only tonic labyrinthine reflexes which can be measured exactly.

The best way to study them in rabbits is the following: The whole of the animal's body is tied to a board so that it cannot move, and the head is firmly held in Czermak's head-clamp so that its position is in accurate relation to the trunk. This board can be brought into all different positions in space by being turned in different axes. A frame with two thin wires is fixed to the head-holder of Czermak, and under cocaine a cross is branded on the cornea. Opposite to the animal a cinema kodak is fixed on the board to photograph the positions of the eyes in different positions of the head in space. If we now photograph the corneal cross and the wires together, we can record, exactly and easily, changes in the vertical, lateral, and rotatory positions of the eye (figs. 13, 14, pp. 18, 19).

We know of two varieties of labyrinthine compensatory eye positions: rotatory and vertical.

#### (1) *Rotatory Compensatory Eye Positions.*

There is only one position of the head in space in which the rotatory displacements of the eyes with the upper pole towards the nose are maximal, namely, with the head in the vertical position, the snout above. There is only one position of the head in space in which the rotatory displacement towards the ears are maximal, namely, with the head in the vertical position, the snout below. We do not know which part of the labyrinth originates the rotatory compensatory eye positions. If we take the model of the labyrinth, then it becomes clear that in the two above-mentioned positions neither the saccular nor the utricular maculae have a typical position. The supposition some time ago suggested by Magnus and myself that they arise in the so-called corners of the saccular maculae (the little part of the saccular maculae innervated not by the saccular but by the utricular nerve) cannot be right, taking into consideration the latest anatomical investigations of de Burlet and de Haas.

Both eyes always undergo a rotatory displacement in the same direction in the sagittal plane of the head. After unilateral labyrinth extirpation the rotatory eye displacements are only diminished. They are not changed in character.

#### (2) *Vertical Compensatory Eye Positions.*

Concerning vertical compensatory eye positions I can only repeat what was said about the labyrinthine righting reflexes. If we want to know in which part of the labyrinth these reflexes arise, it is practical to study them after unilateral labyrinth extirpation. With respect to these compensatory eye positions, we can again prove with certainty that the strongest stimuli occur when the saccular macula lies horizontally, the otolithic membrane pulling at the sensory epithelium; that is to say, the vertical compensatory eye positions are maximal in the lateral position of the head, with the intact labyrinth underneath; the vertical eye positions are minimal or zero in the lateral position of the head with the intact labyrinth uppermost. Both eyes always undergo vertical displacement in the same direction in the frontal plane of the head, i.e., in the right lateral position of the head the right eye is deviated upwards, the left eye downwards (both having moved clockwise, seen from the front).

When both labyrinths are intact, we can say in general as much with respect to the rotatory as to the vertical eye positions that if the head passes from one position into another, the eyes perform such movements to attain their new position in the orbits, so that they, as it were, try to retain their original position in space. That they only *try* to retain their original position in space I shall demonstrate to you by the following example: If we bring the head from a position with the line of the mouth in the horizontal plane, into a vertical position with the snout below, the head moves through  $90^\circ$ . If the eyes are to remain in their original position in space, they must undergo a compensatory rotatory displacement also through  $90^\circ$ ; really the movement is only through about  $70^\circ$ . What is, then, the physiological gain from these compensatory eye positions? To consider this we must first consider

#### *Tonic Neck Reflexes on the Eye Muscles:*

These reflexes were discovered by Bárány in 1907. He showed that tonic reflexes on the eye muscles not only arise when the position of the head in space is changed but also when the position of the head is altered in relation to the trunk. He, however, studied these reflexes on normal rabbits with intact labyrinths and could not therefore make an exact analysis. Tonic neck reflexes can only be studied free from complications after bilateral labyrinth extirpation, or by changing the position of the head only in the horizontal plane. (They disappear after section of the posterior cervical nerves.) Special experiments have shown that if we fix the trunk of a rabbit after bilateral labyrinth extirpation and bring the head into different positions in space, the tonic neck reflexes *always* produce the same effect on the eye muscles as do the tonic labyrinthine reflexes: that is to say if the head is brought into another position the eyes always *try* to retain their original position in space.

These compensatory eye positions caused by the tonic neck reflexes are much less in degree than those caused by the tonic labyrinthine reflexes. It is by the combination of these two groups of tonic reflexes that it becomes possible for a rabbit to change the position of its head without altering the position in space of the eyes and therefore without alteration in the visual fields.

Special experiments showed that a rabbit can move its head from the *normal* position into other positions in the sagittal plane to the limit of about  $45^\circ$  upwards and  $45^\circ$  downwards, in the frontal plane by rotation to right or left up to about  $20^\circ$ , and in the horizontal plane up to about  $17^\circ$  in each direction (this last only through tonic neck reflexes) without changing the visual fields.

The striking coöperation of different reflexes is still much greater. If the animal wishes to bring its head into another position, it first must make a movement; this movement reacts upon the eyes by compensatory eye movements arising in the semicircular canals; when the head becomes stationary in its new position, this special position reacts upon the eyes by compensatory eye positions arising in the otoliths and neck; in other words the visual field remains unchanged during the movement in consequence of reflexes arising in the semicircular canals, and after the movement in consequence of reflexes arising in the otoliths and neck. This only concerns animals with two separate visual fields; other animals, such as cats, dogs, monkeys, &c., and also man, do not fix their visual fields by labyrinthine and neck reflexes, but by optical reflexes.

*Clinical Observations on Tonic Neck Reflexes on the Eye Muscles.*

Clinical observations about these reflexes are very seldom possible. Only two cases have been described by Simons in Berlin and two were observed in the Neurological Clinic in Utrecht in patients with tumor cerebri. The best way to study them is by fixing the head and bringing the trunk into another position in space.

*Labyrinthine Compensatory Eye Positions.*

These reflexes are of great theoretical and practical importance. They are the only *tonic* labyrinthine reflexes we can study in normal persons and which we can measure exactly. In studying these reflexes we must remember, however, that the eyes in men are placed in the frontal plane, and, therefore, we get, in lateral positions of the head, rotatory compensatory eye positions instead of vertical ones as in the rabbit. In different positions of the head in the sagittal plane we get vertical eye positions and not rotatory, as in the rabbit.

In which part of the labyrinth the vertical compensatory eye positions arise we do not know. The rotatory compensatory eye positions, from analogy with the experiments on animals, in all probability arise in the saccular maculae. Therefore it is of great importance to determine the rotatory eye positions in patients, because it is the only way to gain a knowledge of the functions of the otoliths, particularly those of the saccule. Up to the present time we have only two *objective* methods at our disposal for measuring the rotatory compensatory eye positions in man: the method of Bárány and the method of van der Hoeve.

The apparatus of Bárány you all know; it is very expensive, and besides it is only possible with this apparatus to investigate the combination of tonic labyrinthine and tonic neck reflexes on the eye muscles, the position of the iris being examined in different inclinations of the head to the shoulder. By this method not only is the position of the head in space changed but also the position of the head in relation to the trunk.

The apparatus of van der Hoeve is only applicable to patients with astigmatic eyes.

A simple method of measuring the compensatory eye positions in patients is the following; it is similar to that used in the case of animals. A pair of spectacles without glasses but with two thin cross-wires instead, is placed before the eyes. Now we need only a cross upon the cornea. This can be made by cutting from the dried shell-membrane of an egg, round pieces as large as the cornea, and marking a cross on these membranes. One of these marked discs is placed on the cornea (after applying cocaine), where it remains by cohesion without injuring the eye at all. If we now photograph the eye and the frame together we can measure all displacements of the eyes as easily as in animals (fig. 15).

If we want to investigate the rotatory compensatory eye positions, it is best to examine them in the normal and in the two lateral positions of the head in space; in all these positions the symmetrical position of the head in relation to the trunk must not be changed at all, to exclude tonic neck reflexes on the eye muscles.

To summarize: We have seen that the different tonic labyrinthine reflexes do not play a great physiological role separately; they do so in combination with other reflexes: Tonic labyrinthine reflexes on the body

musculature combined with tonic neck reflexes are of great importance in the different postures of normal animals. Labyrinthine righting reflexes combined with various other groups of righting reflexes cause the animal to bring back its head, trunk and limbs from any abnormal position into the normal again.

Tonic labyrinthine reflexes, combined with tonic neck reflexes affecting the eye muscles, and combined with reflexes arising in the semicircular canals, make it possible for the visual field to remain unchanged when the animal moves its head from the normal into some other position in space.

There is nothing new under the sun. In 1875 Breuer gave us the classification of the labyrinthine reflexes in two special groups and at the same time he said: "If you throw a frog, after bilateral labyrinth extirpation, into the water this animal will be absolutely disorientated, because the labyrinths, the sensibility of the body walls, and the eyes do not function at all." Thus be expressed in a few words exactly what I have told you to-day in many words.

#### DISCUSSION.

Sir JAMES DUNDAS-GRANT.

Very much of what we have heard is new to me and Professor de Kleijn has raised so many points that it would have been most difficult to grasp them had it not been for the admirable way in which Mr. Tweedie has already presented much of the matter before us. As practical people we look for the application of these reflexes in our ordinary work. We have a good deal of knowledge of the dynamic reflex produced in the semicircular canals, but the complications arising from the other static reflexes are still unfamiliar to us. The question asked by many of us is, "why is it that after the compensatory movement we have the return movement?": that is to say, why do we get the "jerk" of the nystagmus after the "slow drag"? I have read with interest the description of the experiments in which Professor de Kleijn cut through all the motor nerves of both eyes except the left sixth and detached the left external rectus from the eyeball, connecting it with a lever so that a tracing of its movements could be obtained. He removed the cerebral hemisphere and the brain-stem down to the anterior corpora quadrigemina; when he applied cold to the opposite ear he obtained the slow drag on the part of the left external rectus followed by the jerk. This, I understand, was supposed to test whether the jerk of the return movement is of cerebral origin: I confess that I cannot see how the experiment exactly proves it.

There have been some further experiments made on the brain in which the bone has been removed from the frontal region and caloric stimuli applied there; the forthcoming results have been very much the same as those obtained by stimulating the labyrinth.

While expressing my great admiration of Professor de Kleijn's description of his operations, I would point out that he has not referred to Ewald, whose labours are not forgotten, and from whom we have derived much illuminating information. I remember when Breuer's experiments were published, and it may perhaps amuse you to hear how they could be misinterpreted in translation. In a translation he is stated to have made his experiments on the "deaf," whereas in reality it was upon "*pigeons*," but I might remind you that the German word for "*pigeons*" is also the word for "deaf persons."

Mr. SYDNEY SCOTT.

Referring to yesterday's papers, I do not think we need any better hypothesis than that of Dr. Hartridge, which he clearly announced yesterday.

Can Dr. Walshe, in connexion with his observations, yet tell us whether

there is an alteration in Babinski's reaction in hemidecerebrates, when the positions of head and trunk were altered in space without flexing the neck? (I understand Dr. Walshe is already engaged in elucidating that point.)

In regard to the demonstration to-day, we must all greatly admire Dr. de Kleijn's brilliant piece of work and investigation. To be able to destroy the otolithic membrane by centrifugalization, without damaging the semicircular canals, enables the observers to isolate the functions of the saccule and utricle from those of the semicircular canals. He has shown that the changed reflexes after centrifugalization are really due to injury to the particular parts of the labyrinth, and not, as some might possibly suppose, to injuries to brain tissue, or to the labyrinth as a whole. I understand from Mr. Tweedie that the laboratories at Utrecht contain thousands of sections of labyrinths of animals that have been rotated before and after all these observations on tonic reflexes were made. This represents an enormous monumental work. As Dr. Gray says, "the human labyrinth is virtually a closed cavity," for we find that pressure-changes are produced in the labyrinthine cavity as a whole, which can be estimated manometrically by inserting glass capillary tubes filled with coloured fluid. The manometric readings show that the extreme mobility of the stapes causes a range of movement equal to quite 3 or 4 c.mm. (in a tube of the calibre of the superior semicircular bony canal). Unilateral obstruction in the Eustachian tube of an intact tympanum with a mobile stapes can lead to appreciable differences of the intra-labyrinthine pressure, comparing one side with the other. These facts have a bearing on the elucidation of certain forms of vertigo. In that class of vertigo now under consideration, all the canals of the labyrinth react normally: the tympanum is intact: hearing for low tones is preserved: one Eustachian tube is inefficient. The vertigo coincides with this inefficiency.

Dr. de Kleijn will no doubt allow me to draw attention to a case that was under my observation some five years ago, of a young man who had an extremely obstinate obstruction in the Eustachian tube of the left ear. I saw him every day in hospital in France, but it was about a week before I succeeded in inflating that very obstinate ear. At last when the air went in, he was seized with vertigo and nystagmus to the left. His head became inclined to the right, and the face was turned to the right. I kept the catheter in place and waited till the vertigo ceased, then gave one more blow, and to my astonishment the vertigo returned more violently, the head moved in the *opposite* direction, and he showed such intense nystagmus to the right that I felt alarmed. However, the attack stopped in a moment, and everything untoward disappeared. Now that was not only one single observation, but I had opportunities of carrying out these tests every day for about three weeks. When the drum membrane was invaginated from external atmospheric compression, and suddenly released, the head movement was always in one direction; when the tympanum was distended, the head moved in the *opposite* direction. F. S. Lee's investigations on elasmobranchs showed that he obtained eye-movements in one direction when he pressed *gently* on the ampullary nerve, and when he pressed harder the eye movements were in the *opposite* direction. There seems to be a parallel with Lee's observations and clinical findings, in connexion with the differences in pressure which are transmitted to the normal labyrinth from the tympanum.

I should like to raise the question of the influence of retinal reflexes on the tonic reflexes, which I believe have been erroneously attributed to labyrinthine stimuli, though we are acquainted with the forced movements of individuals

who have a prism placed in front of the eye. During investigations on flying I observed that slow rotation, as in a slow spin of an aeroplane, can set up forced movements of the limbs. Many experienced airmen had to close their eyes or avoid looking at the ground, if in a spin, to avoid these forced movements. It is a most important matter in connexion with flying but one to which little attention seems to have been paid.

Dr. ALBERT A. GRAY.

The reason for reserve in making definite statements as to the relationship of the different structures in the vestibule is that it is very difficult to be sure of one's facts. This is because for the most part the membranous structures are floating in a fluid which bathes them on both sides. Consequently when the preparations are put into alcohol distortion is almost certain to occur, with the result that incorrect judgments are formed in regard to the anatomy of the organ. An example of such an error is that made by Frau Anna Kraut in stating that the canalis reuniens is a closed tube in the adult human subject. As a matter of actual fact the ductus cochlearis in the adult human subject can be injected from the saccus endolymphaticus, if the injection be made before the preparation has been subjected to the influence of hardening and dehydrating agents.

Referring now to the paper which we have just heard from Dr. de Kleijn, I would like to pay the highest tribute of admiration to the work which has been carried out by himself and his colleagues at Utrecht. This work opens up a whole new field not only for the otologist, but for the physiologist and the physician as well. It explains many of the curious symptoms which occur in diseases of the labyrinth and also in those which affect the cerebellum. In addition it will enable us to make more accurately the diagnosis of diseases affecting these parts.

Mr. ALEXANDER R. TWEEDIE.

It is most gratifying to me to find my friend Dr. de Kleijn before the Section, giving us a first-hand demonstration of the work being conducted by Professor Magnus and himself in Utrecht on the otolithic and allied reflexes, an account of which I endeavoured to give you—very imperfectly I am afraid—on my return from Utrecht in the autumn of 1921.<sup>1</sup>

Dr. de Kleijn would have better been seen in his own laboratory, because it is impossible under these circumstances to realize one hundredth part of all their fascinating investigations thus separated from the environment in which they are carried out.

I would like to take this opportunity of expressing before Dr. de Kleijn my very keen appreciation and gratitude for the warm welcome I received from everyone on my visit to Utrecht. Everyone seemed most anxious to give me every possible opportunity of seeing and learning all that was being done, and Dr. de Kleijn particularly spared himself no time or trouble in showing me all he possibly could, so that I should have ample demonstration and instruction in the various steps on which their experimental work has been based.

Referring to Dr. Walshe's communication, I would point out that the particular aspect of our specialty, which we are discussing to-day is so inseparably associated with the nervous system, that it is not only important for us to

<sup>1</sup> *Proceedings*, 1921-22, xv (Sect. Otol.), pp. 15, 19.

receive occasionally the views of neurologists on these matters, but also that it is only by very close and constant collaboration with them that we shall be able to progress in our knowledge of this subject.

There is, as you know, already established in France the "Société d'Oto-Neuro-Oculistique de Strasbourg," which is only concerned with phenomena of interest to these particular specialties. So if we wish to keep abreast of developments in this direction we must follow the example of the French and seize every opportunity we can of obtaining assistance by collaboration with the oculist and neurologist.

I was also most interested in hearing suggestions made by Dr. Hartridge yesterday, as to the function of the labyrinth, and in finding that they were so accurately in accord with the theories of those conducting this work in Holland, theories which I attempted to indicate in the paper I read before the Section in March, 1922.

The labyrinth appears to be essentially an organ for presiding over balance and progression, and further, according to one theory, it is primarily stimulated in fishes by sensations received by way of the "lateral line." I believe that one day we shall find the missing links between the association of the "lateral line" with the labyrinth and the development of the cochlea. I shall be very pleased to give Dr. Hartridge some Dutch articles referring to this particular point.

Of course, however, the main point at issue, as has been mentioned, is the clinical significance of all this work. Accounts of several cases, in which these phenomena could be demonstrated, have already been published by Professor Magnus and Dr. de Kleijn; to these I referred in my original report to the Society<sup>1</sup>; and though perhaps it is yet too early to dogmatize on their clinical value, I am pleased to think I have already had cases of intracranial lesions in which certain of these phenomena could be demonstrated, and I have also had other patients with vertigo and with raised blood-pressure, probably hypersensitive cases, in whom nystagmus could be induced by an altered position of the head in space; but whose responses to the routine tests of the semicircular canals were normal.

I believe that we should regard the otolithic apparatus in man as essentially a degenerative organ, normally of extremely little importance, and only becoming evident, so to speak, when involved in pathological disturbances. This view I think has already been suggested by the descriptions Dr. Gray has given us of the labyrinth of various fishes, birds and animals, indicating its gradual decadence as we ascend the animal scale, and it is also supported by the fact that the otolithic reflexes are said to disappear in normal children after the age of some three months, and that they thereafter, as yet, can only be elicited in cases with intracranial lesions, and other pathological conditions. One reason for this gradual degeneration of the otolithic apparatus is, I submit, the higher development of other senses—alluded to, I think, by Dr. Hartridge—I mean that, as our sense of touch, sight and above all our volition become elaborated we learn to depend perhaps more on the greater efficiency of these senses, rather than be governed by an organ which automatically controls the movements of the lower animals. It is most interesting in this connexion to note the results of certain very practical experiments that have already been undertaken. I would remind you of the account by Mr. Sydney Scott of his personal investigations in an aeroplane (*Journal of Laryngology*, 1920, No. 8)

<sup>1</sup> *Proceedings*, 1921-22, xv (Sect. Otol.), p 24.

where he found that observers, if blindfolded, lost their sense of position in dense clouds. Similar experiments were also conducted in Holland by van Wulfften Palthe, who came to the conclusion that the vestibular organ is of no special significance to the aviator, and does not enable him to steer an aeroplane in clouds or mist (*Acta Oto-Laryngologica*, vol. 4). This attitude of "disregard" towards the perceptions received by way of the eighth nerve is not confined to its vestibular branch, as I would urge that our audition is also largely a matter of "disregarding" the many sounds which we have gradually learnt to ignore. Indeed it is only by these means that we are able to regulate what we hear in contradistinction to the methods we can adopt in respect of our other senses, where we have the opportunity, for example, of contracting our pupils, or even closing our eyes altogether against excessive light or objectionable sights, and in the case of our sense of smell, holding the nose to avoid offensive odours.

Dr. Gray raised the question of the old experiments of "hypnotizing" hens by laying them down, and drawing a chalk line from the beak, at a meeting of the Scottish Otological Society in Glasgow last March, where we discussed the otolithic reflexes. Since then Dr. Guthrie of Edinburgh has drawn my attention to a German article on the subject describing similar experiments with various animals ("Werkstatt des Lebens," Koelsch, Stuttgart). I see there that these experiments were already made as early as the seventeenth century, and amongst the various animals on which they were carried out, it was stated by this author that fish, if turned on their back, and undisturbed, could be induced to lie still in that position. Although rather incredulous I found to my surprise the other day that I could keep a goldfish in a bowl of water, turned on its back, with a very little manipulation. It seems to me quite reasonable therefore that these so-called instances of "hypnotizing" animals must really, be otolithic reflexes as suggested by Dr. Gray.

I hope that many others of the Section will be able to visit Holland, and realize for themselves the sure foundation on which this work of Dr. de Kleijn is based by the initial tests on the animals, the operative experiments, subsequent re-testing and final submission of the animals for histological examination in the Neurological Department under Professor Winkler.





## Section of Otology.

President—Mr. SYDNEY SCOTT, M.S.

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### Congenital Deafness in a Dog.

By J. S. FRASER, M.D.

THE conditions found in the present case of a congenitally deaf white bull-terrier pup are almost exactly the same as those shown before this Section in May, 1921, from a congenitally deaf boy.<sup>1</sup> Briefly, they are as follows :—

Middle ear and labyrinth capsule, utricle and canals normal; cochlear and vestibular nerves and ganglia normal; collapse of the cochlear duct and of the saccule; adhesion of Reissner's membrane to basilar membrane; Corti's organ replaced by an irregular arrangement of cells; colloid bodies in cochlear canal; membrana tectoria tucked into internal spiral sulcus; connective tissue in cochlear aqueduct.

Alexander has examined a deaf albinotic cat and found changes similar to those seen in the present case. Beyer has found similar changes in two albinotic cats and an albinotic dog. Alexander and Tandler have examined three dogs and twelve cats, with much the same results. They also examined four young cats, born of deaf mothers, and two of these showed changes typical of congenital deafness, the other two were normal.

Ruttin has examined an albinotic deaf dog and found the aqueduct of the cochlea filled with connective tissue—probably the remains of foetal connective tissue. In all these cases the changes are similar to those in Scheibe's type of deaf-mutism.

We cannot breed human beings to order, but we can breed animals. Is it not possible to get funds to start a research into congenital deafness in animals and its hereditary transmission, seeing that the changes are the same in human beings and in cats and dogs? We should then know if Dr. Kerr Love's views as to the application of Mendelian laws to sporadic congenital deafness were correct. Alexander has observed a pair of cats, of which the mother was deaf and the father normal—about half the kittens were deaf. The changes in cases of congenital deafness are due to faulty development and not to intra-uterine meningitis, as Siebenmann and Nager believe.

The conditions present in the case I have shown are not the same as those found in dancing or waltzing mice. It is true that dancing mice are quite deaf, but they also have a deficient power of balancing. They have a broad-based gait and run in zig-zag lines or turn round a vertical axis passing through the middle of the body. Dancing mice do not react to rotation like normal

<sup>1</sup> *Journ. Laryng. and Otol.*, 1922, xxxvii, p. 14.

mice, but do react normally to galvanism. In dancing mice there are changes in the neuro-epithelium, especially in the pars inferior (cochlea and saccule), but the main changes are in the nerve ganglia of Scarpa and Corti, and in the nerves. The changes in the peripheral sense organ are regarded as being secondary to those in the ganglia. The utricle and cristæ of the canals are normal in dancing mice, but the macula of the saccule shows atrophy. Gray has found the semicircular canals normal in dancing mice, and agrees with Alexander and Tandler as to the nerve changes.

There are several grades of congenital change in the inner ear which may produce deafness. The same conditions are seen in cases of cleft palate and hare-lip. In the slightest cases of this congenital abnormality we have a bifid uvula; in the worst cases the whole palate is cleft and a hare-lip is also present. In the same way, in the slightest cases of congenital deaf-mutism the maldevelopment affects only the ductus cochlearis; in the second grade the ductus cochlearis and the saccule are involved; in the third case the whole membranous labyrinth is affected, though the changes are more marked in the pars inferior than in the pars superior. This is in accordance with what we know of the comparative anatomy of the labyrinth, i.e., the much greater age of the static as compared with the acoustic labyrinth. The cochlear duct develops later than the vestibular portion of the membranous labyrinth. The epithelial lining of the cochlear duct is still undifferentiated at a period at which the maculæ and cristæ are already well formed (Keith). In still more marked cases of congenital maldevelopment the bony labyrinth may also be involved, especially that of the cochlea. The cochlea develops from base to apex, and therefore changes are more marked at the apex. Here the bony partition which should divide one scala from another may be absent, especially towards the apex of the cochlea, with the result that a "scala communis" is found. Finally, the whole labyrinth—bony and membranous—may be absent, as in Michel's case.

The following questions call for elucidation: At what stage of development do the changes set in? What is the cause of the faulty formation? Alexander holds that a deficiency in the vascular supply, i.e., the stria vascularis, is an important cause of the changes in the cochlea. Lastly, the relation of pigmentation, or rather "the want of pigmentation," to congenital deafness requires to be studied.

#### DISCUSSION.

Mr. SYDNEY SCOTT (President) said that in this valuable demonstration Mr. Fraser had clearly shown that lesions in the congenitally deaf dog and child were visible in the region of the stria vascularis, and that apparently the disease was limited to this part of the membranous labyrinth. The contraction of the duct of the cochlea was probably the result of the failure of secretion by the stria vascularis, which he regarded as analogous to the choroid plexus.

Dr. ALBERT GRAY said that he had never examined a deaf dog's cochlea, but he had been told by veterinary surgeons that deaf dogs were far more common than were deaf horses. He had examined sections from an albino cat, and they were very similar to those shown by Mr. Fraser that day. He considered that the lesion was confined to the ductus cochlearis. The organ of Corti was flattened, and the tectorial membrane was rolled over it. With regard to the wide aqueduct of the cochlea, he did not think that was pathological. There was a popular fallacy that the aqueduct of the cochlea was a narrow tube. It was narrow in the human subject, but wide in such animals as the carnivora and others.

With regard to the relationship of these cases of deafness to inheritance, it was

desirable to try to correlate inherited troubles with Mendel's law. But with complex structures, such as the ear, it was a difficult matter.

Mr. F. T. G. HOBDAY said that congenital deafness in bull-terriers was well known to veterinarians. They had no remedy for it. Quite expensive dogs were bought without any suspicion that they might be deaf. To test this point in a dog it was the custom to drop a book or other heavy article unexpectedly behind the dog, and notice whether any response followed. This condition was especially frequent in white cats and dogs, particularly in the bull-terrier breed and the white Persian cat; also in white rabbits, Angoras especially. He hoped Mr. Fraser would continue his investigations; it was another instance which illustrated the importance of the study of the comparative aspect of disease, and the hope to be derived from a mutual study.

Dr. DAN MCKENZIE said the question of the pigmentation in sense organs was of peculiar interest and mystery. It was very fully discussed by William Ogle in the *Medico-Chirurgical Transactions* for 1870.<sup>1</sup> Little knowledge on the subject had been added since that.

Mr. J. S. FRASER (in reply) said that Siebenmann and Nager had stated that these cases of sporadic congenital deafness were due to foetal meningitis. He (the speaker) did not see how intra-uterine meningitis could account for these conditions, as the otic vesicle developed from the surface, and was only met by the nerves coming from the brain side. Why the meningitis should be confined to the cochlea and saccule, leaving out the other parts, was difficult to explain. With regard to the connective tissue in the scale of the cochlea, Ruttin regarded it as the remains of foetal tissue, and probably he was right. In the early stage of the foetus, the perilymph space was full of foetal connective tissue, but normally it disappeared from the scala vestibuli and the scala tympani.

In answer to Mr. Hobday, Mr. Fraser stated that in Edinburgh there was a department which was going into this question of heredity, and a day or two before coming to London he had a letter from the head of this department, Dr. Crew, who had asked him to take part in a joint investigation. He hoped, therefore, on a future occasion, he would have some more material to bring before the Section.

### Otoliths of Fishes.

By G. ALLAN FROST (Introduced by Mr. SYDNEY SCOTT, President).

THERE are many types of otoliths, and it is possible to follow the course of their development from the simpler forms to the more highly specialized.

Aberrant forms occur in some species, and in other cases there is an affinity of the otoliths in genera not closely related; this may be due to parallelism or to reversion to an earlier type.

In Teleostean fishes each labyrinth contains three otoliths, the *sagitta*, the *asteriscus*, and the *lapillus*, one always being more fully developed than the others.

Where the *sagitta* is the most developed, a groove on the inner side, the *sulcus acusticus*, is an important feature, the form it takes enabling us to identify the otoliths of living and fossil fishes.

[A comparison of the sulcus in the various groups followed, and was illustrated by diagrams and specimens shown on the screen.]

<sup>1</sup> "Anosmia; or, cases illustrating the Physiology and Pathology of the Sense of Smell," *Med. Chir. Trans.*, 1870, liii, pp. 263-290.

## DISCUSSION.

Mr. SYDNEY SCOTT (President) in the name of the Section, thanked Mr. Allan Frost for the interesting demonstration of his beautiful and wonderful collection. He (Mr. Scott) thought that Mr. Frost's work would stimulate further research into the anatomy and functions of the otoliths in connexion with the various tonic reflexes which were now receiving attention.

Mr. H. TILLEY said that Mr. Frost had remarked that one of the functions of the otoliths was in connexion with hearing. As a fisherman, he (Mr. Tilley) had been greatly impressed by the acuteness of sight of fish, but he did not feel convinced that fish were able to hear. Any noise on the bank—apart from vibration communicated to the water—left fish undisturbed, but immediately the water was struck with a "line" or other object there was a commotion among its inhabitants.

Mr. J. F. O'MALLEY said he was interested in the subject from the standpoint of the evolution of the labyrinth; several writers, especially Sir Arthur Keith, said that most observers who had written on the subject of the labyrinth believed that fish did not hear.

Dr. ALBERT GRAY said that if one opened the labyrinth of many fishes one found a large otolith, and the fluid which came out, i.e., the endolymph, was sticky, almost like gelatine. He asked whether Mr. Frost could say what was the consistency of the endolymph in the fishes he dealt with. He (the speaker) wondered whether the gelatinous envelope surrounding the otoconia of mammals was not morphologically a remnant of this glycerine-like substance, which spread through the whole labyrinth in many fishes.

Mr. A. TWEEDIE said he presumed that the "sulcus" to which Mr. Frost referred was only of functional importance in reference to its representation of the underlying neuro-epithelium. He (Mr. Tweedie) asked what was the comparative size of the various otoliths in a fish, and in different species of fishes. One would expect from the recent experimental work carried out in Utrecht that the sagitta would be the largest, the lapillus the next in size, and the asteriscus would be the smallest. From the same experiments in Holland one would be led to believe that the sagittæ (i.e., the *sacculi*) had a function which presided over, so to speak, the rolling of the ship, the lapilli (i.e., the *utricle*) being concerned with its pitching, and the asterisci (i.e., the *lagenæ*) acting in some way as a depth indicator.

Could Mr. Frost say anything about the relation of the innervation of the lateral line to the nerves supplying the sensory epithelium in connexion with the otoliths? Was the lateral line supplied by the eighth nerve? Did Mr. Frost regard the lateral line as a receptive organ for vibratory sensation?

Sir JAMES DUNDAS-GRANT said that the late Sir George Savage had told him that in a certain piece of water the fish never rose on week-days, but they did so on Sundays, when the church bells rang and no fishermen were about. On Good Friday the church bells rang and the fish rose to Sir George's fly. He, therefore, assumed that the fish could hear.

Mr. G. J. JENKINS said that the real point in this discussion was, what was understood by "hearing?" Where did ordinary sensation end and where did hearing begin?

Mr. G. WILKINSON said that in some lower organisms such as jelly-fish, some of the primitive organs which were supposed to be for hearing contained small solid bodies, like otoliths. In the earliest forms of hearing-organs there must be some connexion between the presence of otoliths or a solid concretion, and the sense of hearing. In the case of creatures living in water, a solid body would act like the clapper of a bell, and the animal, with its hearing organ, would vibrate. It was extraordinary that the labyrinth and the hearing-organ should have been developed in connexion with one another. He thought there was something in the original connexion between the

otolith and the vibratory sense, which might explain the after-development of the static organ and the organ of hearing in conjunction with one another. There seemed to be no evidence that in the higher types of animal the otolith served any purpose in connexion with hearing.

Professor BURGER (Amsterdam) said he felt much indebted to Mr. Allan Frost for his beautiful preparations. They reminded him (the speaker) of a Dutch work published twenty years ago which contained skiagrams of the heads of fishes, and in which the author compared the otoliths in fishes with those in man. The author had observed certain movements of the heads of the fishes, but the attempt to secure photographs of these had not been very satisfactory, though the otoliths were quite clear. What Professor Hobday had said about the importance of comparative study was certainly applicable to the investigation of the otoliths.

Mr. SYDNEY SCOTT (President) said that Isaak Walton had discussed the subject of hearing in fishes.

Mr. ALLAN FROST (in reply) said his opinion had always been that fish could hear, but he felt diffident about saying so before that assembly. He agreed with Mr. Jenkins that the question was, where did hearing begin and ordinary perception of vibrations end? It had been said that the carp had been known to come for meals when a bell was rung. In the case of the beryx which lived at 850 fathoms, the large plate-like otolith must be very susceptible to vibrations. In answer to Dr. Gray, the endolymph seemed to vary in the different fish; sometimes it was very thin, in others its consistency was thicker than that of glycerine. As to the size of the otoliths, the sagitta was usually the largest, the asteriscus was small and frail, and the lapillus about the same size but stouter. With regard to the lateral organs, Professor Bridge had stated that there were independent nerve fibres, which returned from these organs to the tuber acusticum, in which the auditory nerve had its root.

### **Case Illustrating Digital Compression of the Vertebral Arteries for Pulsating Tinnitus.**

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, a woman, aged 51. The patient was referred to the ear department of the West End Hospital for Nervous Diseases on account of tinnitus aurium. It was found to be of a low-pitched pulsating character, and was diminished by compression of the vertebral arteries in the sub-occipital triangles, pressure being effected by the thumb and middle finger of the observer's left hand, while with the right, counter-pressure was exercised on the forehead. The menopause had taken place four years previously, and the patient had suffered from flushes of heat; her blood pressure was 150. She was relieved by the administration of bromide of potassium and magnesium sulphate. The tinnitus was assumed to be due to a neuro-vascular arterial congestion of the labyrinth.

#### **DISCUSSION.**

Mr. SYDNEY SCOTT (President) said that he always doubted whether the lumen of the vertebral artery could really be occluded by compression in the suboccipital region.

Mr. J. S. FRASER said that most cases of tinnitus were due to neuritis of the cochlear nerve or to an affection of the cochlear ganglion cells.

Dr. P. WATSON-WILLIAMS said that it was easy to exaggerate the value of this test; it was also easy to fail to appreciate its usefulness. He doubted, however, whether the vertebral arteries were really compressed, but in some cases of central tinnitus the

### 34 Dundas-Grant: *Tinnitus*; Cheatle: *Lupus of the Lobule*

method of Sir James seemed to stop the tinnitus. When desiring to know whether a tinnitus was of middle ear origin, one compressed the corresponding common carotid, and often the tinnitus was stopped for the time being if it was a middle ear affection, but was unaffected or even increased if it was not a middle ear tinnitus.

Sir JAMES DUNDAS-GRANT (in reply) said that in 1887, in order to make sure that pressure really acted on the vertebral artery, he had practised it on the cadaver. He exposed the artery in the lower part of the neck, fixed a syringe in it and injected water. The outflow of this water through the cut end of the basilar artery completely stopped when pressure in the way now described was exerted. It was not likely, however, that, in the living patient, the lumen was so completely obliterated as to cause any danger. When this pressure checked the tinnitus, it showed that the condition causing it was congestive, as in many climacteric cases. Bromides in some form were likely to be useful. The pressure might be useful in the diagnosis of aneurysm of a vertebral artery.

In reply to Mr. Somerville Hastings, he suggested that the congestion was in the region most directly supplied by the internal auditory artery, a branch of the basilar, which was formed by the junction of the two vertebrals. In cases of congestive middle-ear disease one expected to diminish the tinnitus by compressing the carotid artery. In cases of tinnitus, he tested the vertebrals first, then the carotids; the former had no large companion veins associated with them, so that the result was more clear-cut than in the case of the carotids.

The patient was not brought forward for the discussion of tinnitus, but for the demonstration of this piece of "applied anatomy."

#### Case of Lupus of the Lobule.

By ARTHUR H. CHEATLE, C.B.E., F.R.C.S.

PATIENT, a woman, aged 34, came to King's College Hospital with a pear-shaped tumour, which had been forming for two years, hanging from the lobule of



FIG. 1.

the right ear; it was dusky in colour and felt soft; there was no ulceration and the skin was smooth. Behind the auricle and just above the attachment of the tumour was a small apple-jelly spot. No sign of lupus or tubercle else-

where. No history of injury. The tumour and nodule were removed in one piece and the skin sutured. Dr. Creed reported that the section showed nodules of granulation tissue with a few giant cells. Politzer refers to a similar case reported by Neisser.

Mr. J. S. FRASER said that in a similar case of his own the patient had worn earrings, but the present patient had never done so, and she attributed the trouble to a warty growth, which she scratched and infected.

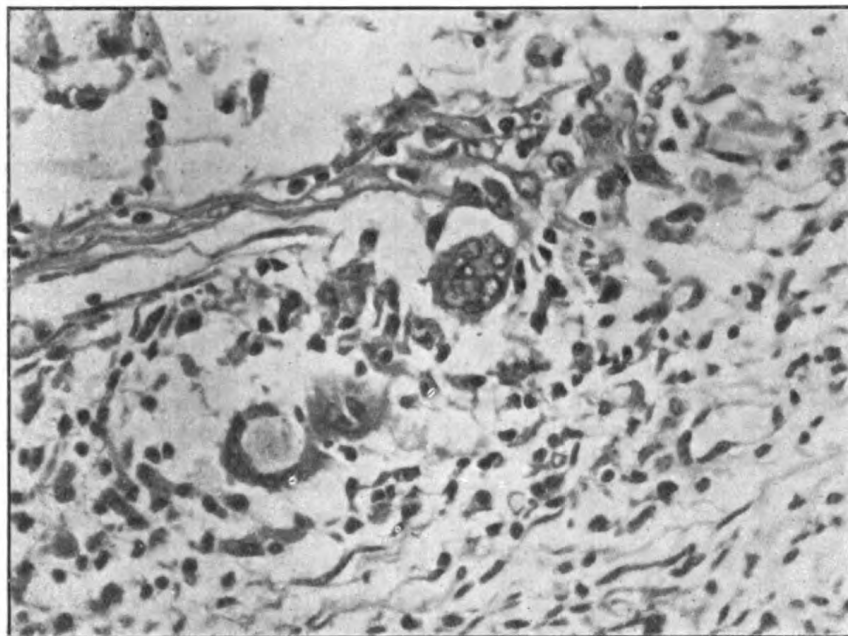


FIG. 2.—Photomicrograph of lupus of the lobule.

### **Case of Meningitis which recovered without Trans-labyrinthine Drainage.**

By T. B. LAYTON, D.S.O., M.S.

C. C., MALE, now aged 19, was admitted on Tuesday, December 6, 1921. On the previous Friday, December 2, he had been seen by Dr. Wallis Kemp, of Kingston, apparently suffering from a very acute attack of influenza. There was nothing to draw Dr. Kemp's attention to the ears until Monday, the 5th, when he noticed that there was some strabismus and then discovered discharge from the ears. He advised immediate removal to the hospital, but owing to inability on the part of the father to obtain transport this was not effected until 11 a.m. the next day, December 6. The boy stated that he had only suffered from his ears since Sunday, December 4, but a history was obtained from the parents of chronic ear disease with discharge for years.

On admission: there was severe pain over the mastoid region with well-marked œdema, and there was frequent vomiting with severe headache. When



seen in the surgery by me he looked so desperately ill that I doubted whether he would survive an operation. He was taken into the operating theatre immediately and therefore no detailed functional examination was made. The following description of the operation is copied from the report by the dresser—Dr. A. M. Deif:—

"Pus was seen to exude even from the first removal of bone. Diseased bone was taken away by the rongeur forceps in a wide area over the posterior cranial fossa, exposing a stinky smelling, yellowish, evidently sloughing dura. The dura affected was that overlying the lateral sinus and extending posteriorly to some distance over the posterior cerebral hemisphere and downwards over part of the cerebellar hemisphere. The sinus was empty under the slough, but the removal of part of the slough caused the sinus to bleed and it was then packed. The wound was packed and its edges left widely open.

"Lumbar puncture was performed.

"The fluid was under increased pressure and turbid. Microscopically, pus cells were numerous. Albumin increased. Culture revealed no organisms after 72 hours."

The next evening the patient shivered and the jugular vein was tied in the neck by a transverse incision. Mr. E. J. Crisp, the house surgeon, performed lumbar puncture for eleven days in succession after the operation.

The patient left hospital on January 13 with the ear still discharging.

Cultivations were made from the cerebro-spinal fluid on December 8 and on five subsequent occasions, and they remained sterile, except on one occasion when a growth of a Gram-negative bacillus and a Gram-positive coccus was believed to be a contamination.

It is regretted that a quantitative microscopical and chemical examination was not made of the original specimen; subsequent reports are as follows:—

| Date              |     | Cells                                                         | Protein<br>Per cent. | Sugar     |
|-------------------|-----|---------------------------------------------------------------|----------------------|-----------|
| December 12, 1921 | ... | Many pus cells in blood ...                                   | 0.06                 | Absent    |
| " 13, 1921        | ... | 2,000 per cub. mm. mainly polymorphonuclear leucocytes        | 0.06                 | In excess |
| " 14, 1921        | ... | 46 per cub. mm. ...                                           | 0.06                 | In excess |
| " 16, 1921        | ... | 15 per cub. mm. ...                                           | 0.08                 | Normal    |
| " 18, 1921        | ... | Much blood, white cells slightly in excess of red-white ratio | 0.045                | Normal    |
| " 19, 1921        | ... | Much blood present, white cells in slight excess              | 0.04                 | Normal    |

#### DISCUSSION.

Dr. P. WATSON-WILLIAMS asked whether there was any labyrinthine involvement in this case as the report afforded no evidence on this point. He had brought before the Section a case of meningitis in which there was definite labyrinthitis. He opened the external semicircular canal, and, in addition to repeated lumbar punctures, he carried out intrathecal and intravenous colloidal silver injections with success. In the present case he thought the meningitis was relatively circumscribed, and therefore he did not know why translabyrinthine drainage was considered at all, as there was a free opening for drainage by exposure of the portion of meninges which seemed to be involved, also the opportunity of repeated lumbar puncture.

Mr. G. J. JENKINS said he thought Mr. Layton should be asked to show the case after a complete examination had been made as it was important to know whether the patient had had labyrinth trouble. He (the speaker) did not think lumbar puncture was helpful in treatment, except for the fact that it relieved intracranial tension. He had now a case in which the patient was recovering three weeks after the operation. The cerebro-spinal fluid showed a cell count of 1,500—polymorphs were

95 per cent.—and a full amount of sugar, the protein slightly increased, and the chlorides normal. This patient had only this one diagnostic lumbar puncture. He regarded his case as a *méningite de voisinage* and therefore did not drain the subarachnoid space.

Mr. SYDNEY SCOTT (President) said he did not think the title of this case was a happy one, but presumed Mr. Layton chose it to raise a discussion on trans-labyrinthine drainage. He (the President) would not advocate a translabyrinthine drainage except for meningitic infection secondary to that of the labyrinth, and Mr. Layton did not say whether the labyrinth in his case was infected. Mr. Tweedie had pointed out, in the next room, one sign which suggested something was wrong with at least one portion of the boy's right labyrinth. He hoped Mr. Layton would be willing to give a further report of a complete examination at a later date. It was not uncommon for patients to have acute mastoid infections with pus under great tension, followed by an effusion into the cerebro-spinal system, without its passing through the labyrinth. He agreed with Mr. Layton's treatment, and considered that lumbar puncture had effected the drainage of the meninges. Probably all members had been faced with the problem as to whether the labyrinth ought to be fully investigated clinically; and what Mr. Layton did was what many members would have done, viz., to open the mastoid, and drain it, then do lumbar puncture. It was easy, within the next twenty-four hours, to apply, say the caloric test, if this had not been done before; and by the result one would be guided whether to proceed at once to drain the labyrinth, and open the internal auditory meatus.

### Radical Mastoid Operation without Grafting or Meatal Plastic (Bárány).

By DAN MCKENZIE, M.D.

IN this method the radical mastoid operation is performed on the bone in the usual manner, but the membranous meatus is not interfered with. The operation is rather more difficult by reason of the meatal and tympanic landmarks not being exposed. This patient, however, shows that complete healing is possible without contraction of the meatus.

#### DISCUSSION.

Sir JAMES DUNDAS-GRANT said he hoped to show a case on which one of the earliest mastoid operations in London was done, and the result was excellent, though no plastic operation was performed. Also another patient, who had had a temporo-sphenoidal abscess, in which he made no attempt to do a "plastic," and now there seemed to be a result almost more perfect than surgery would have achieved. He thought there was perhaps too much keenness about doing a "plastic," and that if there was not too great an area of bone removed, plugging the membranous meatus sufficed, Nature being relied on to draw the soft parts closer to the bony walls, and to dilate the membranous tube.

Mr. L. COLLEDGE said he regarded this proposal as retrograde, as in such cases débris collected behind the meatus and discharge continued. Six weeks ago a woman was admitted into St. George's Hospital as an emergency case, who, fifteen years previously, had been operated upon for sinus thrombosis. No flap had been cut. She now came with a cerebral abscess, from which she recovered. But if a proper plastic had been done at the time, she would almost certainly not have had this complication fifteen years later. He (Mr. Colledge) now had another patient in the hospital who had been operated upon by a general surgeon in the summer. A good bone operation had been done, but no plastic. She had been sent to him (the speaker) because she had facial palsy, and it was suggested that a nerve anastomosis should be done. She still had a

## 38 McKenzie: *Mastoid Operation; Disease of the Middle Ear*

foul discharge. He reopened the wound, and found a good radical cavity; she simply needed a proper plastic operation to render the ear dry.

Mr. H. TILLEY said that he agreed with Mr. Colledge. When a case came for treatment, one should base the treatment on the best one could do for 95 per cent. of the cases, not upon what might happen in 5 per cent.

Dr. P. WATSON-WILLIAMS asked what was the reason for the modified mastoid operation being selected in this case. When the hearing of the ear to be operated on was useful, and particularly when the other ear also was diseased and deaf, and therefore when the patient had to depend on the ear for which operation was necessary, it was an advantage to run the risk of doing an operation involving a slow recovery, because by not interfering with the tympanic wall, one might retain a degree of hearing which was useful, but which a more complete operation would cause to be lost. Often a graft did not cover the inner tympanic wall which had been left uncuretted, but at all events, there seemed to be no reason why one should not graft on to the operated area and leave the inner tympanic wall uncovered. He (the speaker) had often preserved very useful hearing in this way, although the operation was a departure from the ideal and required more time in recovery.

Mr. SYDNEY SCOTT (President) said he did not suppose Dr. McKenzie suggested that this modification should take the place of the ordinary procedure as a routine practice. Much depended on the anatomical varieties of the particular mastoid being dealt with.

Mr. G. J. JENKINS said his observations were not yet complete. He was not doing the operation described by Bárány; he made a big periosteal flap, removed all the diseased material in the region of the antrum, and took away the incus from behind. He then put the flap deep into the region of the antrum and aditus; the lower part of the cavity filled with blood clot. In suitable cases the result was perfect. The difficulty was in deciding which class of case was suitable for the procedure.

Mr. J. S. FRASER said that Dr. William Haskins, of New York, had been doing a similar operation to that described by Dr. Dan McKenzie. The bony operation was completed as usual, for the radical mastoid operation, and no meatal flap was cut. The cavity was packed from behind for a few days. Later it was allowed to fill up with blood clot and granulations, and the wound closed. The meatus was packed, and the result was what Mr. Cheate had just mentioned; there was no big cavity left liable to become filled with wax and epithelium and some pus. He (Mr. Fraser) however had had no personal experience of this new type of radical operation.

Dr. DAN MCKENZIE (in reply) said that his reason for exhibiting this case was to show that it did not matter very much what one did to the meatus in the radical mastoid operation. If one was fortunate in getting sepsis removed, there was a good result. He (the speaker) had tried many methods. No matter what method suggested itself, however, one found that somebody else had tried it before.

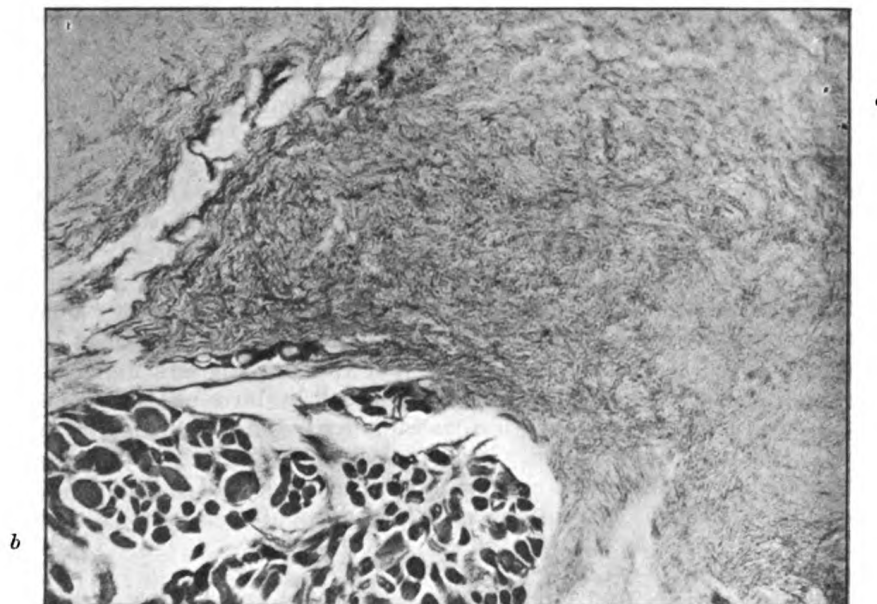
### **Simulation of Malignant Disease of the Middle Ear by Cholesteatoma with Fibromatous Changes.**

By DAN MCKENZIE, M.D.

THE patient is a woman aged 60. The symptoms consisted of an old-standing discharge from the right ear with complete facial paralysis of recent appearance. The external meatus was infiltrated and occupied by vascular granulations which prevented a view of the tympanic region. In addition, there was present, arching over the attachment of the pinna in the temporal region, a hard semicircular mass just under the skin and extending down in front of the ear as far as the tragus.

These appearances left no doubt in my mind as to the nature of the disease, and the parts were opened up by a large incision so devised as to include the auricle in a large flap hinged forward like a door.

The hard mass was removed entirely. It extended from a cholesteatomatous cavity in the mastoid process round the temporal region as far as the pre-auricular gland. In the mastoid process itself old cholesteatoma and granulations were found, but no signs of any neoplasm. The meatal walls were ulcerated and ragged. The facial nerve was not encountered.



Excessive overgrowth of fibrous tissue adjacent to old cholesteatoma of middle ear. (a) Fibrous tissue. (b) Muscle fibres (of temporal muscle).  
(Photo: Dr. A. Renshaw).

Portions from all parts of the field of operation were examined by Dr. A. Renshaw and Dr. Powell White, of Manchester, and their report is that nowhere was there any indication of malignancy discoverable. The fibrous mass contained fibromatous material and gland tissue.

Sir JAMES DUNDAS-GRANT said he wondered whether this was not a case of genuine cholesteatoma, as distinguished from the post-suppurative cholesteatoma so often met with.

## Two Cases of Chronic Suppurative Otitis Media, treated by Ionization.

By A. R. FRIEL, M.D.

(I) P. F., MALE, aged 16. History: Aural discharge for many years, which persisted in spite of treatment by drops.

When patient was first seen, there were granulations on the posterior wall of the right ear, at the junction of the tympanum and the meatus. On aspiration with Siegle's speculum foetid pus appeared here.

The essential points in the treatment of this case have been as follows: Destruction of the granulations by zinc electrolysis and nitrate of silver followed by zinc ionization of the cavity from which the pus was aspirated.

In order to distribute the electric current to the interior of the cell in sufficient amount the following device was used. A few inches of fine silk-covered copper wire were taken, doubled, and twisted. The twisted portion, slightly over an inch long, was fastened to a light handle. It was then, together with the lower part of the handle, dipped in a warm solution of a zinc salt thickened with gelatine and then hung vertically, so that a small blob of gelatine collected at the tip. When dry, the twisted wire, except the very tip, was painted with shellac varnish. When the patient was ionized, one of the free ends of the wire was attached to the positive terminal of the rheostat, while the end of the twisted portion was insinuated into the narrow opening of the cell after it had been filled with a few drops of zinc sulphate solution. The silk and gelatine prevented a short circuit on to the tissues, and the shellac varnish prevented the current from being dissipated in the meatus and tympanum.

The discharge has ceased.

(II) F. S., male, aged 23. The history is one of aural discharge from the left ear for over four years.

On examination the condition was found to be attico-mastoid disease with foetid discharge and cholesteatoma, also polypus and granulations in the tympanum.

Treatment has been as follows: Portion of the outer wall of the attic was destroyed by electrolysis, the remains of malleus and incus removed, and the mastoid antrum treated with zinc ionization. The disease within the tympanum has been treated with zinc electrolysis and nitrate of silver, zinc ionization and boracic powder.

The ear has remained dry for several months.

### DISCUSSION.

Dr. W. HILL said that many people were sceptical as to the value of this method of Leduc's. In the selected cases which he (the speaker) had sent to Dr. Friel, there had been only one failure; in two there had been granulations. Dr. Friel did electrolysis first, ionization afterwards. Success depended on a very careful technique. At first he (Dr. Hill) was sceptical about it, but that was not the case now. In cases with granulations, the discharge had all stopped—a very important matter, especially in children. He had sent to Dr. Friel cases of recurrent furunculosis of the meatus, and he had stopped the further formation of these tumours.

Dr. T. B. JOBSON said he thought the destruction of the outer attic wall by electrolysis was a new and original idea, and it might lead to a great advance in the treatment of these attic cases. If this treatment were generally carried out for such cases he thought it might cure a good many without the hearing being sacrificed.

Mr. SOMERVILLE HASTINGS asked how often Dr. Friel had destroyed the outer attic wall by electrolysis. It seemed a dangerous process to remove bone by causing its necrosis; he (the speaker) considered the area too near the meninges to be safe for removal by necrosis. He would like to know how many times the exhibitor had removed a sequestrum by electrolysis.

Dr. FRIEL replied that he had carried out this proceeding seven or eight times, mostly on children, in ordinary out-patient work. He had adopted local anæsthesia, injecting novocaine into the posterior wall, at the junction of the cartilaginous and bony parts. He used one or two dry cells to furnish current to the apparatus.

### **Case of Cerebral Abscess as a sequel to Acute Otitis Media, which had completely healed. Operation. Recovery.**

By E. H. RICHARDS, M.B., B.S.

(Introduced by Mr. W. M. MOLLISON).

BOY, aged 16. Acute otitis media (left). May, 1923: paracentesis; recovery with normal hearing, followed by headaches over left side.

Eight weeks after the otitis, admitted to hospital on account of severe headaches, vomiting, irritability, optic neuritis, diplopia and sensory aphasia. The boy showed some weakness of the left external rectus, bilateral papilloedema, and some spontaneous nystagmus to either side. His hearing was normal, and both labyrinths reacted normally.

From the presence of sensory aphasia the diagnosis of abscess was made and an operation was performed. No sign of disease found in mastoid process or antrum; swabs showed the antrum to be sterile. Dura mater of middle fossa under some increased tension; exploration of temporo-sphenoidal lobe revealed an encapsulated abscess separated from the surface by  $\frac{1}{2}$  in. of apparently normal brain; the abscess contained 1 oz. or more of odourless pus, from which a pure culture of streptococci was grown. Drainage by means of a tracheotomy tube for twenty-four hours only. Uninterrupted recovery.

Mr. SYDNEY SCOTT (President) said that this case re-introduced the subject of the drainage of brain abscess as well as other interesting points, such as the development of the signs of brain abscess after the original infection had resolved. He (Mr. Scott) had seen such cases, in one of which an enormous brain abscess formed, filling the whole temporo-sphenoidal lobe, and extending back into the occipital lobe, the symptoms of which arose after the acute suppurative otitis media had completely disappeared without operation.

**Facial Paralysis Associated with Actinomycosis, and Simulating a Bezold Mastoid Abscess.**

By J. F. O'MALLEY, F.R.C.S.

W. D., MALE, aged 67. History: At the beginning of August, 1923, history of mosquito bite near right ear, followed by large swelling and inability to move his face.

August 15, 1923 (seen by Dr. Verbrugge): Temperature 101.5° F., which persisted for five days. Swelling prominent below ear and extending on to mastoid and parotid region.

August 21, 1923: Swelling incised, pus escaped and has continued to do so since.

November 1, 1923: On account of mastoid infection seen by exhibitor, who suggested actinomycosis. Slides shown with mycelium.

## Section of Otology.

President—Mr. SYDNEY SCOTT, M.S.

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### Three Cases of Injury to the Ear caused by Spirit Injection through the Face for Neuralgia; two to the Middle Ear and one to the Auditory Nerve.

By ARTHUR H. CHEATLE, F.R.C.S.

*Case I.*—Male, aged 71½. Severe deafness in and slight discharge from the left ear immediately following spirit injection under an anæsthetic, about one year previously. No pain, giddiness, or tinnitus. Temporary paralysis of the facial nerve. No history of former ear disease. Right ear normal. The neuralgia was cured and patient said that the ear trouble was a small price to pay for the cure of the pain.

The membrana tensa was entirely gone, exposing the pink lining membrane on the inner middle ear wall; ossicles intact; a small granulation was present on the floor of the tympanic cavity with slight purulent discharge. Conversational voice—2 in.; whisper—touch. Tuning-fork on vertex to the left. Bone conduction greater than air.

*Case II.*—Male, aged 72. Severe deafness, singing and slight discharge on the left side following spirit injection under an anæsthetic, about two years previously. No pain, giddiness, or facial paralysis. No history of former ear disease. Right ear normal. Neuralgia cured. The hearing, local conditions, and tuning fork tests were identical with those in the first case except that there were no granulations.

In these two cases we can only infer that spirit entered the Eustachian tube and reached the middle ear. Considering the close proximity of the tube to the oval foramen one can see how easily the accident can happen. The painlessness and the amount of the deafness are interesting features. Perhaps the accident might be avoided if the injection were made without a general anæsthetic, the patient then being able to give warning.

*Case III.*—Female, aged 22. Complete deafness on the left side following spirit injection under an anæsthetic nine months previously. Immediate deafness, giddiness, and paralysis of the sixth and seventh nerves. Very ill in bed for five months.

There had been chronic suppurative trouble in the left middle ear for some years, but nothing new locally was observed as a result of the injection. Neuralgia not cured. Subsequent operation on the fifth nerve cured the pain. The left eye was lost. Right ear normal.

The sixth and seventh nerves recovered and the giddiness ceased, but complete nerve deafness was found on examination.



In this case the spirit had apparently reached the subarachnoid space and injured the sixth, seventh, and eighth nerves directly.

In all three cases the injections were made by experts.

#### DISCUSSION.

Mr. T. B. LAYTON said that, tragic as were these cases described by Mr. Cheatle, they were slight in comparison with one he (the speaker) had recently seen—that of a middle-aged man whose left eye had gone, and who had corneal ulceration. He spoke in a high falsetto voice, and seemed quite deaf. The left side of his face was paralysed. This paralysis had occurred as a result of an injection through the mouth on the right side. He had had nine injections, six in Hong-Kong and the seventh in Montreal, for neuralgia of the mandibular nerve. The seventh injection was the only one under local anæsthesia. Pain was absent for three months only after each injection. His present condition was the result of the tenth injection: the facial paralysis had been noticed by a nurse before he returned to the ward. He (Mr. Layton) had seen the man several times, once with Dr. Watkin Thomas, and had tested him with the "cold" test. The left side as well as the right was syringed, but there was no response. There were a few slight twitches in the eye, but he (the speaker) did not think they amounted to nystagmus. At a later stage the patient was able to read print in large capitals with difficulty. He (Mr. Layton) had not seen the patient during the last ten months.

Dr. WATKIN THOMAS said that the patient in the case referred to could hear no notes below C<sup>2</sup>. After prolonged syringing there was what might be regarded as a slight nystagmus.

Mr. G. J. JENKINS suggested that these notes should be brought before the Section of Neurology, so that a correct estimate of the value of the procedure might be obtained. It might be worth while to take even such risks as those described in order to secure as many recoveries as possible from this distressing complaint. It was a question as to which was the more serious operation: this injection, or removal of the Gasserian ganglion. He (the speaker) had known two cases in which damage had been done by this injection. One of the patients, a female, aged 28, who had had a severe tic on the left side of the face, was now in hospital, having had the labyrinth on the injected side completely destroyed. This patient had had violent vertigo, vomiting and deafness immediately after the injection had been made, and a burning pain on the left side of the throat, which had persisted for forty-eight hours. It seemed that the alcohol had gone through the Eustachian tube to the middle ear. There had been some damage to the tympanic membrane, and a few days afterwards a thin watery discharge from the ear had occurred, and a radical mastoid operation had been performed. The patient had come to him (the speaker) because of the persistent discharge from the ear and the attacks of giddiness. When operating, he had found a little granulation on the anterior part of the semicircular canal, and this he had left alone. In time the cavity had healed and become dry. A year ago the patient had returned, with severe giddiness, and he (Mr. Jenkins) had found ulceration on the anterior aspect of the semicircular canal, which he had treated until three months ago, and again the cavity had become dry, but the giddiness had persisted. The internal semicircular canal had become functionless; the superior and posterior canals reacted normally. He had decided to destroy the labyrinth, and this had been done two weeks ago.

He (Mr. Jenkins) knew of another case, in which the patient had died in a short time from injection into the meninges.

Mr. SYDNEY SCOTT (President) said he had heard of, but had not seen, cases like those described by Mr. Cheatle, Mr. Jenkins and Mr. Layton. In the first of his cases, in which alcohol had been used to render the labyrinth defunct, there had been marked facial paralysis, and recovery had been very slow. With regard to the question as to

whether alcoholic injections were of use to allay pain in malignant disease, he (the President) had personally applied alcohol to the third division of the fifth nerve. Complete anæsthesia had been produced over the region supplied by this nerve, but pain had persisted in the occipital region. The injection of alcohol called for very careful technique to guard against the dangers which had been mentioned.

Sir WILLIAM MILLIGAN said he thought that this communication ought to be submitted to the Section of Ophthalmology also, as the Members of that Section could probably relate some experiences of bad results due to these injections. He (the speaker) had not had much personal experience of the procedure, but he knew of cases in which irreparable damage had been done by it. He regarded it as a serious proceeding, for two reasons; first, because he did not agree with the President that it was easy to hit off the sensory nerves, even when following the ordinary landmarks; and, secondly, because the effect was only temporary, and in some cases no relief had occurred. He (Sir William) had done the operation in a few cases; in some of these there had been relief for a period of from nine to eighteen months, in others there had been no relief at all. There was also danger to the eye itself from the injections.

He (Sir William) had been summoned to Wales to see a patient who had been tormented for two years. As she had undergone every form of ordinary treatment, and as she did not wish to submit to the severity of a Gasserian ganglion operation, he (the speaker) had been eventually persuaded to use an alcohol injection. The injection had been made under a general anæsthetic, and the spirit had been mixed with a small quantity of cocaine. The immediate effect had been fairly satisfactory, but within two months an acute inflammation had developed in the anterior chamber of the eye. This had gone on to suppuration, and the eye had become so disorganized that it had to be removed. Within a few weeks of the injection facial paralysis had developed, but that disappeared. The pain was now practically as bad as it had been before, and in addition she had lost one eye. It was essential to take all the dangers into consideration before recommending the procedure which sometimes succeeded but sometimes did not.

### Case of Diabetes in which a Mastoid Operation was performed under Local Anaesthesia.

By T. B. LAYTON, D.S.O., M.S.

THE patient, a middle-aged man, had a fluctuating swelling which extended down the neck parallel with the sternomastoid below the angle of the lower jaw. The skin was tense and there was œdema above and behind the pinna. Anæsthesia was produced by blocking the nerves which turn round the posterior margin of the sternomastoid muscle and by making injections behind and in front of the pinna. The skin incision caused him some pain, but after this there was not any. The bone work was done slowly, with a pause between the removal of each piece of bone. It did not cause him any particular discomfort. There was extensive suppuration in the bone behind the ear, and the whole of the mastoid process was removed and the lateral sinus exposed over a considerable area. The region of the middle ear and external auditory meatus was not touched and it was very doubtful whether anæsthesia of this area had been obtained, and therefore the case affords no evidence as to the possibility of performing an operation on the mastoid in a case where it would be necessary to interfere with these parts. Before the operation the specific gravity of the urine was 1038 and it contained 3 per cent. of sugar. By means of dieting while in hospital all the sugar disappeared from the urine.

## DISCUSSION.

Mr. LAYTON said that Dr. Mutch had diagnosed the case as one of true diabetes. The patient had then had only a chronic discharge from the ear, and he (the speaker) had not considered that operation was needed; there was no deafness. Some weeks later pus had been pouring out of his patient's ear, and swelling had extended as far down the neck as marked by the scar, and there had been a small sinus in the external auditory meatus. He (Mr. Layton) had been glad to have the opportunity of doing the operation under local anæsthesia, as there were occasions when that might have to be done. He had used 1 per cent. novocaine, with one drop of adrenalin to the drachm. He did not get much help from Smith's block anæsthesia, because of the swelling and induration of the skin. He had inserted the needle at the posterior margin of the sterno-mastoid muscle. It had been difficult to know what to do with the external auditory meatus; he had felt afraid because of the temporo-mandibular joint. It did not seem to be much use blocking the auriculo-temporal nerve at that point, as the tympanic branch had already come off deep to the joint; therefore he had tried to hit it off behind the neck of the lower jaw. The patient had cried out when the long incision was made. The whole mastoid had been a shell filled with pus, and was of the Bezold type. Chiselling had not caused much discomfort. Nothing had been done to the external meatus or middle ear. Insulin had not been given, as the sugar could be controlled by other means.

Mr. MUSGRAVE WOODMAN said he did not think it was possible to cut off sensation from the ear by block anæsthesia, as many of the sensory nerves came from a depth beyond the reach of block anæsthesia. He thought Mr. Layton would have obtained equally good results by local infiltration.

Recently he (the speaker) had had a case of extensive suppuration of the neck in a patient who had diabetes, and was an "alcoholic," and who therefore would probably not have stood a general anæsthetic. He (Mr. Woodman) had injected novocaine into the layers of the skin, making a wide wheal, anæsthetizing the skin in several places, and the patient had had no discomfort. During operation under local anæsthesia, the patient complained of the jarring of the mallet during chiselling, and consequently he (Mr. Woodman) advocated placing a single layer of lint over the mallet head; this did not interfere with the efficacy of the hammering, but it reduced the shock.

Mr. JAMES ADAM said that some months ago he had performed a similar operation under local anæsthesia on an old gentleman who was a bad "life." The patient had had recurrent suppuration, pain and infiltration over the mastoid lasting some weeks. A general anæsthetic was refused, so he (the speaker) had used eucaïne, which he had employed for twenty-five years. The operation had been fairly satisfactory, but he had waited for twenty minutes, according to the French advice, and he ought to have started at once. Where there was much infiltration of skin and subcutaneous tissue, as in this case, it was best not to wait. The patient did not complain much; he was out of bed in two days, and had left the nursing home in a week. He (Mr. Adam) asked what was the experience of scopolamine and morphia as an aid.

Dr. P. WATSON-WILLIAMS said that when the tissues were inflamed there was a marked tendency to inhibit the effect of local anæsthetics. The combination of surface—with infiltration—anæsthesia was nearest to the ideal for these cases. In a recent case in which a post-aural flap was bound down by spreading epithelium, it was difficult to be entirely successful with infiltration anæsthesia or novocaine, and he (the speaker) had had to assist the effect by applying cocaine solution. In mastoid operations under local anæsthesia, it was well to do away with the mallet; he (Dr. Watson-Williams) did not now carry one for these cases; his gouges, shaped in a particular way, were adequate, and rendered hammers superfluous. Even when the patient was under a general anæsthetic it was an advantage to avoid the jarring inevitable with hammering, however gentle.

He (the speaker) wondered how long the diabetes had existed in this case. He had known marked but transient glycosuria in connexion with—and, he thought, arising

from—sinus suppurations in the nose, and he thought this might be an instance of that. Had the glycosuria diminished since the operation?

Mr. A. R. TWEEDIE said he had also seen the mastoid operation performed under a local anæsthetic. The essential part of this method was the sub-periosteal injection of a local anæsthetic in the posterior wall of the bony meatus, by means of which both the middle ear and sub-periosteal space behind the auricle were anæsthetized. The anæsthetic, however, had not appeared to be completely satisfactory, and the procedure had met with some resentment from the patient.

Mr. LAYTON (in reply) said that he did not wish it to be thought that he was advocating local anæsthesia instead of general anæsthesia in mastoid operations; but there were cases in which local anæsthesia might be necessary. With regard to the glycosuria, he would obtain the medical report to complete the case.

### Case of Mastoiditis—Peri-sinus Abscess—Meningitis— Pneumococcal Infection—Recovery.

By T. B. JOBSON, M.D.

W. G., MALE, aged 31, came to the Royal Surrey County Hospital, Guildford, on May 22, 1923.

History of discharging ears since childhood. His doctor reported that ten days previous to admission he complained of pain in right ear and giddiness. Temperature normal, pulse 80. Had become gradually worse.

May 22, 1923.—On admission he looked very ill—face sallow and drawn, tongue foul and dry, breath fœtid, mentality dull. Temperature 102° F. Foul discharge from right ear—tenderness behind ear, and œdema. Occipital headache marked. Eye reflexes normal, ? slight nystagmus to left. Neck muscles stiff. No Kernig's sign. Knee-jerks equal and normal. Grips equal and strong. Co-ordination good. No history of vomiting.

May 22, 1923.—Operation (1). Radical mastoid. Bone very dense. Extradural abscess found above and behind antrum—fœtid pus. Cholesteatoma and polypi in antrum. Lumbar puncture: fluid turbid, tension about normal.

Pathologist's report: "Slight turbidity. Cell count 3,400 per c.mm. Type of cell—polymorphonuclears. Culture—pneumococcus (pure growth)."

May 24, 1923.—Patient looks unexpectedly well. Mentality clear. No pain or headache—feels better. Spontaneous nystagmus to left. Slight but definite. Head retraction still present, but less. Fundi normal. Vision good.

June 4, 1923.—Patient did well on pneumococcal serum and autogenous vaccine until June 4, when he had a rigor, vomiting, frontal headache, temperature 103° F.

Operation (2). Wound opened up. Left sinus exposed—small abscess found in the groove—sinus clot incised, but not turned out as it looked healthy.

Patient made a good recovery and was quite well within a month. Nystagmus to left lasted for about a week.

In this case of acute purulent diffuse otitic meningitis the patient recovered, although the C.S.F. had shown a very large pleocytosis and pneumococcal infection. The operations done were for the removal of the primary focus. No attempt was made to drain the meninges. This case supports the dictum of Mygind that "if one does not stop the development of a meningitis by elimination of the primary focus, nothing can stop it."

## DISCUSSION.

Mr. E. D. D. DAVIS said that he thought this was a localized meningitis, arising from sinus infection. Twenty-seven per cent. of cases of otogenic meningitis were said to arise from that source, and thirty per cent. from the internal ear. The reports showed that in the majority of the successful cases the meningitis was due to sinus infection, and those were cases in which one would not drain the dura if the patient was doing well; at least drainage should be delayed for twenty-four hours and careful observations of the cerebro-spinal fluid should be made. If the dura were opened, it should be done in front and behind the lateral sinus. These cases were more favourable. It was a very different matter when the infection of the meninges arose through the internal auditory meatus and labyrinth, for then there was fulminating leptomeningitis, and if prompt action was not taken, the patient would be lost. It was difficult to know when to open the dura, and when not to do so, and he asked the opinions of others. To leave a clot in the lateral sinus was dangerous, as one could not tell by inspection whether it was infected or not.

Mr. G. J. JENKINS said that such a case appeared to indicate that the individual was capable of dealing with the organism in the sub-arachnoid space, i.e., if the virulence of the organism was low, and the patient's resistance was high. It must not, however, be concluded from this that all cases should be left alone. If there were no resistance at all, it would be of no use to do anything for a patient with meningitis. Spontaneous recovery after lumbar puncture indicated that operators should go further in meningitis cases.

Dr. KERR LOVE asked whether, assuming this to have been a general meningitis, drainage of the sub-arachnoid space had been considered. One or two successful cases had been recorded in which that space had been drained, an opening being made over the frontal region, and counter-openings behind the base of the skull and in the lumbar region. An obstacle to this procedure was the number of skilled assistants it required, and the danger of removing too much cerebro-spinal fluid. The operation had been done by Dr. Eagleton, of New Jersey, and he (the speaker) wondered whether any Members of the Section had done it.

Mr. E. D. D. DAVIS said that he meant that meningitis arising through the internal auditory meatus and labyrinth was a very dangerous infection. In cases of sinus infection the meningitis was more localized, as contrasted with a general meningitis in the other case.

Mr. TWEEDIE said that Dr. Jobson had no doubt dealt with the case correctly, as events had proved, but he (the speaker) would be glad to know what particular appearance of the clot had determined Dr. Jobson to leave it untouched.

Dr. JOBSON (in reply) said that the wound was opened up within twenty-four hours of the secondary rise of temperature and the dura was firm and hard; it was not necrotic. There was no evidence of breaking down of the clot. The pneumococcus organism was recovered from the cerebro-spinal fluid, therefore the infection had travelled 'down'; this fact was against it being regarded as merely a localized infection.

### Case of Mastoiditis and Double Peri-sinus Abscess with Left Cerebellar Abscess. Abscess Cavity sterilized by Zinc Ionization.

By T. B. JOBSON, M.D.

C. D., AGED 13. Three years ago: Acute otitis in *right* ear; symptoms of mastoiditis; no operation. Ear continued to discharge for two years with intermittent headache and pain.

May, 1922: Admitted to hospital, with acute mastoiditis.

Operation: Peri-sinus abscess containing about a drachm of pus. Ligature of deep jugular vein. Good recovery.

Two months later, readmitted under my care. He looked ill; temperature, 99'6° F.: complained of headache and pain in *left* ear, but no history of discharge. Nevertheless the left meatus was full of pasty offensive cholesteatomatous pus. Slight spontaneous nystagmus to right. No vomiting or giddiness. Fistula symptom present, a slow single wide transverse excursion to right.

August 24, 1922.—Operation: Radical mastoid. No sinus to labyrinth found. Posterior to antrum an extradural abscess was found containing about a drachm of pus. This was exactly the same size and in the same situation as that found by Mr. Butler on the right side. Sinus not opened.

August 25, 1922.—Next day nystagmus more marked. Headache still bad. All reflexes normal.

August 26, 1922.—Nystagmus increasing.

August 27, 1922.—Nystagmus now of third degree. Headache agonizing: mostly occipital; spreading down neck. Vomited several times. Mentality clear but tires quickly if asked several questions. Grips equal. No past pointing with either hand. No inco-ordination. Operation: Wound re-opened. Bone over Trautmann's triangle removed in front of lateral sinus giving an exposure 1 cm. wide. On incision of the dura about three or four drachms of stinking pus came away. Cavity gently mopped out. Rubber tube drain inserted.

August 28, 1922.—Much better. No nystagmus. Headache only slight. Vomited twice after his dressing.

August 29, 1922.—Grips equal, but left hand tires quickly. This was the first muscle weakness noticed. Rubber drain replaced by gauze.

August 30, 1922.—For the next three weeks he gave cause for anxiety. He "flared" several times with a return of headache and vomiting. The sinus was opened up more than once and the rubber tube replaced. Nystagmus returned. Deviation to left well marked.

September 26, 1922.—*Fistula test of cerebellum*: With the rubber tube in the cerebellar track pneumatic pressure was applied. This produced no alteration in the nystagmus. Crossed deviation of limbs—to left with left arm, to right with left leg.

*Walking*: He slaps left foot on ground as if it were "asleep."

*Ophthalmic examination*: "Definite slight papillitis—about equal on both sides."—Dr. E. J. Smythe.

October 4, 1922.—Has been up for a couple of weeks. Walks falteringly. Tends to fall to left. To-day he "flared" again. Temperature 102° F. Headache frontal and occipital. Nystagmus to right. Opened up again. Two small tubes inserted. For some days condition "ebbed and flowed," with occasional vomiting.

October 27, 1922.—Ionized the sinus with zinc solution. This I did with some trepidation, giving 4 ma. for 15 minutes. No nausea or vertigo produced. The tubes were left out, the skin margin painted with iodine and the area covered with sterile gauze.

November 3, 1922.—There was no relapse after this. Wound healed without any further suppuration. Bacteriological report, August 27, 1922 (Dr. E. Shaw): "Micro. Gram-negative organism. Culture: A pure growth of coliform organisms."

Interesting points in this case are:—

- (1) The symmetrical condition of mastoiditis with extradural abscess.
- (2) The extensive disease of the left temporal bone without any history of otorrhœa.
- (3) The presence of the fistula symptom without labyrinthitis.
- (4) The diagnosis of cerebellar abscess was made from the increasing violent nystagmus, the boring, agonizing, occipital headache and the vomiting.
- (5) The muscular signs, deviation, dysdiadokokinesis, ataxia, &c., did not appear for several days after the abscess was evacuated.
- (6) Direct pneumatic pressure to the abscess cavity caused no alteration in the nystagmus. I thought that possibly the original fistula symptom might be explained by pressure being transmitted through the antrum and extradural abscess to the cerebellar abscess, but I do not think now that this theory is feasible.
- (7) Sterilization of the cerebellar sinus by zinc ionization yielded a surprisingly happy result. The boy's condition had been ebbing and flowing for two months previously, whereas after the ionizing he got rapidly well, and has had no recurrence. This is, as far as I am aware, the first time that sterilization of a brain abscess cavity by zinc ionization has been tried.

### Case of Cerebellar Infection without Clinical Manifestation. Lateral Sinus Thrombosis. Removal of Sinus Walls.

By G. J. JENKINS, F.R.C.S.

G. S., MALE, aged 26. Admitted to King's College Hospital, June 22, 1923, complaining of pain in and behind the left ear; had had pain for a week.

*Earache*: In the left ear in childhood; also two years ago.

*Deafness*: In the left ear has been present since infancy. There had been discharge for three days. In boyhood much discharge from the left ear.

*Tinnitus*: In the left ear (noise like a train running).

*Vertigo*: There had never been any at any time.

*Headache*: Frontal and left occipital.

*Rigors*: For two days before admission.

*Examination*: Eye movements good; pupils equal and react equally to light; no spontaneous nystagmus. No facial paralysis. No rigidity of neck. Plantar reflex, flexor both sides. Knee-jerks equal and active. Abdominal reflexes normal. Co-ordination good. Periosteal thickening over mastoid on the left side. General tenderness, most marked in region of triangle. Left superior deep cervical glands enlarged. Left ear: Pus in meatus, meatal swelling posteriorly. Weber, to left.

*Eye*: Right fundus, veins engorged. Edges of disc blurred and very slight ring of œdema. Left fundus, venous engorgement. Blurring of disc margin.

*Operation*: Anæsthetic—chloroform and ether. Lumbar puncture was performed and the cytological report made as operation proceeded.

The report was as following: Cells per c mm., 2,750; lymphocytes, 31 per cent.; polymorphs, 69 per cent.; total protein, 0.3 per cent.; globulin reaction, positive; glucose, normal; chlorides, 0.72 per cent.; culture, pneumococci; the left jugular vein was ligatured and divided; cellular mastoid, general infection; extradural abscess in the middle and posterior fossæ.

Lateral sinus was thrombosed and the occipital bone was removed for about three-quarters of the horizontal part. The lateral sinus was thrombosed to, or almost to, the middle line, and was not obviously septic beyond junction of posterior and middle third of horizontal portion.

There was pus in the sinus to the lower end of the vertical portion.

Free flow of blood was obtained both sides.

The sinus wall and dura of the posterior fossa below and behind (and slightly above) the sinus were obviously necrosed and occipital bone had to be removed very extensively to reach healthy membrane.

The necrosed areas of membrane and sinus walls were removed as it seemed probable that the deeper structures would be infected, especially when considered with the result of the examination of the C.S.F. This exposed a large area of cerebellum and a small narrow area of cerebrum.

An area (about the size of a two-shilling piece) of the cerebellum was obviously infected, and whilst under observation a small bead of pus, about the size of a pin's head, appeared. This region was found to be in a semi-fluid state, and a mass about the size of a small walnut was removed. (Before the removal the margins of the opening of the dura were protected by a sterile iodoform-bismuth mixture.) The whole wound was then wiped over with a bismuth-iodoform mixture and packed lightly with gauze. A large drainage tube was left in the cavity in the cerebellum.

The cavity closed-in rapidly, and at the end of five weeks a plastic operation was performed and the wound closed. The patient returned to work three months after the operation.

The noteworthy features of the case are:—

- (1) The extensive disease of the lateral sinus.
- (2) The extensive sloughing of dura mater in association with extradural abscess.
- (3) The extensive lesion of the cerebellum without any clinical manifestation, indicating the importance of removing dura when such a condition as that described is found. Was the cerebellar condition due to infection or to venous obstruction, in the first place?

#### DISCUSSION.

Mr. G. J. JENKINS said that in Dr. Jobson's case there was the fistula sign without labyrinthitis. If there had been severe labyrinthitis the fistula sign would disappear. Another important point was that a patient might have the fistula sign well marked for three or four weeks, and then it would spontaneously disappear without apparent reason. He (Mr. Jenkins) had a case of the kind very recently. The important point to decide was, whether pressure on the cerebellum would necessarily cause a cerebellar ataxia. It was not uncommon to find very large tumours of the posterior fossa, and there might be extensive suppuration in relation to the cerebellum without ataxia.

Mr. A. R. TWEEDIE said he did not think that responses to any tests could be regarded as referable to the cerebellum alone. Some time ago Bárány had published an account of his attempt to map out the cortex of the cerebellar hemispheres, by application of cold, endeavouring in this way to establish "centres" for the pointing reaction. On reconsideration, however, this attempt had been shown to be of questionable value. More recent research seemed to indicate that if any such "centres" for co-ordination did exist in the cerebellum, they were situate in the middle lobe and not in the hemispheres. He (the speaker) questioned the applicability of any such "fistula test" for the cerebellum as described, but he would be grateful to Dr. Jobson for further information on this point.



Mr. E. D. D. DAVIS asked how Mr. Jenkins removed the lateral sinus, and whether there was trouble with hæmorrhage. Eagleton's book led one to think that excision of the lateral sinus was difficult.

Mr. SYDNEY SCOTT (President) said it was interesting to contrast Dr. Jobson's second case with his first and with the case shown by Mr. Jenkins. Surgeons who had had experience would say it was safe to do what Dr. Jobson did. Evidently in Dr. Jobson's case the trouble was attacked early.

Personally, he (the President) would not like to claim that the use of electricity in an abscess cavity "sterilized" the abscess cavity. He would be interested in the views of other members.

Dr. KERR LOVE said that eighteen months ago, in connexion with the Glasgow Education Authority, treatment of middle-ear cases by ionization was begun systematically, at first under his care. After eighteen months of it, the following excerpt from the Education Authority's report was of interest: Treatment by ionization had been extended and another set of apparatus had been provided. Since the inauguration of this form of treatment, 216 children had been treated, on 901 occasions, with proved benefit on many occasions. For the year covered by this report, the figures were 152 children on 798 occasions. When he was in charge of the work the cases were selected by him according to the directions now given by Dr. Friel; i.e., they were cases with large perforations, and, as far as possible, those in which mastoid involvement was not present. The strength of current used was from one to two ma., continued for from eight to ten minutes, and the usual coagulation effect was seen in the middle ear. He (Dr. Kerr Love) did not deliberately exclude other cases, such as of mastoid disease following scarlet fever and measles. They were dealing with chronic otorrhœa of mixed origin, which had resisted the ordinary cleansing methods. In only a few of the cases was the mastoid operation done, and in those in which it had already been done the treatment was not successful. Before the treatment the most careful cleansing of the ear had been carried out, including drying. The number of cures recorded was less than 10 per cent., and this was not a brilliant result. The general impression he (Dr. Kerr Love) had formed was that if one excluded, as one must, the cases which presumably had mastoid involvement, i.e., those arising from scarlet fever, measles, &c., and in the remaining cases performed operations on tonsils and adenoids, and disinfected the ear, there would be no material left requiring ionization; the patients would have been cured by the other methods.

Dr. A. R. FRIEL said he agreed largely with the theoretical part of Dr. Kerr Love's statement. In these cases it was essential to diagnose the condition which was keeping up the suppuration. Cases in which there was sepsis in the mastoid antrum and attic were not suitable for treatment by ionization alone. One patient whom he (Dr. Friel) had shown to-day had had infection of the mastoid with cholesteatoma visible at the aditus, and it had been found necessary to destroy part of the outer wall of the attic by electrolysis. The object of this was to convert this "mastoid case" into one of sepsis in an accessible situation. He (the speaker) had diagnosed mastoid infection by aspirating pus from the aditus with Siegle's speculum. Dr. Kerr Love had said that the tonsil, adenoid, and nose cases would get well with ordinary treatment if the nose were properly attended to, but he (Dr. Friel) had seen at the ionization clinics many children who had had their tonsils and adenoids removed, yet the otorrhœa still persisted, but it was readily cured by zinc ionization; he (the speaker) had also seen a fair number of children with enlarged tonsils and, presumably, adenoids, in whom the otorrhœa had been readily cured by ionization alone and did not return. There were, on the other hand, a few patients with inflamed tonsils in whose case no permanent result could be expected unless the tonsils were removed. This was true also of cases in which there was rhinitis. He (Dr. Friel) thought the importance of operating on tonsils and adenoids for otorrhœa had been exaggerated.

The results he had obtained at the ionization clinics were considerably better than those in Glasgow recorded by Dr. Kerr Love. He (Dr. Friel) believed that 50 per cent. of the cases of chronic otorrhœa in a school were readily curable, and that a

further 25 per cent. were made curable by the removal of an accessory factor—such as a polypus, a deflection of the septum nasi, or inflamed tonsils—before ionization. The remaining 25 per cent. probably needed some form of mastoid operation.

Mr. G. J. JENKINS congratulated Dr. Jobson on the success of his case, but said that he (Mr. Jenkins) thought the treatment by ionization of an abscess cavity in the cerebellum was not without risk, and that he (personally) would not adopt it, even with a current of very low strength. He thought some of the centres of the medulla might be involved. Many cases were cured without ionization, and if they were not, there were other means available, without running this risk.

Dr. JOBSON (in reply) said that he had no intimate knowledge of the results of applying pressure to the cerebellum; it had occurred to him to try the experiment, and the pressure had not caused any alteration in the nystagmus.

He thought it was agreed that zinc ionization was capable of sterilizing a cavity when the fluid was in immediate contact with all the organisms. Accessibility was of the first importance. It was not quite correct to say that the abscess cavity was sterilized; he ought to have said that the *cerebellar track* was sterilized; by that time the abscess had narrowed down to the dimensions of a small tube  $2\frac{1}{2}$  in. deep. The patient had been having symptoms for two months, and after ionization was done he got well; therefore he (Dr. Jobson) felt that some credit was due to the ionization. Since then he had ionized a temporo-sphenoidal abscess, the size of a hen's egg, in a case of *B. coli* infection. The first attempt had not proved very satisfactory, therefore he had used a current stronger than that used in the present case; there had been no bad symptoms at all and the patient had gone on very well afterwards. A week ago he (Dr. Jobson) had again ionized the cavity, and there had been a great diminution of the pus. There was still some pus, and he intended ionizing again. If suppuration in such cases could be cut short in this way the means were well worth carrying out. In ionization of ear cases, much depended on the technique. He (the speaker) had seen cases in various hospitals in which the ionization had been done by nurses, and in those circumstances unsatisfactory results were not surprising; often the ears had not been cleared out properly, and there had been pus and debris left behind, so that the cavity was not accessible to the ionizing process. To get sterilization of the tympanic cavity it was necessary to have an open tympanum; everything possible must be sucked out with a Siegle's speculum; the cavity must be washed out with an intra-tympanic cannula; cocaine and adrenalin must be put in to effect shrinkage of the oedematous tissue, and then the cavity should be washed out again with the zinc solution. During the year 1922, at the Central London Throat Hospital, he (Dr. Jobson) had ionized fifty patients who had been under the treatment of surgeons for months—some of them for years—and the good results had been almost astounding. In about 80 per cent. of the cases dry ears had resulted; some of these ears had been suppurating for ten years, some for longer time and one application had been sufficient to dry them up. He (the speaker) was a convinced believer in ionization in certain cases of middle ear sepsis, if carried out under proper conditions and by a competent aurist.

### Model Illustrating the Movements of the Otoliths.

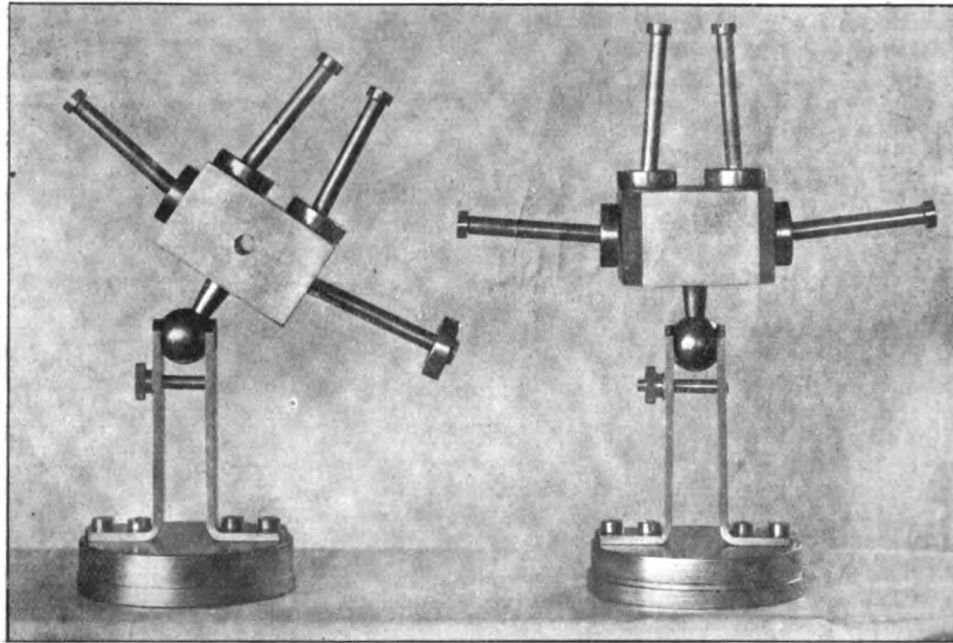
By A. R. TWEEDIE, F.R.C.S.

This model is based on the results of the investigations of Professors Magnu and Dr. de Kleyn, of Utrecht.

The right hand figure shows all four otoliths *at rest*. On the left side one sagitta is shown *in action*. By moving the "head" to the opposite side this sagitta comes into the resting position, and the other into action.

54 Tweedie: *Movements of the Otoliths*; Muecke: *Petrous Bone*

The "head" can also be tilted backwards and forwards so as to bring the lapuli into action.



**Complete Sequestrum of the Petrous Bone.**

By F. F. MUECKE, C.B.E., F.R.C.S.

IN 1917 patient was in the neighbourhood of a shell-burst, the effects of which caused his ear to bleed. The ear has since been suppurating. A month ago facial paralysis set in. The patient thought that the paralysis was caused by a defective tooth, so he went to a dentist and had the tooth removed. The anæsthetist noticed the paralysis and ear discharge and the patient was consequently transferred to the aural department.

Operation: Complete mastoid. The whole floor was seen to move. A large complete sequestrum of the petrous portion at least  $\frac{3}{4}$  in. in thickness was removed with difficulty. The carotid artery could be seen. Smaller sequestra of the inner wall of the aditus were also removed. The patient made a good recovery.

## Section of Otology.

President—Mr. SYDNEY SCOTT, M.S.

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### The Surgical Treatment of Suppurative and Certain Non-Suppurative Affections of the Labyrinth.

By Sir WILLIAM MILLIGAN, M.D.

To discuss affections of the labyrinth with our President in the Chair is rather like "carrying coals to Newcastle"; but he will I am sure place his knowledge of, and practical experience in, this particular domain of Otology at our disposal to-day, and so assist in unravelling many of the difficult problems which surround this particular subject.

The clinical picture presented by disease of the labyrinth varies with the particular segment of the internal ear involved, whether its static segment, its dynamic segment, or a combination of both. The fact that one segment is involved does not necessarily mean that disease has extended to the other, and the practical deduction from this postulate is that if any surgical interference is called for it should be so planned as to cause the least possible interference with the non-infected or only partially infected area.

Implication of the cochlear segment is indicated clinically by a varying degree of nerve deafness and by persistent tinnitus; implication of the vestibulo-canalicular system by vertigo, disturbances of equilibrium, nystagmus, nausea or actual sickness.

Where both segments are involved there is a combination of these symptoms, the clinical picture varying with the area more particularly affected.

Purulent disease of the internal ear may result from a septic process spreading from the base of the brain by way of the internal auditory meatus along the sheaths of the auditory or facial nerves. In rare cases, as the outcome of an extradural or subdural abscess, the eminentia arcuata may become eroded and afford an entrance for pyogenic organisms into the superior semicircular canal after which the invasion may become diffuse.

In fracture of the base, infection of the internal ear may also occur. The most frequent route, however, is by way of the middle ear following erosion of one or other portion of its inner wall or capsule of the labyrinth, and in my experience this pathological process results as a rule from chronic infective disease of the middle ear and only rarely from acute infective disease.

When a labyrinthitis is induced as the result of acute purulent otitis media it is almost invariably an acute serous process, in other words an "induced serous labyrinthitis."

The actual portal of entry for infective organisms is variously stated by those who have written on the subject. My own experience is that erosion of the external semicircular canal is far and away the most frequent portal in cases of circumscribed labyrinthitis, and the fenestra ovalis in cases of diffuse purulent labyrinthitis. Invasion by way of the fenestra rotunda is rare. At times however the outer labyrinthine wall may be so riddled with caries or so destroyed as the result of a sequestering and necrotic process that the actual and original portal is not recognizable.

In cases of tuberculous otitis media which have subsequently spread to the internal ear I have frequently found the pars promontoria destroyed, so offering a wide and easy entrance for successful invasion.

The question naturally suggests itself why such a hard and compact bony wall as the capsule of the labyrinth or the dense fibrous tissue forming the stapedio-vestibular ligament, or that covering the fenestra rotunda should not be able to resist organismal invasion. Is it owing to the virulence of the infective organisms or is it the result of a mechanical cause?

In examining cases I have paid particular attention to this point and am disposed to think that the exciting cause may be summed up in two words—defective drainage. This may be due to an inadequate perforation *ab initio* of the membrana tympani, to the presence of exuberant granulation tissue in the middle ear, or to cholesteatomatous formation in the tympano-antral cleft or possibly to the configuration of the osseous walls; but neither granulation tissue nor cholesteatome would ever have been present had a free exit for pus been provided in the first instance either by nature or by art. The types of labyrinthitis met with clinically are:—

- (1) Circumscribed labyrinthitis.
- (2) Diffuse purulent labyrinthitis.
- (3) Diffuse serous labyrinthitis.

The fact that an erosion of the lateral canal is found does not justify a diagnosis of circumscribed labyrinthitis. The erosion may be, and probably frequently is, quite superficial and it must have been the experience of many here present to have found these erosions quite unexpectedly and in cases where no objective or subjective labyrinthine symptoms were present.

I know of no means of diagnosing an erosion *per se* until it is actually seen by the naked eye. Nor in my opinion does it call for any special treatment, it should be left severely alone, reliance being placed upon the effects of the drainage afforded by one or other form of post-aural operation. Interference is likely to cause a circumscribed labyrinthitis and who knows when a circumscribed process may not become a diffuse one? The proof that non-interference in cases of erosion is the proper course is afforded by the way these cases clear up after drainage has been effected. The time to interfere is when such cases do not clear up.

That circumscribed labyrinthitis is a real pathological entity has been histologically proved, but clinically is almost impossible to diagnose. That a labyrinthine infection is present is made evident as the result of static and caloric tests; but can any one really say what the limits of the process actually are? The labyrinthine symptoms in such cases may be merely the expression of a serous cedema, the result of the presence of a septic focus in the neighbourhood, just as cedema of the cellular tissues of the face follows a mosquito bite. The labyrinth is irritated with a resulting increase of peri- and endolymph with increased pressure and consequent static incoördination and nystagmus. If we believe that the erosion is more than an erosion, that it is in fact a fistula,

what should be the line of treatment? The consensus of opinion is, I believe, to leave the fistula alone—certainly that is my opinion—and over the portal might well be written “*nemo me impune lacessit*.” In the absence of definite symptoms it is an unpardonable offence to probe the fistula or to curette—it is only courting disaster by breaking down possible adhesions and converting what may have actually been a circumscribed process into what almost certainly will become a diffuse one, with all its dangers. At the moment the clinical differentiation between the “open door” and the “protected territory” defies solution.

Entirely different however is the picture of a case of diffuse purulent labyrinthitis with the clinical symptoms of profound loss of hearing, nystagmus, nausea, high temperature, &c. Such cases are dangerous in the extreme and call for prompt and efficient surgical interference. The chances of infection reaching the base of the brain and inducing an acute septic meningitis are imminent, apart altogether from the fact that unless free drainage is effected the function of the auditory nerve will almost certainly be completely abolished.

I have, however, noted in quite a number of cases which presented the clinical picture of a diffuse purulent labyrinthitis, that the main infection apparently resided more in one segment than in the other, the clinical symptoms being predominantly static or predominantly acoustic.

Where the path of infection is through the static wall the symptoms at the onset at least will be mainly static, where through the acoustic wall, mainly acoustic. Later on in a diffuse infection no differentiation is possible.

How far do these facts assist us from the point of view of surgical interference? Some surgeons may say that they should make no difference, that the one and only indication is to provide ample drainage even at the expense of a total unilateral loss of hearing.

There is, however, to my mind a *via media*, a course of action which, while providing efficient drainage, may yet do something to conserve some functional activity, and after all the ear is the organ of hearing.

In selected cases my practice has been, whenever possible, to leave the cochlear segment alone as much as possible, and it is surprising what a useful amount of hearing may still be preserved in apparently the most unpromising cases. To attain this object I freely ablate the static segment and merely remove the pars promontoria without disturbing or in any way interfering with the columella. Instinct would teach us to ablate the cochlear segment as freely as the static, while practical experience shows that it is not always necessary to do so, the “*vis medicatrix naturae*” asserting itself and with the aid of the excellent drainage afforded frequently allowing of the preservation of a useful amount of hearing. When, however, the function of hearing is practically destroyed, when clinical tests point to a severe and diffuse infection the freer the drainage the better. When a complete labyrinthectomy is called for I perform what I have called the “bridge” operation in preference to Scott and West’s double vestibulotomy, or to Hinsberg’s or Neumann’s operation. The results are surprisingly good, the technical bugbear being the Fallopian aqueduct with its contained facial nerve.

With strong illumination, with sharp labyrinthine chisels and gouges, and with meticulous precision, the nerve should escape injury. Formerly I used a facial nerve protector and a bur, but have discarded both. I cannot plead not guilty to having never injured the facial nerve, but so far as I am aware in the fifty-four cases on which I have operated any injury has

never been permanent, and one is generally reminded by the patient or his friends when it is !

#### NON-SUPPURATIVE LABYRINTHITIS.

I now pass to a much more debatable and much more difficult problem, viz., the surgical treatment of certain non-suppurative affections of the labyrinth.

The minute pathology of labyrinthine disease still leaves much to be desired, and the difficulties of founding accurate treatment on incomplete pathological findings is naturally great.

The minute pathology of true Ménière's disease, a distinctly rare disease, is still *sub judice*, but what of the pathology of those cases diagnosed for lack of more accurate knowledge as pseudo-Ménière's disease ? What is the pathology of the pseudo-Ménière syndrome ? Are those cases due to effusion into the internal ear, to atheromatous changes in the terminal blood-vessels of the part or to some form of neuritis, e.g., a toxic neuritis ?

As clinical entities, such cases are more frequently met with amongst those who are subject to mental strain and brain worry, e.g., bankers, financiers and even doctors ; but are also found amongst ordinary manual labourers, and are characterized by attacks of severe vertigo, sickness, tinnitus and progressive loss of hearing. In some of these patients high blood-pressure is met with, in others a distinctly low pressure, in some evidences of granular kidney, and in others one or other form of digestive disturbance. There are, however, a certain group of otherwise apparently healthy individuals who are subject to violent and sudden attacks of vertigo with sickness, tinnitus and loss of hearing, the attacks recurring at irregular intervals and causing complete incapacity. In such cases, usually with a history of former ear disease, treatment by means of bromides, hydrobromic acid, repeated blistering, &c., is at times followed by some relief, at other times by no relief whatever. The incapacity and depression caused by such attacks is serious and calls for more heroic measures. In such cases—very carefully selected—I have for many years past advised and carried out a partial or complete destruction of the labyrinth.

Realizing as I do the severity of the operation and the risk to the facial nerve, to say nothing of the risks of septic infection of the meninges, the number of cases actually submitted to operation is not large. In all I have done twenty such cases with no fatalities, and in all with relief to the vertigo, but in only 40 per cent. with complete relief to the existing tinnitus. In 20 per cent. the tinnitus was much relieved, in 40 per cent. it was quite unrelieved, from which I conclude that it must have been central in origin *ab initio*, or that, as the result of an ascending neuritis, the nuclei of origin had become involved. The operation is carried out on the same lines as in suppurative cases, the canalicular system being first opened up, then the cochlear and, finally, a free communication made between them.

No attempt is made to preserve any residue of hearing which may exist. In opening the canalicular system in such cases, I have been struck by the fact that there has been little if any increase in the amount of peri- or endolymph, the deduction being that it is not an increase of labyrinthine tension which produces the clinical symptoms. To the naked eye the appearances presented during operation are those of the normal structures, and as no opportunity has ever presented itself of securing a post-mortem specimen, I am unable to give any histological findings. The absence of intralabyrinthine tension, combined with the clinical picture, inclines one to the belief than one has to deal with

a toxic neuritis affecting at first the nerve proper, but ultimately extending to the maculæ acusticæ and there setting up the vicious circle of static inco-ordination, oculo-motor and gastric disturbances.

Owing to the kindness of Mr. J. S. Fraser I am able to throw upon the screen slides which show hæmorrhage into the internal auditory meatus in a case of what was diagnosed as Ménière's disease.

In a case recently operated upon, that of a gentleman, aged 68, quite incapacitated from carrying on business, the internal ear, both static and dynamic segments, was exenterated with complete relief. Within three months of the operation he was able to play an 18-hole round of golf without any trouble, whereas previous to operation the mere act of swinging the golf stick was prone to cause severe vertigo.

In another case, that of a retired merchant, vertigo was so frequent and so pronounced that he could not venture to walk out alone. Since operation, eighteen months ago, he was perfectly well, with the exception that he is still troubled with tinnitus.

In a third case recently operated upon, a signalman on the L. & N. W. railway, who had been off work for two years and who had tried almost every available medicine likely to do good, without effect, complete relief followed operation. In this case slight facial paralysis followed operation, but has quite cleared up.

I merely cite these cases as examples of the relief following ablation of the internal ear, where persistent medicinal treatment had been faithfully tried for periods varying from one to three years without effect.

Realizing as I do the severity of the operation, while at the same time appreciating its benefits, I have only ventured to perform it as a *dernier ressort*.

[At the conclusion of the paper Sir William Milligan showed a number of slides by means of the epidiascope.

(1) Development of the temporal bone. This was of some importance, because the coalescence of the two segments explained the fact that the labyrinthine circulation was practically a terminal circulation.

(2) Section of a child's head, to show the position of the middle-ear cleft and the position of the Eustachian tube, the middle ear, and the internal ear to the base of the brain.

(3) A transverse section showing both middle-ear clefts, both Eustachian tubes, the naso-pharyngeal cavity, and the base of the brain.

(4) To show that in performing the operation, whether for suppurative disease or for non-suppurative, a very free radical mastoid operation was called for. The more room the operator could get, the better, for he then ran less risk of injuring important structures, such as the facial nerve.

(5) Horizontal section showing a portion of the middle ear, cochlea, the cells at the apex of the petrous portion, and the carotid canal.

(6) A dissected specimen showing the position of the semicircular canals in relation to one another and to the Fallopian aqueduct.

(7) Another specimen with the same object, dissected in a different way, a small wire being passed through to indicate the course of the facial nerve.

(8) Specimen of a temporal bone, cut to show the position of the three semicircular canals in relation to one another; also the line of the Fallopian aqueduct, and the differentiation between the compact bone and the cellular bone surrounding it.

(9) A pathological specimen, recovered in the post-mortem room, showing a large fistulous tract leading into the lateral canal. It was the portal of entry in a case of diffuse labyrinthitis, the patient dying of meningitis. No operation had been done.



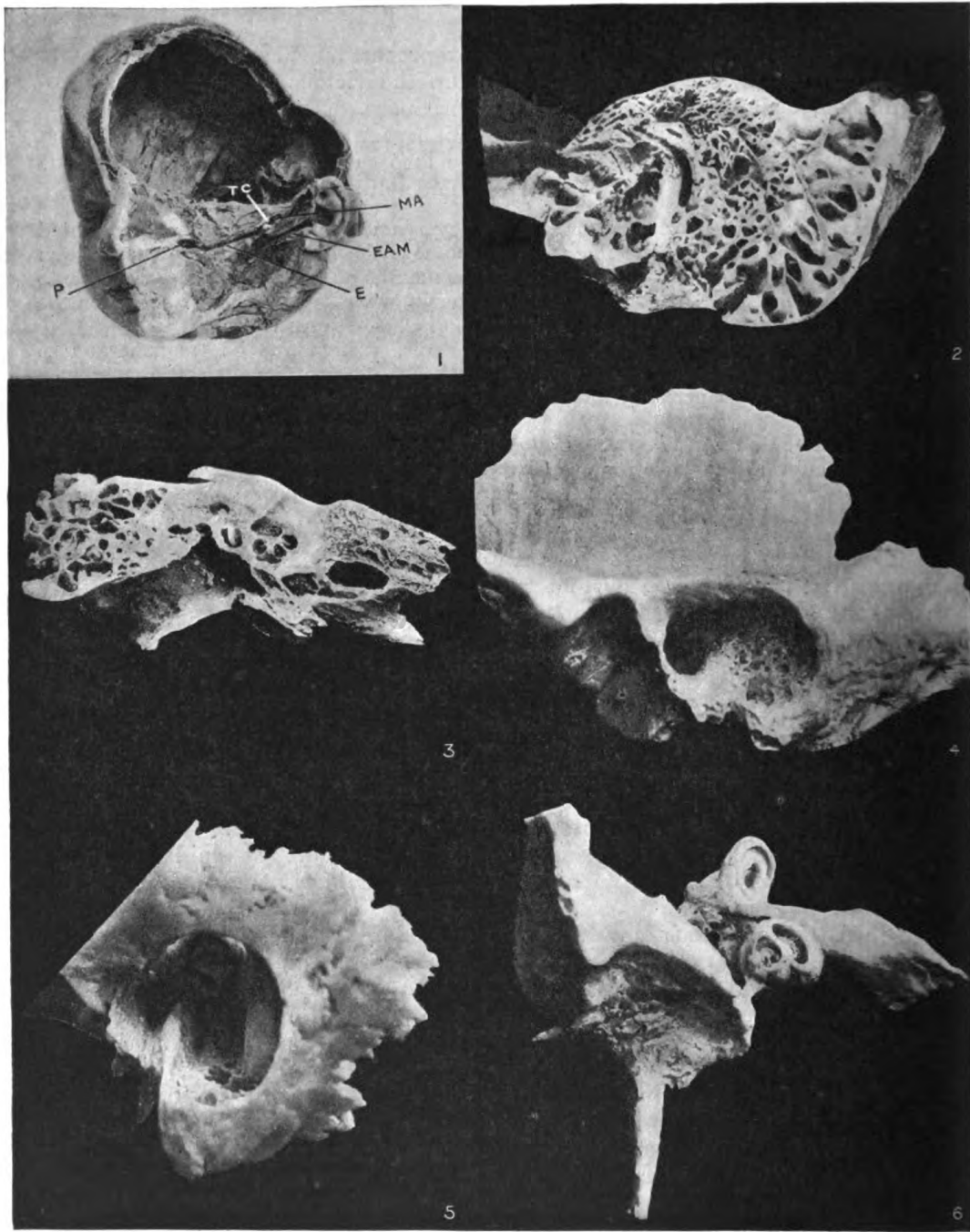


FIG. 1.

No. 1.—Section of foetal head showing axis of middle-ear cleft and external auditory meatus.

No. 2.—Section of temporal bone showing opened-up Fallopian aqueduct and external semicircular canal.

No. 3.—Horizontal section of temporal bone showing middle-ear cleft in its relation to the cochlea.

No. 4.—Temporal bone after the performance of the complete post-aural operation.

No. 5.—Temporal bone to show the relation of the semicircular canals to one another.

No. 6.—Temporal bone dissected to show cochlea and superior semicircular canal. A wire is inserted in the opened-up Fallopian aqueduct.

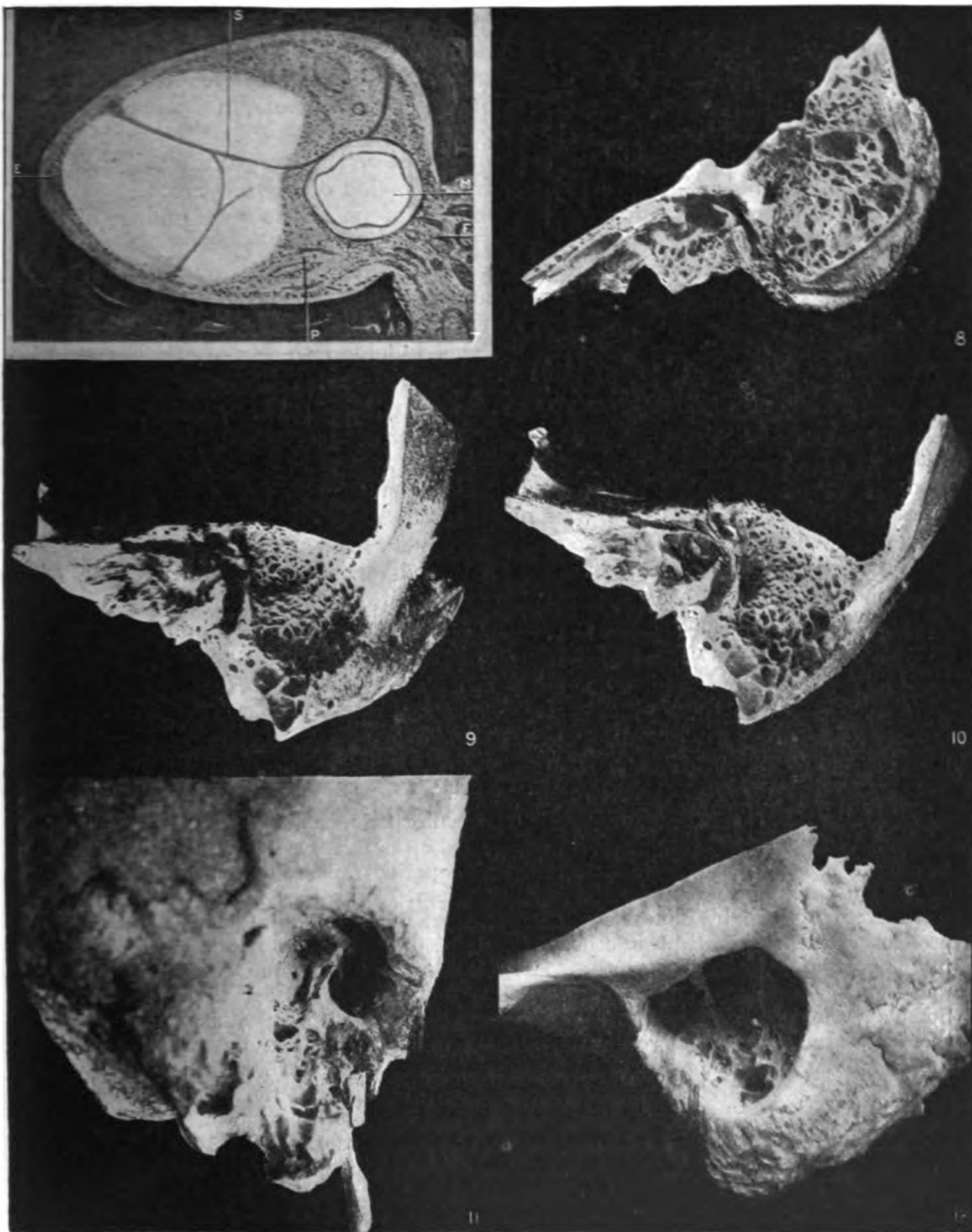


FIG. 2.

No. 7.—Semi-diagrammatic view of a fistula of lateral canal showing the effects of septic invasion.

No. 8.—Temporal bone showing large fistulous tract through the fenestra ovalis leading into internal ear.

No. 9.—Temporal bone showing large erosion of lateral canal.

No. 10.—Temporal bone showing fistulous erosion of lateral canal.

No. 11.—Temporal bone showing the "Bridge" operation. Fallopian aqueduct opened up.

No. 12.—Temporal bone showing the completed "Bridge" operation.

(10) A pathological specimen showing erosion of the lateral canal and its relation to the facial canal; it was a diffuse purulent labyrinthitis.

(11) A semi-diagrammatic representation, showing the spread of infection through what might have been an erosion first, then a fistula, in a case of diffuse infective labyrinthitis.

(12) Showing destruction of the inner wall of the tympanic cavity and of the fenestra ovalis. There was a very free invasion of the labyrinth.

(13) A specimen showing the preliminary steps in the performance of the "bridge" operation.

(14) To show the operation completed, and that there was a very free communication between the two segments. It was an ablation of practically the whole of the internal ear, leaving only compact bone on the outside.

(For the following specimens he was indebted to Mr. J. S. Fraser, of Edinburgh.)

(15) A case which was under the care of the late Dr. Alexander Bruce and Mr. Fraser. The patient presented all the symptoms of what was regarded as true Ménière's disease: sudden vertigo, sudden loss of hearing, &c., with static incoördination, and the patient died of syncope some little time after he was first admitted. The cochlea was normal, but a hæmorrhage could be seen under the epineurium and in the course of the auditory nerve.

(16) Showing the ganglion, also healthy, likewise the organ of Corti. A small nerve fibril was seen crossing the tunnel of Corti.

(17) The semicircular canal system, also apparently normal; no hæmorrhage into the canals, and no hæmorrhage or change in the lining membrane of the membranous canal.

(18) Showing hæmorrhage along the course of the nerve, in the sheath of the nerve and round one of the branches of the nerve itself.]

#### DISCUSSION.

Mr. SYDNEY SCOTT (President) said his own experience did not begin before 1904, but the first case he knew about was operated on by his late colleague, Mr. West: it would be twenty years ago next April. The patient suffered from symptoms very similar to some of those described by Sir William. He believed Mr. West had been inspired to operate by Jansen's work in Berlin, and the same source of inspiration probably influenced surgeons in other cities, and induced them to take a greater interest in the functions of the labyrinth. Mr. West and he, working together, operated upon twenty-nine or thirty cases by 1909, and their work led to some criticism. Some did not believe labyrinth cases occurred so frequently; others thought that there was no necessity to operate on the labyrinth at all. Yet it was clear from the details of the cases reported, that in the majority, the labyrinth had become destroyed by sepsis. This was discovered in the course of the mastoid operation. In the majority of these cases there was no giddiness at the time of the operation, and in many cases there had been no giddiness for years. Since 1909 our clinical observations led us to diagnose with very tolerable accuracy, before doing the mastoid operation, whether we should expect to find disorganization of the labyrinth.

That led to the question whether it had not been too readily assumed that because a patient had vertigo, therefore there was disease within the labyrinth. He would not be contradicted when he said some unnecessary operations had been done. They had to guard against needless destruction of the labyrinth. The following was an example: A child was attacked with tonsillitis on a Saturday, and on the following Thursday there was acute earache. Two days later, the patient was admitted into hospital with violent giddiness and marked nystagmus, and the drum was bulging under tension. It was found that the labyrinth reacted to tests. The drum was incised, and in less than twenty-four hours the giddiness and nystagmus had disappeared, without loss of hearing

or loss of labyrinthine function. Another class of case was that in which the infection of the middle ear was so virulent that the organisms penetrated into the internal ear. Quite recently he had operated on a patient sixty hours after the onset of symptoms of otitis media, and the patient already had acute destructive labyrinthitis and streptococcal meningitis, and died eleven hours later. Sir William Milligan had also raised the question whether one should operate on non-infected cases of vertigo, and in deciding this important question it was agreed that the clinical examination was of the utmost importance.

Sir CHARLES BALLANCE said that he had not himself a great deal to say, but he hoped that what he did say would encourage others to participate in the discussion and give their experiences.

The President had pointed out that there were certain cases which should not be operated upon, and with that all the Members would agree; but in the evolution of every operation in surgery the same could be said, and the operations had not been wrongly done deliberately, but because the surgeons felt it was their duty to operate.

With regard to suppurative labyrinthitis, he thought the operation Sir William Milligan had described was the one which should be done. In the presence of supuration, in all parts of the body, there must be drainage. Without opening the capsule of the labyrinth and freely draining that cavity, pus in the labyrinth could not escape, and the patient would certainly die.

Concerning the method of operating, each probably had his own little manœuvre, designed to prevent damage to important structures; he thought the most important thing was to have a very free area of bone exposed behind the facial canal and the prominence of the horizontal semicircular canal. Then the rest of the operation became comparatively easy.

He well remembered the operations of Jansen, as he was in Berlin in 1900, and saw that operator deal with some of his cases. He was a skilful surgeon, and one whose dexterity was to be admired, but he (Sir Charles) would rather have Sir William Milligan operate on himself, as the quickness of Jansen gave a feeling of great uneasiness lest some disaster should occur. This was an operation which should be done with great care and deliberation, and time should not be considered.

With regard to the non-suppurative cases, that part of the paper was of great interest, and he thought it would come to be looked upon as one of the foundation papers on the subject.

The first case on which he tried to relieve Ménière's symptom by operation was that of an engine-driver, who drove one of the long-distance expresses to the North. He used to have these attacks while on the foot-plate, and during them his fireman placed him on the coal stack and drove the train himself until his mate recovered. As the man would have lost his work if the officials had come to know of his disability, Sir Charles acquiesced in the man's request that the failing should be kept secret, but insisted on his entering St. Thomas's Hospital that very day. He was in the hospital six to eight weeks, and the speaker was waiting to see him in an attack, but during all that time he never had one. He went out afterwards, and had not since been traced.

He agreed that in many of these cases there was toxic neuritis, and in the majority there was no visible change in the internal ear. The toxæmia affected the nerve, the nerve-endings or the nerve-cells of origin in the brain. He questioned whether, with these dreadful symptoms, the patient could live a happy life. Sir William talked of a bone operation, and Sir Charles suggested that in some of these cases the nerve could be divided behind the internal auditory foramen. Such an operation was much easier, and the wound healed quickly. It was less likely to be followed by infection than an operation on the internal ear—he was speaking of non-suppurative cases. It was very easy to expose the cerebellum and press it backwards by means of dry marine sponges. These sponges were not used now-a-days, but in all his experiments on animals Sir David Ferrier used them for displacing the brain from the area in which he was operating, and as the sponges expanded they shifted the brain tissue.

When the nerves coming out of the internal auditory meatus were exposed, a probe could be passed between the facial and auditory nerves. A downward cut would divide the auditory nerve.

He agreed with Sir William that in suppurative disease it was not always necessary to take away the capsule of the cochlea, for in some cases drainage posteriorly alone was sufficient, i.e., posterior to the facial canal.

Dr. ALBERT A. GRAY said it must have occurred to members how rare cases of Ménière's syndrome were in children of less than 12 years of age.

With regard to Sir William Milligan's theory as to a toxic condition being present, as distinct from the idea of there being alterations in pressure in the labyrinth, Sir William said he did not notice any excess of pressure in the labyrinth when he was operating. But he (Dr. Gray) thought the causation of Ménière's symptoms was not so much a question of whether there was high or low intra-labyrinthine pressure, but rather of a sudden change from one degree of pressure to another. In operating one would not expect to ascertain this. It was so easy to bring on giddiness by merely stooping down, and it was not necessary to invoke toxæmia. Compensation for alterations of pressure in the labyrinth was by means of a very fine tube, and a sudden contraction or dilatation of the auditory artery would suffice to cause increased pressure on the terminations of the nerves in the vestibule and semicircular canals. He agreed there must be many toxic cases but this question of alterations in pressure must be also considered, and that was probably the reason why the condition occurred so often in people who were overstrained and overworked. Those were people who had lost vasomotor stability.

Mr. SYDNEY SCOTT (President) (in further remarks) said Ménière's original paper accurately recorded the various types of vertigo which they met with now, and which Sir William Milligan had described. In investigating cases of recurrent vertigo, he had been struck by the extraordinary prevalence of catarrh in the middle ear. He felt convinced that many of the cases exhibiting Ménière's symptoms were due to sudden alterations in pressure of the labyrinth caused by changes in the middle ear. He recalled seeing two patients who were under observation at the same time, both sent to have operations done on the labyrinth, because they had recurrent incapacitating vertigo, associated with suppuration of the middle ear. A radical operation had already been done on the suppurating ear, but the labyrinth had not been touched. Labyrinth tests yielded no abnormal reactions on either side; there was no fistula symptom, therefore he (the speaker) did not feel justified in opening and destroying the labyrinth on the side of the suppurating disease. On examining the so-called normal ear of one of these patients during an attack of vertigo, he found the drum membrane became diffusely, brilliantly red, and on inflating the tympanum through a Eustachian catheter, the attack ceased at once. The patient was taught to self-inflate the normal ear, and the attacks were cut short by this proceeding, becoming less and less frequent. The second patient noticed a little noise in the good ear before an attack came on. She too responded to treatment, like the first patient, when the good ear was regularly inflated. He had seen the same condition in a patient whose labyrinth had been surgically destroyed nineteen years previously, because she had again become subject to attacks of giddiness which ceased so long as she kept her good ear free from Eustachian obstruction.

He found that in eighty-five out of his first hundred cases of vertigo, there was *obvious* evidence of catarrh and Eustachian obstruction at the time of examination. He had reported to his colleagues at the National Hospital that he could not find any abnormal response to the labyrinth tests to explain the vertigo in these patients, but that there was obvious middle-ear catarrh and post-nasal catarrh, and that those affections should be treated. When that was done the vertigo disappeared. Recurrence of vertigo in these cases was associated with recurrence of the Eustachian obstruction or inefficiency or disregard of treatment.

The remaining cases included certain anomalous conditions, nervous diseases or unproved cases. He agreed that the group justifying destruction of the labyrinth would be very small.

Dr. P. WATSON-WILLIAMS said he had felt there was a group of cases which had symptoms bringing them into the pseudo-Ménière group, unassociated with gross disease in the ear, but which were essentially catarrhal in origin, and probably due to a "toxic neurosis," or else to reflex vascular changes, perhaps both. He referred to one patient with vertigo as a case in point. He was liable to fall if not supported, and had considerable nerve deafness, and could not continue in his business. After the corresponding sphenoidal sinus had been drained, there was marked relief. The symptoms, however, recurred, and further exploration showed the patient had definite infective conditions in the posterior ethmoidal cells of the same side. From the time they were opened vertigo had almost ceased to trouble him.

Sir William Milligan had drawn attention to the relatively acute labyrinthitis as compared with chronic cases, which caused a definite infection of the labyrinthine system. Perhaps one of the factors was progressive absorption of the salts of the bone in chronic suppurative processes. In dental conditions, in which the apices of the dental fangs became absorbed by septic processes, it went on so slowly that the patient was usually unaware of anything wrong. It was also seen in chronic suppurative conditions of the nasal sinuses. In opening the maxillary antra, the outer meatal wall was often found to be of only paper-like consistence, no doubt due to the chronic absorption of the bone salts. That was one reason why chronic suppuration caused purulent infection, through a fistula, whereas the acute suppurations were more likely to be serous, as the hard layer of bone tended to protect the semicircular canal from penetration by pyogenic organisms.

Mr. E. MUSGRAVE WOODMAN confirmed the President's remarks as to the necessity of eliminating simple conditions before considering operative treatment. Three years ago he saw a man, by occupation a tax-collector, who was so incapacitated from duty by attacks of tinnitus and vertigo that he was likely to be pensioned off. He was found to have a tight blockage in his right Eustachian tube. A fine bougie was passed through the tube, and it was tightly gripped at the isthmus. In a month it was repeated, and this time the bougie passed fairly freely. Last week the man came complaining of a little deafness in the other ear, and he volunteered the statement that since the two applications of the bougie there had been no further attacks of giddiness.

Recently he (Mr. Woodman) had investigated a few cases of Bell's paralysis, and in a certain proportion of the cases there was definite evidence of the auditory nerve and the vestibular nerve being affected, but not to the same extent as the facial nerve. Some showed diminished labyrinthine reaction and often some tinnitus, which had passed unnoticed in the presence of the more gross lesion of the facial nerve.

The simple conditions having been eliminated, there remained some cases of non-suppurative labyrinthine trouble which must be operated upon, and in regard to these Members felt much indebtedness to Sir William for his paper. His own experience was limited to one case. Stimulated by Mr. Mollison's paper a year ago, he operated upon a nurse who had attacks of vertigo and tinnitus, which had caused her dismissal from the staff of the nursing home. During the attacks she vomited and was much collapsed. He opened the mastoid and the external semicircular canal, thus obtaining free drainage, and then shut it up. For two or three weeks she had bad attacks of giddiness and sickness, but it cleared up at the end of three months, and she was now earning her living.

Mr. E. D. D. DAVIS said that he had at present two people at hospital suffering from attacks of vertigo. One was a nurse who had been incapacitated twelve months on account of violent vertigo, with nystagmus only during the attacks. A neurologist asked the speaker why he did not open her labyrinth. Occasionally there was slight loss of hearing in one ear. She was a highly-strung, nervous woman. Lumbar puncture was done and she was now quite well, and before leaving the hospital she asked to be lumbar-punctured again. The fluid was normal, but there was a little increase in pressure.

The other patient was a doctor's widow, who had had a good deal of anxiety and worked hard. She came with intense vertigo and sickness with nystagmus, and had

also lost confidence in herself. He examined her twice and concluded she had commencing otosclerosis; the Eustachian tube was patent and normal. A Eustachian bougie was passed, and since then she had not had another attack.

Another case was that of a man who was very deaf from secondary sclerosis following suppuration, with intolerable tinnitus. He wrote and asked Mr. Lake what his results had been from opening the labyrinth for tinnitus; Mr. Lake replied that his results were bad and that he had given up the operation.

With regard to division of the auditory nerve, he (the speaker) thought there had been ten cases of this. It must be remembered that there was a fairly large auditory artery accompanying the nerve, and that there might be severe hæmorrhage.

Mr. G. J. JENKINS said that it could be said that the operation of superior and inferior labyrinthotomy was a valuable procedure in cases which were suitable for operation.

With regard to the amount of hearing there might be after doing inferior-superior labyrinthotomy, he had a case which gave very much anxiety. The patient, a female, seemed to hear conversational voice at 3 or 4 ft. in that ear, and the difficulty was to eliminate the sound ear in the test. With the Bárány apparatus in the good ear she could not hear on the side operated on at all.

The difficulty in regard to the non-suppurative cases was the lack of a definite pathology; it was difficult to get specimens of the class of case referred to as pseudo-Ménière's disease. There was a marked difference in the flow of the labyrinthine fluid in a case of Ménière's or pseudo-Ménière's disease as compared with that in certain other conditions in which he had opened the labyrinth. His difficulty had been what to do with the cases in which there was considerable hearing, and yet the vertigo symptoms persisted even after inflation for long periods, and there were definite signs of involvement of the posterior part of the labyrinth.

Mr. L. COLLEDGE said that if matters were desperate, the question arose as to what operation should be done when any catarrhal condition of the middle ear had been excluded. Sir William said he had not observed evidence of increased pressure in the internal ear. But there might be increased pressure in the posterior fossa. The case mentioned by Mr. E. D. D. Davis in which lumbar puncture relieved the symptoms suggested that increased intracranial pressure might lead to symptoms. A young woman patient of his (the speaker's) had had a radical mastoid operation done at another hospital. It was quite healed when he saw her, but she had these symptoms very persistently. He trephined her over the cerebellum, and when the dura was opened, the cerebellum bulged into the wound, and on its being pushed aside, there was a very free flow of cerebro-spinal fluid. The symptoms disappeared from that time. He suggested that in some cases it would be simpler and safer to do a cerebellar decompression than to open the labyrinth. This also opened the way to dividing the auditory nerve if it was later desired to do so.

Dr. H. SMURTHWAITE said that last year he had a case with marked dizziness and vomiting. In the left ear there was slight deafness, the drum was indrawn, and there was only slight loss of bone conduction. The patient stayed in bed three days, and two days after he got up he had another attack. He could not pass a catheter through the patient's left nostril, as there was a deviated septum. The septum was put in order, and a fortnight after that procedure he had yet another attack, and a very violent attack of vomiting, and was in bed three or four days. This went on for three or four weeks, with periods of health. After catheterization he improved and during the last six months his attacks had not been renewed.

Another case he had was simpler. A man complained of vomiting, and all that he (Dr. Smurthwaite) could find amiss was a small concretion of hard wax which was pressing on the drum. The wax was softened and removed with hard skin, and there were no further attacks. He thought there was a reflex action on the stapedius muscle in that case, causing increased pressure.

Dr. W. S. SYME said he thought it was the experience of all Members of the Section that in nasal conditions, both purulent and obstructive, vertigo of all degrees was



encountered, and in many cases it was no doubt due, as the President said, to middle ear conditions. He always attended to the nasal conditions before regarding a case as a serious one requiring more severe operative measures. In only two non-suppurative cases had he felt justified in recommending the opening of the labyrinth. Once he did lumbar puncture in a non-suppurative condition, but that patient disappeared.

In acute suppurative conditions of the middle ear with apparent labyrinthine involvement, he approached the case more warily than when the suppuration was chronic. In acute conditions he first tried thorough rest to hearing as well as bodily rest.

In operating on the labyrinth he tried to get as much room behind as possible, by removing the posterior wall of the antrum, well beneath the sinus.

Sir WILLIAM MILLIGAN (replying on the discussion) said that since arriving at the meeting he had received a letter from Mr. J. S. Fraser, of Edinburgh, in which he said: "I do not think that you are looking at a hæmorrhage into the internal meatus; I believe that the cellular exudate surrounding the seventh nerve, and, to a less extent, the eighth nerve, is composed of white cells, and that the condition is really one of neuritis, and not hæmorrhage."

On the general question, a number of the remarks made had been somewhat wide of the mark, as he was dealing with the class of case in which members might conclude that every precaution was taken to eliminate the existence of accessory sinus disease, tubal catarrh, &c.

With regard to the remarks of Sir Charles Ballance, it was Jansen who stimulated him (the speaker) to take a special interest in the subject. He saw Jansen operate in Berlin many years ago, and he came away with but a poor impression of that authority's operating powers. In a way, he was a brilliant surgeon, but his operations were done far too rapidly, and great rapidity of operation in a region like the labyrinth was too dangerous. During the operation the anæsthetist constantly said that the facial nerve was twitching; and the instant response was "It does not matter!" Before the patient left the operating table the facial nerve was completely paralysed!

Sir Charles Ballance suggested that, if operation was going to be done, approach was more easily made through the posterior fossa. With that he (Sir William Milligan) disagreed; he had tried it on two occasions. No doubt Sir Charles, as an expert brain surgeon, was more accustomed to operate in that way. He (the speaker) did use sterilized marine sponges, at the suggestion of Sir David Ferrier, and his first operation took him three and a half hours. The facial nerve was injured though there was full recovery from the vertigo. In that case, however, he did the operation in a way no surgeon would do it now—he removed a flap from the side of the head, then put in marine sponges, getting brain and meninges gradually pressed aside. He determined he would not undertake such a tedious procedure again. His second operation was in the posterior fossa, and that also he found a very difficult one; it was difficult to get the cerebellum back with the aid of the sponges, and during the time the patient was under the anæsthetic he lost a good deal of cerebro-spinal fluid, and was much prostrated afterwards. He managed to divide the nerve, and the result was good. It was much easier in his opinion to operate through the outer labyrinthine wall.

He had tried lumbar puncture time after time in these cases; it was originally suggested by Babinski. He had never found that it gave any permanent relief.

With regard to decompression over the posterior fossa, Quick had published a paper very strongly advocating it, remarking that in a large proportion of cases there was an increase of fluid in the fossa. He (Sir William Milligan) had tried decompression years ago, but gave it up.

He admitted that if tubal catarrh was present some vertiginous symptoms might occur; but he was not dealing with that type of case, he was speaking of cases in which there was neither evidence of tubal catarrh nor of a middle-ear lesion. He would not think of proceeding to operation until every other source of trouble had been eliminated.

Dr. Gray advanced the important suggestion that such cases might be intermittent in regard to pressure, and gave this as a reason why at the operation no increased tension of fluid was noted. But if that were so, why was the condition unilateral?



He had never seen a bilateral case. He was always careful to ascertain the patient's blood-pressure, and if it were high, treatment was first directed to reducing it; similarly, if the patient had a catarrhal condition, that was treated first, and if the symptoms subsided the internal ear was not touched.

In answer to Dr. Watson-Williams, he (Sir William) had always tried to eliminate sinus disease, both by careful examination and by using X-rays.

In reference to the question of acute serous labyrinthitis, which was raised by Dr. Syme, he did not think those cases needed operation; he had never operated upon an acute serous case, and the purulent cases upon which he had operated were either cases of acute purulent labyrinthitis, or were chronic and secondary to chronic purulent middle-ear disease. The reason why the bone was affected was, that it was a slow, septic process, causing first erosion, then a fistula, then definite infection.

He wished to emphasize the fact that every possible care was taken in selecting cases for operation; he looked upon the operation he had described for non-septic disease as in the nature of a somewhat desperate remedy, justifiable only in the cases in which the patient had been rendered *hors de combat*, and was quite unable to continue his avocation. And, as was only just to the patient, operation was not considered until he had had the benefit of a long course of medical treatment. So far, he had had no fatalities. The operation was not difficult if the field was sufficiently exposed; but one had to proceed very carefully, and to take special care not to damage the facial nerve. There was only a limited field for the operation, but it gave relief in a class of cases which medicine failed in curing, and the operation appealed to him as a scientific one. The chief disadvantage was that the pathology could not yet be stated in black and white. Even if one had a specimen from a case the difficulties of elucidating the pathology from one temporal bone would be very great. But it was something to think about, and Members could ponder as to whether, in certain clinical conditions, it was worth while to take the risks of the operation. The results were quite satisfactory if the cases for operation were carefully selected.

### A Case of Cerebral Abscess.

By T. B. JOBSON, M.D.

MRS. E. F., aged 26, admitted to the Royal Surrey County Hospital on November 7, 1923. History of discharge from right ear since childhood. Seven days previous to admission, discharge stopped and she had pain, headache, vomiting and giddiness.

On admission: Temperature 102° F., pain, tenderness and œdema over right mastoid. All reflexes normal. Grips equal. No incoördination. Slight spontaneous nystagmus to left. Radical mastoid operation the same evening. Temporal bone sclerotic. Antrum full of pus—cavity lined with dirty white cholesteatomatous membrane. Lateral sinus exposed and found healthy. Just above tegmen antri, a small extra-dural abscess was found. Dura covered with granulations but not sloughy.

After this operation she made apparently an excellent recovery. But on November 18 (eleven days after) she complained of headache on right side. Temperature 97·8° F., pulse 86, and she vomited once. Knee-jerks, ankle, wrist and elbow reflexes increased on both sides. Ankle clonus + both sides. Kernig and Babinski negative. No head retraction. No paralysis. Eyes normal. Mental condition mildly excitable.

November 19: Vomited once. Pain in back as well as head. Lumbar puncture: fluid clear—under pressure—sterile—leucocytes 3 per cm.

Blood count: Leucocytes 25,000; polymorphs 93 per cent.

November 20: Temperature rose to 100·8° F. Pulse 100. Condition otherwise the same. No vomiting.

November 21: Vomited twice in the night, "feels dazed." Temperature 98'2° F. Pulse 70. Nystagmus to left.

Ophthalmic examination (Dr. E. J. Smythe): "Edges of discs are 'woolly'—suggesting early optic neuritis."

On the combined evidence a diagnosis of cerebral abscess was made.

Operation: Mastoid incision reopened, flap of scalp turned down, bone over tegmen antri removed 1½ in. by 1 in. At the second incision of the dura (which looked healthy and pulsated) an abscess of foetid pus was struck at a depth of 1 in., size about a hen's egg. Two rubber tube drains inserted. The pus yielded a mixed growth: (1) a short streptococcus; (2) pneumococci; (3) *Bacillus coli*.

November 22: Much better. Pulse 84. Temperature 100°. Pupils dilated and do not react to light—? cause. This eye condition lasted four days.

November 25: Abscess track ionized with zinc. This first ionization caused no diminution of the discharge.

December 2: Second ionization of abscess track and mastoid cavity. Rubber tubes left out, small gauze wick inserted. After this there was extremely little discharge, although the dressings were only changed twice a week. A third ionization was given on December 9. A final ionization was given on December 16, when the wound was clean and free from odour. Gauze drain was left out and the wound covered with a sterile dressing after painting the skin edges with iodine. No further dressings were done. The only untoward incident in her recovery was an epileptiform fit on December 18, when she was unconscious for a couple of minutes. No localizing contractures were observed, but subsequently she complained of some pain in her left leg. The interesting points about the case are:—

(1) The diagnosis of cerebral abscess was made on the temporal headache, vomiting, leucocytosis, and early optic neuritis. There were no localizing paralyses, the pulse was not slowed until the day the abscess was opened.

(2) Treatment of the abscess cavity by zinc ionization appeared to cut short the suppurating process. The wound was clean on December 16, less than four weeks after the opening of the abscess, although no antiseptics were used in the dressings.

Dr. FRIEL, referring to the supposed danger in treatment where electricity was used for ionization, said that if the patient was ionized in a theatre with a concrete or similar composition floor, and the source of the current was the electric light mains, it was important to insulate the table on which the patient lay, if it were of metal, by placing rubber or linoleum under the feet. It would then be impossible to introduce into the patient more current than was indicated by the milliamperemeter.

### **A Specimen of a Chronic Abscess of the Pons arising from Middle Ear Suppuration.**

By E. D. D. DAVIS, F.R.C.S.

A MAN, aged 35, was admitted to hospital on September 6, 1923, for headache and nystagmus, and with a diagnosis of cerebellar abscess arising from mastoid suppuration.

A left radical mastoid operation had been performed 2½ years before (April 1, 1921), at another London hospital, for otorrhoea of ten years' duration, pain for one week and incomplete left peripheral facial paralysis; no nystagmus, but

considerable deafness. A sclerosed dense mastoid with small crack-like antrum, with granulation tissue over the facial and external semicircular canals. No exposure of the lateral sinus, and patient made an uninterrupted recovery with no temperature, and left the hospital after 14 days.

When admitted on September 6, 1923, patient complained of attacks of headache every two or three months of one to fourteen days' duration following his mastoid operation. During the headache, which he localized to the left parietal eminence, he occasionally vomited with no apparent reason or warning, and five weeks previous to admission he often saw double on looking to the left. In addition, he complained of giddiness and loss of memory.

On examination by Dr. Adie, pulse was 76 to 84; temperature, 98° F.; pupils and optic discs normal; marked rotatory nystagmus on looking to the left; this has the character of labyrinthine nystagmus. Numbness on left side of face and tongue of eighteen months' duration; weakness of left side of face, particularly of the orbicularis oris; reflexes increased on left side; X-ray of skull and Wassermann negative; blood count 10,300 leucocytes per c.mm. Cerebrospinal fluid: cells, 390 per c.mm; lymphocytes, 84.5 per cent.; polymorphonuclears, 15.5 per cent.; excess of globulin; albumin, 2 per cent.; no tubercle bacilli or other organisms; culture sterile; mastoid satisfactory, no bone disease or suppuration; slight tubal discharge. Monochord, tuning-fork and voice can be heard by left ear. Indefinite labyrinthine reaction to irrigation. No fistula phenomenon.

*Diagnosis.*—Circumscribed labyrinthitis, and patient was discharged, improved by rest, at the end of fourteen days. He was readmitted a fortnight later with temperature 101° F., increased headache and slightly drowsy. No labyrinthine reaction to irrigation, but hearing as above.

*October 18, 1923.*—Mastoid opened up. Middle ear contained débris, and middle fossa dura had been exposed in the roof of the antrum. Small area of granulation tissue over the facial canal. Examination with a magnifying-glass showed no fistula of the external semicircular canal or of the promontory. Very dense, hard bone; and access to the cerebellum in front of lateral sinus was difficult.

The posterior fossa was trephined behind the lateral sinus and the cerebellum thoroughly explored with forceps and finger with a negative result. Wound closed without drainage, and healed by first intention and recovery was uneventful.

*December 18, 1923.*—Patient returned with definite increase of facial paralysis. Orbicularis palpebrarum completely paralysed.

*December 27, 1923.*—Patient readmitted with right hemiplegia of one week's duration, commencing with paralysis of right arm; drowsy, incontinence. Paralysis of left sixth cranial nerve.

Nystagmus as before.

Left temporo-sphenoidal abscess diagnosed, and middle fossa and temporo-sphenoidal lobe explored with negative result. Condition unchanged the day after operation and patient smoked cigarettes. Coma and death four days after operation.

*Post-mortem.*—No meningitis. Posterior fossa and exploration of cerebellum beautifully healed with no signs of inflammation, and almost complete absence of trauma. Temporo-sphenoidal lobe, except for local hæmorrhage, in good condition.

Left half of pons was prominent and on section found to contain an encapsulated abscess,  $\frac{3}{8}$  by  $\frac{1}{2}$  in., on a level with the middle cerebellar peduncle. The

seventh and eighth cranial nerves were cedematous, fused together, and the internal auditory meatus contained pale granulation tissue. A dissection of the vestibule and semicircular canals showed them to be absolutely normal and there was no fistula of the labyrinth. The facial canal contained granulation tissue.

The interesting points of this case are :—

- (1) The unusual position of the abscess and the difficulty of diagnosis.
- (2) The probable path of infection along the facial nerve.
- (3) The slight amount of damage done by a very thorough exploration of the cerebellum.
- (4) The avoidance of drainage following a negative exploration operation.

#### DISCUSSION.

Dr. ALBERT A. GRAY asked whether hiccough was a symptom in this case. Many years ago he had had a case of a destructive lesion in the same region, and there was very severe and persistent hiccough lasting six weeks.

Sir WILLIAM MILLIGAN said he had never seen an abscess in the pons after middle-ear disease; he had wondered whether in this case there was a tuberculous deposit in the pons, which had broken down, so forming an abscess.

Mr. SYDNEY SCOTT (President) said Mr. Davis's case was particularly interesting because it was unusual to meet with abscess in, or in close proximity to the pons, even secondary to internal ear suppuration. He had seen a patient of the late Dr. Frederick Batten who had a mass of tubercle in the pons, and he asked Mr. Davis whether there was likely to be tubercle in his case, as Sir William Milligan had suggested was possible.

Mr. E. D. D. DAVIS (in reply) said that there was no hiccough in this case. It was just like other otogenic brain abscesses. In this patient there was no tubercle elsewhere. Tubercular abscess in the pons was always secondary to tubercle elsewhere. The organism was a pneumococcus, and there was no evidence of tuberculosis on section of the capsule of the abscess.

### Case in which the Chorda Tympani (?) is Easily Visible.

By W. M. MOLLISON, M.Ch.

MARY K., aged 34, attended Guy's on account of wax in the left ear. On examination of the right ear a strand is seen running from the posterior margin of the membrane towards the short process of the malleus, probably the chorda tympani.

#### DISCUSSION.

Mr. SYDNEY SCOTT (President) said he had not seen a similar case. The only means that occurred to him of settling the matter was by section.

Dr. H. SMURTHWAITE said that this patient had evidently had suppurative otitis media. There was a large calcareous deposit in the lower portion of the drum extending from side to side. He doubted whether what one saw as a grey streak just below Shrapnell's membrane was the chorda tympani.

Sir WILLIAM MILLIGAN said he did not think there was a calcareous deposit in this case, but there was evidence of old catarrh on both sides. Mr. Cheate had shown a similar case before the Section some years ago.

**A Case of Extreme Unilateral Deafness to Sounds by Air  
but with Good Bone Conduction in the Affected Ear.**

By A. R. FRIEL, M.D.

CAPT. A. J., aged 38, was in the artillery for some years and had no noticeable deafness. Six years ago he was buried by a shell explosion and was unconscious for three days. About five days later, when in hospital, he noticed he was deaf in the left ear.

Condition in 1922: Hearing, right ear, conversational voice 19 ft., whisper 15 ft. Left ear, nil by air. The tuning-fork on the right mastoid referred to the left ear. Watch heard on left mastoid. Cochlear palpebral reflex absent on left side, present on right. Syringing left ear with cold water causes nystagmus: and clockwise rotation produces nystagmus lasting 25 seconds. The right and left drums are intact.

Dr. FRIEL explained that he brought this case to show that when a pensioner complained of deafness he should not be disbelieved because he had good bone conduction.

## Section of Otology.

President—Mr. SYDNEY SCOTT, M.S.

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### Case of Radical Mastoid Operation performed before Plastic Methods were generally adopted.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

MRS. C. was operated on by me for chronic suppurative otitis media with headache and vertiginous symptoms at a time when plastic methods were not being generally adopted. The meatus was simply slit and a drainage-tube was retained between the mastoid wound and the meatus. At the present time the cavity is seen to have as smooth and shiny a lining as a Thiersch graft would produce.

### Case of Radical Mastoid Operation with Temporo-sphenoidal Abscess in which no Plastic Measures were practised.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

MISS L. H. was operated on by me for temporo-sphenoidal abscess a number of years ago. Drainage of the abscess was kept up through the post-aural opening and no "plastic" of any kind was practised. The radical mastoid cavity is now quite smooth and apart from an occasional accumulation of cerumen gives no trouble.

These cases are shown to illustrate the possibilities referred to by Dr. Dan McKenzie.<sup>1</sup>

#### DISCUSSION.

Dr. KERR LOVE said he thought there was still room for this type of operation by which the flaps were left alone, but the room was a comparatively narrow one. In these cases one was tied down too much to cutting a definite flap, because in many of the cases in which the inner end of the posterior soft parts had gone the soft parts contracted and left a good cavity, as Sir James Dundas-Grant had shown.

Dr. DAN MCKENZIE said that when he showed his case of a similar kind there was a justifiable condemnation expressed of the operation being done in every case. He agreed it was applicable only to a small number of cases. The object of showing such cases was to encourage a more flexible outlook on mastoid problems; sometimes it was good to be jostled out of a groove.

<sup>1</sup> See *Proceedings*, 1923-24, xvii (Sect. Otol.), pp. 37, 38.

## 74 Dundas-Grant: *Two Cases Illustrating Trans-tubal Irrigation*

Mr. G. J. JENKINS agreed with what had been said, and remarked that recently he had been doing a different operation on cases of chronic middle-ear suppuration. He left the posterior wall intact and left a ring of bone. He removed the incus from behind, and by a flap, filled up the cavity in the mastoid. Cases so treated did very well when there was a certain definite lesion of the middle ear. One class of case where there was a little membrane left between the posterior wall and the perforation did very well under the operation referred to; healing could be almost guaranteed in such cases. Later on, he would show some of the cases treated in this manner.

Sir JAMES DUNDAS-GRANT said that the first of the mastoid cases occurred in 1898; the second, twenty years ago.

### Two Cases illustrating Trans-tubal Irrigation.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

(a) MRS. W., aged 56, was first seen in August, 1922, on account of discharge from the right ear of twenty-three years' duration. The special point was the extreme tenacity of the discharge, which was almost jelly-like in consistence. After inflation, intra-tympanic syringing and suction, it could only be extracted by means of fine forceps passed into the tympanum through the perforation. It resisted all treatment until copious irrigation with soda lotion through the Eustachian catheter was practised, when it gradually disappeared.

(b) Miss H., aged 48, was first seen in November, 1923, on account of deafness and discharge from the left ear since the occurrence of scarlet fever in childhood, and on account of neuralgia of about two years' duration. A small intra-tympanic polypus and some granulations were removed. As progress was slow I syringed an alkaline solution through the Eustachian catheter and thereby forced a long thin polypus out of the perforation. Apart from this procedure the polypus might not have revealed itself, and the case is brought forward to illustrate a use for Eustachian irrigation which is new to me.

### DISCUSSION.

Sir WILLIAM MILLIGAN said his difficulty with trans-tubal irrigation had always been that, when he had tried to flush out the middle ear with a Eustachian catheter, he had been disappointed with the small amount of fluid which had got into the middle ear. He suggested another method, namely, filling the meatus with the fluid with which it was intended to wash out the middle ear, using Politzer's bag, and driving it through from the other direction. This seemed to accomplish a better flushing. He asked whether Sir James Dundas-Grant used any particular catheter for these cases; did he use compressed air, or drive up the fluid with an ordinary bag?

Dr. DAN MCKENZIE asked whether the exhibitor had ever encountered serious after-effects from the trans-tubal method. At the Central London Throat and Ear Hospital the method was tried years ago and then abandoned, because it was said to produce so many acute mastoids. In Vienna, however, it was still the routine method. If Sir James could give some assurance as to its safety, he would not mind trying it again. The feeling of most otologists was against syringing of any kind; one did not quite know where the fluid was going to.

Mr. F. C. CLEMINSON said the note stated there was a polypus forced out from the meatus by the irrigation, and he asked whether in the method suggested by Sir William Milligan there would not be a danger of forcing a polypus into the Eustachian tube.

Mr. H. LAWSON WHALE said he thought the danger could be avoided by using an ordinary mechanical pump. There was a valve in such a pump, so that it would either suck, or blow, or suck and blow in rapid alternation, and the strength of the blast could be nicely regulated.

Mr. J. F. O'MALLEY said he had found the method somewhat inconvenient on account of the quantity of fluid that got through the Eustachian tube, also because some of the fluid trickled into the throat and set up coughing. In preparing to treat a condition in the middle-ear cavity, in which there was a definite localized focus, with perforation of the membrane, he used the Eustachian catheter and tried to clear the Eustachian tube from the inner opening by inflation with air, then he cleared the tympanic cavity itself. He then inserted a few drops of 5 per cent. nitrate of silver through the external meatus, and used the Politzer bag, exerting a certain amount of pressure to drive it into all the crevices of the middle ear and through the Eustachian tube into the throat. In a certain number of cases that procedure had been very effective.

Mr. H. J. BANKS-DAVIS said the amount of fluid liable to get into the patient's throat depended on the distance the tube passed into the Eustachian tube. If a long enough tube were used he thought there would be no danger. A Weber-Liel flexible catheter passed through the ordinary silver or vulcanite catheter answered the purpose well.

Sir JAMES DUNDAS-GRANT (in reply) said that the catheter he used was a silver one, with a rather long beak. Through it he pushed the Weber-Liel tympanic tube, which went through the isthmus and could pass right up into the tympanic cavity. With an ordinary ear syringe he injected through this tube a weak solution of bicarbonate of soda, thereby forcing a blob of tenacious mucus out through the external meatus. He felt that nothing would dislodge the mucus unless it were got at from behind. There must of course be a large perforation; he would not care to use the method if there were a small perforation. Had he known a polypus was present he would have hesitated more before exerting the pressure he employed. If the method mentioned by Sir William Milligan were successful, the issue would be satisfactory, but in suppurative cases he would hesitate to use it, for fear of driving fluid forcibly into the antrum. Where there was dehiscence it would be dangerous on the whole—more dangerous than the trans-tubal method. In answer to Mr. Whale, in neither of these cases was suction of use. A class of case must be recognized in which a persistence of discharge was due, either to disease located towards the tympanic end of the Eustachian tube, or at a point where there was a thick tenacious secretion from post-nasal catarrh. The cure was actual.

Sir WILLIAM MILLIGAN (in further comment) said that many years ago, the late Professor Young himself carried out experiments on the cadaver, incising the membrane, making a very small perforation in it in some subjects, and a large one in others; then they carried out intratympanic injections of methylene blue. In the large perforation cases they found no fluid in the antrum or mastoid cells, whereas in the others it was almost invariably found. Therefore, if the method mentioned by Sir James were adopted clinically, it should only be used where there was very free drainage from the middle ear.

### Case of Vertigo of Central Origin.

By RICHARD LAKE, F.R.C.S.

*February 16, 1922:* Patient, an old friend of mine, 59 years of age, active and temperate, had spent a quiet day on Sunday, January 29, 1922, and had commenced to eat his supper, in fact had taken some soup, when suddenly, as he described it, everything began to rotate in the direction from right to left; he sat with his head on his hands and had marked nystagmus. Fairly



violent vomiting occurred within half an hour. He slept soundly all night ; in the morning he awoke and drank some milk, which he promptly vomited ; he then slept again until the evening, when he realized that he was completely deaf in the right ear. His recovery was uninterrupted. He was seen by Dr. Morgan, of Seaford, who found his blood-pressure 100, and never higher.

For a fortnight or so music was very distressing, as the ears heard different notes.

On examination I found his blood-pressure 110. Perception of loud voice in right ear ; the left had been a little dull of hearing for some time (shooting). Right ear : bone conduction, 11/25 ; air conduction, C to C5 ; monocard, 18/22 ; Bing positive ; Rinne negative.

Caloric test, 6 oz. of water at 60° F. ; violent nystagmus right to left.

The patient recovered gradually from his instability of gait and the hearing of the right ear improved to two inches for the voice.

August 20, 1922 : A second attack during the night. Objects move right to left. Diplopia. Complete deafness.

#### DISCUSSION.

Mr. A. R. TWEEDIE noted that Mr. Lake referred the origin of the vertigo to some central lesion, but he (the speaker) suggested that the report rather indicated a peripheral affection of the kinetic portion of the labyrinth, especially as the Rinne was negative.

As regarded the response to the caloric test, he would like incidentally to draw the attention of the Section to recent modifications with minimal stimuli, as suggested by Kobrak. The essential part of these consisted in the restriction of the amount of water to some 5 to 10 c.c., the use of a temperature of 15° C. and waiting thirty seconds as a necessary latent period before attempting to investigate the resulting nystagmus (*Journal of Laryngology and Rhinology*, 1923, p. 217). He (the speaker) had adopted this method and considered it was of increased value in this test.

Mr. SYDNEY SCOTT (President) pointed out that Rinne's test might be pseudo-negative in one ear, because bone conduction was transferred to the auditory nerve on the opposite side.

Sir JAMES DUNDAS-GRANT raised the question as to the quantity of water at 60° F., which was required to produce nystagmus in a normal case. Six ounces of such cold water seemed to be a large quantity to require, and to indicate lowered sensibility. 2½ oz. at about 75° should be sufficient in an average case.

Dr. DAN MCKENZIE said that one should be cautious of conclusions depending on the measurement of time in these tests.

Mr. LAKE (in reply) said that he did not test the patient's good ear. When he was doing a large number of tests, he had a standard for the sake of comparison, namely, 6 oz. of water at 60° F., run through the ear in 60 seconds.

His reason for stating that the origin in this case was central was that the deafness and vertigo diminished. And there was an exceptionally low blood-pressure, which was not so likely to cause thrombosis in the vestibule as in the cerebrum. In addition, the monocard showed very good hearing, 22 by ear, 18 by bone, and nearly 50 per cent. of tuning-fork bone conduction. Bing's test was positive, and he regarded Bing's as the most valuable of all the tests.

## Radical Mastoidectomy—Persistent Discharge—Healing by means of Massive Flap.

By E. A. PETERS, F.R.C.S.

Mrs. R., 36, underwent a radical operation in 1908. For many years she was treated by many methods. The persistent discharge appeared to come from a posterior cavity.

In 1921 I opened up the post-aural incision, removed the affected bone and filled up the cavity with a massive flap. The meatus is now of normal size and there is no discharge.

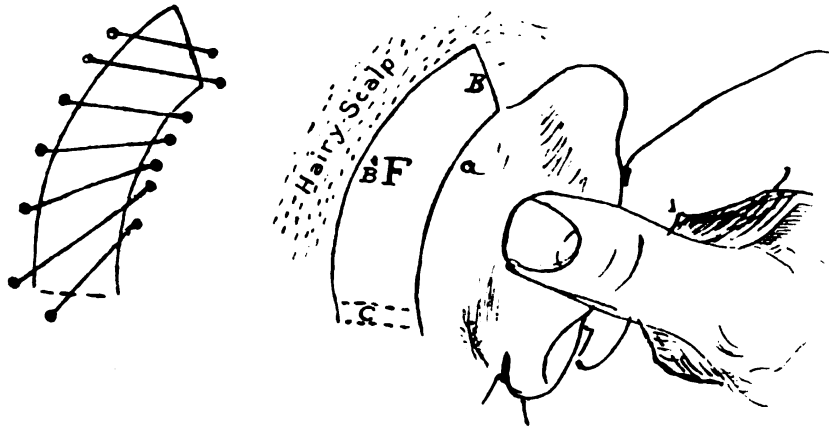
## A Thick Flap for the Partial Lining and Diminution of Too Large Mastoid Cavities.

By RICHARD LAKE, F.R.C.S., and E. A. PETERS, F.R.C.S.

THIS flap has been used by both of us in a fairly large number of cases and has given gratifying results.

It shortens the period of healing and very materially contracts the cavity, these large cavities being, as is well known, the cause of many of the troubles

Sutures



a Post-aural incision.  
B B' Flap-incision.  
C Incisions dividing skin only (intervening piece of skin removed).  
F Flap.

in after-treatment of mastoidectomies in which large amounts of bone have been removed.

*Description of Flap* (diagram shown).—A post-aural incision is made, close to the attachment of the auricle, dividing all tissues down to the bone. A second incision is made parallel with the former and beginning about  $\frac{1}{2}$  in. to  $\frac{1}{4}$  in. higher up.

A third incision joins them above. At the base a narrow strip of skin is removed between the two lower ends of incisions 1 and 2.

The flap after being separated from the subjacent bone is rotated upon itself. Should the auricle be treated as suggested by one of us for labyrinthine operations the rest is easier. The original posterior margin of the flap is attached to the inner end of the cartilaginous external meatus.

#### DISCUSSION.

Mr. F. HOLT DIGGLE said that in Birmingham, four years ago, he saw a similar flap used. The operator attached it to the bottom, and turned it in, leaving it open above; at the second operation he severed the attachment, and closed it behind. The operation was given up because hairs grew in the cavity.

Sir JAMES DUNDAS-GRANT said a somewhat similar flap was made by Witmack; it was described in the *Journal of Laryngology*. The purpose of it was to bring the tip of the flap over the Eustachian orifice in order to close it. To fix it there, a thread was passed up the Eustachian tube on a very thin bougie. The thread was attached to the tip of the tongue of flap and pulled back down to the Eustachian orifice.

Mr. G. J. JENKINS said he did not think there were many opportunities for using this flap, but it might be a successful procedure. Occasionally he had had to fill up a cavity behind and below the meatus, after a radical mastoid operation had been done two or three times. Recently he had had a case in which there was a very marked cavity behind and below the meatus which he filled up by cutting a periosteal flap and placing it in this cavity below the meatus, and allowing it to skin over in the ordinary way.

Mr. E. A. PETERS explained that the method was not only used for filling up the cavities, it was also very valuable in cases in which some chronic osteitis was in progress. The original operation was done in 1908, and in the succeeding years of suppuration most of the methods used for that condition were employed. It was three years ago that he carried out this procedure. The cavity was opened again, and patches of chronic osteitis removed. It was very unphysiological to cover bone with a thin layer of epithelium, and thin grafts were much worse than thick ones. Mr. Lake and he therefore worked up to the idea that it was better to use a solid flap. In this case the osteitis disappeared, and a normal condition of the meatal wall had evolved. It was important to take into account the condition of the membranous external meatus; a certain amount should be shaved off; if the whole of the membranous meatus was left, contraction was liable to follow. It was true that hair might appear in the site, but he had found that it soon dropped off. Sometimes a relaxation incision 1 in. behind the original had to be made. He felt sure that if others tried the method, they would not be dissatisfied with it.

#### A Pair of Forceps for Use in Tonsillar Dissection.

Shown by Mr. RICHARD LAKE, F.R.C.S.

#### DISCUSSION.

Mr. H. J. BANKS-DAVIS said he considered these sharp-toothed serrated forceps, of which there were several patterns on the market, were liable to tear out when traction was made on a friable tonsil. There was insufficient grip.

Dr. W. S. SYME did not agree with Mr. Banks-Davis. Getting a good grip on the tonsil was the important thing; one must go deeply into the tonsil between the pillars.

Dr. W. H. KELSON said much depended on the tonsil. If the tonsil were brittle and disintegrated he had found such forceps tear out, and those were just the cases in which one did not want that to happen. He had now adopted gland-holding forceps; these had no sharp teeth; they efficiently gripped the tonsil, and there was no tearing out.

**Case of Chronic Suppurative Otitis Media of "Attic" Type.  
Cessation of Discharge after Removal of Outer Attic  
Wall by Electrolysis.**

By T. B. JOBSON, M.D.

*April 4, 1921:* W. L., aged 25, discharge from right ear for nine years. No headache or giddiness. Hearing good. Membrane showed reddening of malleus with a perforation about 2 mm. wide in the anterior part of Shrapnell's membrane, through which a scanty brownish foetid discharge exuded.

Treated for three years by a variety of antiseptics: drops, powders, intratympanic injections, intratympanic irrigation, intratympanic irrigation plus ionization, &c.

He improved considerably, but there still remained a very slight discharge with foetor.

On April 3, 1923, the perforation was enlarged by electrolysis. This was repeated four times until the opening was 7 mm. wide. Tympanum now quite dry.

(The patient was not present at the meeting.)

DISCUSSION.

Mr. SYDNEY SCOTT (President) asked why electrolysis was used, instead of the ordinary method? Was it because the patient objected to the removal of the attic wall by operation, or was it for some other reason?

Dr. J. KERR LOVE said it was difficult to discuss such a case when one had not seen the patient, but he thought a more reasonable treatment would be the ordinary Stacke operation, the attic wall being removed, as done by Dr. Dan McKenzie in a later case. The treatment carried out in this case was not one which should be applied to the many cases which failed to yield to the attic syringe.

Sir JAMES DUNDAS-GRANT referred to a paper published many years ago by Schmiegelow on attic suppuration, recommending that ossiculectomy should always be carried out. If the hearing was bad and there were symptoms of blocking, the practice of that operation was right, but not in the presence of good hearing. Later, Dr. Bronner, of Bradford, recommended a set operation for attic suppuration. It consisted of the first steps of Stacke's operation, but with the incision well down over the front of the upper part of the attachment of the auricle. This was drawn down and the attic exposed by chiselling away the outer wall. He (the speaker) once showed a case in which he performed such a modified Stacke, chiselling out the upper part of the attic outer wall, leaving the membrane and ossicles *in situ*, the result being excellent. He had invented a "tack-headed" attic burr; it was good theoretically, but was apt to fail in practice. The wedge of bone was difficult to get away with anything short of gouge and mallet.

There was no headache or giddiness in this present case, and therefore an excess of zeal should not be displayed. A preparation which would dissolve up the epithelial desquamation was a 10 per cent. solution of salicylic acid in sulpho-ricinate of sodium. Alcohol could be used by intra-tympanic irrigation, through the writer's modification of Sir William Milligan's tympanic syringe.

Sir WILLIAM MILLIGAN said the point of this case seemed to be that the electrolysis produced efficient drainage, and the ear healed. Efficient drainage could be produced more simply by avoiding the complicated technique of electrolysis, and he agreed with Dr. Kerr Love that the proper course here was to do a Stacke or modified Stacke

operation. There were two main things in surgery to be learned: (1) To see; (2) to drain. To approach these cases through the meatus was to perform a surgical procedure more or less in the dark. Other things being favourable, it was infinitely better to approach these cases from the outside, and carry on the treatment as in an ordinary first-class surgical procedure. He did not think anything was to be gained by electrolysing bone. How far was that process likely to proceed and what degree of sequestration would result?

Mr. SYDNEY SCOTT (President) agreed with Sir William Milligan, who had summarized the situation concisely.

Dr. A. R. FRIEL said that if a current of electricity were applied to tissues for a considerable time the nutrition of the cells would be so disturbed that they would be killed. It was preferable to use an ion such as zinc, which closed the blood-vessels, rather than ions such as chlorine, sodium, or iron.

Dr. JOBSON (in reply) said that he had exhausted other methods before resorting to electrolysis; nothing seemed to make any difference to the discharge—even five ionizations. The case illustrated the extreme resistance of these attic cases to any form of treatment. The question of operation was not discussed with the patient. There had been no working in the dark: he could see what he was doing. On the other hand, he considered that working with forceps was, in these cases, working in the dark.

Since the notes were sent in, the patient had had a slight recurrence of the discharge, but he had not seen the patient. He proposed to make the opening larger, and report later.

### **Tuberculosis of the Temporal Bone in an Infant, not caused by Milk Infection.**

By DAN MCKENZIE, M.D.

IN view of current opinion that tuberculosis of the middle ear (temporal bone) in artificially fed infants is always caused by milk infection by way of the Eustachian tube, the following case is of interest as showing that this cannot be looked upon as the only cause.

The child, now three years of age, was brought to hospital when six months old with painless bilateral suppuration of both middle ears and complete left facial paralysis. The glands around the ears were enlarged, and one subsequently broke down and was curetted. Pathological examination of granulations from the middle ear showed them to be tuberculous. A radical mastoid was performed on the left ear and a cortical (Schwartz) on the right. The ears are now quite dry, but the facial paralysis persists.

The point of interest is that the child had been brought up entirely upon one proprietary infants' food. I have communicated with the manufacturers of the food used and have received from them the assurances first, that the milk used in making the food comes from herds free from tuberculosis (tuberculin tested); and, secondly, that the food, after manufacture, is regularly submitted to bacteriological examination, with the result that the *Bacillus tuberculosis* has never been grown from it.

These assurances seem to be worthy of acceptance, and we are therefore driven to the conclusion that temporal bone tuberculosis is not always caused by food infection.

Incidentally, the case shows how complete cure of the disease may follow operation.

## DISCUSSION.

Mr. H. J. BANKS-DAVIS said he had seen cases in which babies nursed by the mother had developed the disease. It was not likely that the mother's milk was tuberculous.

Dr. W. H. KELSON, referring to the words in the notes, "complete cure of the disease," asked what was "cure." A case of his had been shown three or four times, in which the diseased area completely healed up for two or three years, and then the disease reappeared. One could not speak of cure until a considerable time had elapsed without a recurrence.

Mr. SYDNEY SCOTT (President) said he did not remember a child with proved tubercle of the temporal bone, who had been reared by breast-feeding only.

**Another Modification of the Radical Mastoid Operation.**

By DAN MCKENZIE, M.D.

IN cases where the mastoid antrum is small and inaccessible, particularly when a forward-lying lateral sinus is interfering with our freedom of approach, it is often advantageous to leave the mastoid surface entirely and to open directly into the aditus and antrum by removing the postero-superior segment of the bone wall of the meatus close to the membrane. This can be quite safely done with a small chisel held horizontally.

The operation is then completed from the antrum outwards towards the mastoid surface, and from the antrum forward towards the tympanum.

**Case of Abscess of Lateral Sinus.**

By Miss D. J. COLLIER, M.B. (introduced by  
Mr. RICHARD LAKE).

R. E., AGED 14, admitted to the Royal Ear Hospital on December 12, 1923, with history of discharge from left ear for four years. Three weeks before admission, attack of severe pain over left side of the head and in the left ear, and a polypus was said to have been removed from the left ear. A week later another attack of pain over the left mastoid process and attacks of shivering and vomiting. These recurred at intervals until admission a fortnight later.

On admission the girl was very ill, temperature 97° F., pulse 120, with pain and tenderness over the left mastoid process. Operation (Mr. Richard Lake): Extradural abscess, 1½ oz. of foul pus and thrombosis of the lateral sinus down to jugular bulb. Septic clot evacuated. Jugular vein not tied.

The following day, general condition still bad, but no rigors, and no signs of early meningitis. Lumbar puncture: fluid found to be normal. Temperature rose from 100° to 104° F. and pulse from 108 to 156 during the day.

The following day a large superficial swelling was found over the occipital bone posterior to the mastoid region. Aspirated, 2 oz. of pus withdrawn. Lumbar puncture, fluid again normal. Abscess posterior to the mastoid process refilled and was again opened.

The next day, temperature rose to 104° F. and pulse to 140. Vomiting during the night. Wound re-opened, and septic clot was turned out of lateral sinus nearly as far back as the torcular herophili. The patient made an uninterrupted recovery from this date.

## Paralysis of the Sixth Cranial Nerve in a Patient suffering from Chronic Otorrhœa.

By W. H. KELSON, M.D.

Mr. B., AGED 28, has suffered since infancy from an intermittent discharge from the left ear. Five weeks ago he noticed that he saw double on looking to the left. An ophthalmic surgeon whom he consulted could not find any cause for this, and sent him on to me. He was found to have an offensive discharge, with cholesteatomatous material, from the left ear and granulations in the middle ear, posteriorly. He has very little pain or tinnitus. Hearing: watch,  $\frac{1}{8}$  left;  $\frac{1}{8}$  right. Bone conduction better than aerial. Wassermann reaction negative.

Is there any connexion between the aural condition and the paralysis of the sixth nerve?

### DISCUSSION.

Mr. SYDNEY SCOTT (President) asked whether Dr. Kelson proposed to do any operation, as a cholesteatoma and granulations were present in the middle ear.

Dr. W. S. SYME said that in this case it was possible that the paralysis of the sixth nerve was not due to extension from the middle ear and mastoid, but rather to infection of the sphenoidal sinus on that side. There was nasal obstruction on the left side, and the patient complained of nasal catarrh. He should be given sufficient nasal airway by means of submucous resection, and then opening up the sphenoidal sinus if necessary. An X-ray examination should be made first.

Sir JAMES DUNDAS-GRANT asked whether there were not likely to be aberrant cells round the Eustachian tube; the zygomatic region should have special attention.

Mr. T. H. JUST said that six weeks ago he saw a patient who had sixth nerve paralysis on the right side. His aural symptoms were very slight. Seven weeks previously he had had slight discharge from the right ear, but none since, and he had had but little pain. When seen, he had hyperæsthesia on the same side of the head, but no definite mastoid swelling or tenderness. The drum was slightly dull, but was not perforated, and there was no active disease in the ear. Because of the nerve paralysis and the hyperæsthesia, it was decided to explore the temporal bone. The whole mastoid process was found infected, and there was muco-pus in the antrum. Behind the external semicircular canal he found a gallery of cells running in towards the tip of the petrous bone, and these were infected. A  $\frac{1}{4}$ -in. hole was made, the diameter of an ordinary pen-holder, and in order to make sure that there was no mischief in the middle fossa, the dura mater over that fossa was widely exposed, and was found to be normal. Lumbar puncture showed clear fluid under slight pressure. The gallery of cells was curetted and packed, and left open. In three days the sixth nerve paralysis had disappeared. He had had diplopia, and in two days that had cleared up. He advised opening up the petrous bone in this case of Dr. Kelson's, as the ear was still moist.

Mr. SYDNEY SCOTT (President) said he saw the man about whom Mr. Just had spoken. The only indication for operating on the mastoid was the internal strabismus, due to sixth nerve paralysis. The drum membrane was intact.

In the case of a child, also under the care of Dr. Hinds Howell, there had been a discharge from her ear two or three weeks previously. There was none when the speaker saw her, and the drum was normal. There had sometimes been headache, before she developed sixth nerve paralysis. He opened the mastoid because Dr. Howell urged it, and found it full of pus. The cerebro-spinal fluid was clear, free from organisms, but under hypertension. They had seen many similar examples of sixth nerve palsy with otitis media, and made a rule of operating on the mastoid and of puncturing the lumbar theca.

Mr. G. J. JENKINS said it was a dubitable point whether this was raised intracranial pressure or extension of inflammation of the dura mater or of the temporal bone. In these mastoid cases he did not think the cause was a rise in intracranial pressure.

Mr. W. M. MOLLISON said that in his experience this was a rare condition, while in acute mastoid cases he had seen three or four cases of paralysis of the external rectus. He felt sure the present patient had chronic disease of bone; there was nystagmus to the right, suggestive of labyrinth irritation, and in his opinion the mastoid should be opened as soon as possible.

Mr. A. R. TWEEDIE said he had recently had a small girl patient with bilateral suppurative disease. He had operated, and all apparently progressed favourably. Some ten days after the operation, however, he was horrified to hear a typical meningeal cry, and two or three days later she had complete paralysis of the left sixth nerve. This was verified by the oculist, who also expressed the opinion that she would recover from it, and fortunately she did so. The patient's convalescence was otherwise uneventful. The paralysis he (Mr. Tweedie) thought must have been due to some temporary localized meningitis.

Dr. KELSON (in reply) said that he intended to explore the temporal bone first; it might not be necessary to proceed to the sphenoid. He considered that the patient was at present in some peril; and there was a definite cholesteatoma, which should be removed, and the mastoid opened.

*Postscript.*—The left mastoid was opened by myself, subsequently, and two large sequestra were removed; one of these involved the middle fossa. The sixth nerve paralysis has gradually disappeared and the patient has made a good recovery. [W.H.K.].

### Three Cases of Chronic Suppurative Otitis Media treated by Ionization.

By A. R. FRIEL, M.D.

*Case I.*—Patient, man, aged 40. Discharge from left ear for many years.

On examination in January 23, 1924, a marginal perforation seen with polypi from a cavity at posterior wall. The polypi removed and silver nitrate was applied. On January 30, zinc ionization. No treatment since that date.

*Case II.*—Patient, boy, aged 8. Discharge right ear nine months. Cause of chronicity tympanic sepsis and granular condition of mucous membrane.

*Treatment.*—Zinc ionization on two occasions and boracic powder, in addition, on last ionization. Boracic powder insufflation once afterwards.

Discharge left ear twelve months. Cause of chronicity tympanic sepsis.

*Treatment.*—Zinc ionization and boracic powder on one occasion. Boracic powder on one subsequent occasion. Both ears quite dry.



84 Friel: *Three Cases of Chronic Suppurative Otitis Media*

*Case III.*—Patient, boy aged 12. Chronic suppurative otitis media.

Right ear: Cause of chronicity tympanic sepsis. Readily and quickly cured by zinc ionization.

Left ear: Cause of chronicity cholesteatoma in aditus. *Difficult* to cure. Outer attic wall has been in part destroyed by electrolysis. The sepsis in the ear has been treated by zinc ionization. There now is scarcely any discharge.

## Section of Otology.

President—Mr. SYDNEY SCOTT, M.S.

### Aural Surgery at a Fever Hospital.

By F. BRAYSHAW GILHESPY, L.R.C.P., M.R.C.S. (Birmingham).

DURING the last few years otologists have received appointments at fever hospitals in increasing numbers. In some cases their sphere of usefulness has been extended by the provision of ample facilities for operative measures to be undertaken, where necessary. Further appointments would follow the favourable criticism of this Section on the results of treatment already obtained.

The situation of the fever hospitals would appear to be as remote as possible. Ours is three miles from the medical centre of the city, and this bears directly

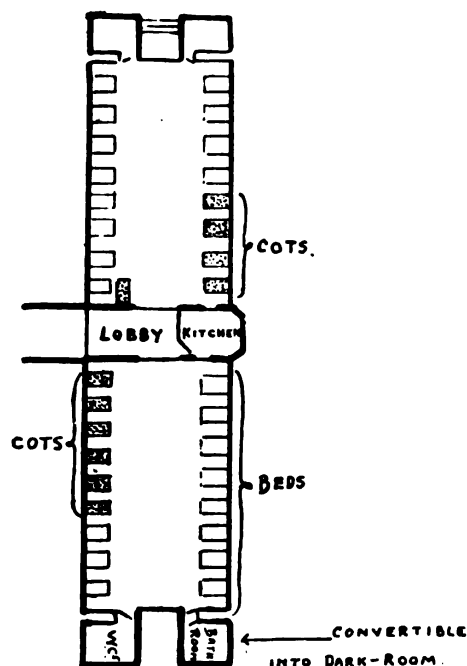


FIG. 1.

on the question of the early treatment of ear cases. The hospital has 500 beds, and two visits are paid regularly each week, besides emergency calls.

It is divided into two sides—entirely separate—for diphtheria and scarlet fever. The majority of the material consists of suppurative otitis media of scarlet fever origin, but with a considerable minority of cases of otorrhoea arising in the diphtheria blocks. The latter remain in their original wards. The former, as soon as is possible, are transferred to a special ear block. I feel that this is important for the following reasons:—

(1) The block is under the charge of a sister who has received a special ear, nose and throat training. (2) Further, her staff is kept as permanent as possible,



It was found last year, when cases of impetigo occurred in the ward, that the ears of the affected children showed great resistance to treatment and since then each case is barrier nursed; that is, a separate gown and washing bowl is provided at the side of the bed for the person who is coming into contact with the patient.

I must apologize for the time spent in laying stress upon the above arrangements, but in practice no detail is too small to be neglected when the final object to be achieved—the discharge of the patient with a dry ear—is in the balance.

Clinically, the ear may begin discharging in the first week of scarlet fever, or as late as the sixth to tenth week. If the discharge arises during the initial febrile period it may be unaccompanied by any further elevation of temperature. If it occurs during the afebrile stage, there would appear to be two modes of onset: one marked by an initial rise of temperature, often accompanied by pain in the ear; and the other occurring painlessly and without rise of temperature, so that the first indication of the complication is the finding of discharge on the pillow by the nurse, often in the morning. The onset of the first type is important, as the rise of temperature may precede the discharge by a day or more. In such cases paracentesis can be performed in time if this danger signal is realized. Figs. 2, 3, 4 illustrate these conditions.

Unfortunately, owing to the inaccessibility of the hospital, one is not always in a position to perform this operation. It is, I think, agreed—and the fact should be emphasized—that the naso-pharynx is primarily and invariably involved in every case of scarlet fever. It has been our custom, therefore, as soon as it was considered safe, to remove the initial focus of infection, i.e., the adenoids, in cases in which the ear discharge is not resolving. The tonsils were removed at the same time, if they were diseased. This operation can safely be done in the fourth week of an ordinary case of scarlet fever. If drainage from the middle ear was impeded a paracentesis was performed at the same time as the operation on the tonsils and adenoids.

The following table shows the results of treatment during the last year:—

TABLE I.—SCARLET FEVER PATIENTS.

*127 cases of suppurative otitis media.*

|                                                                                                    | Average of<br>discharge |
|----------------------------------------------------------------------------------------------------|-------------------------|
| 83 after "T. and A." operation. (Adenoids only<br>removed in 38 cases) ... ..                      | 15 days                 |
| 20 treated in ear ward ... ..                                                                      | 30 days                 |
| 13 acute cases of mastoiditis                                                                      |                         |
| 3 acute cases of mastoiditis in whom tonsils and<br>adenoids had been removed some time previously |                         |
| 1 acute case of mastoiditis arising soon after tonsils<br>and adenoids                             |                         |
| 3 conservative mastoid operations                                                                  |                         |
| 4 radical mastoid operations                                                                       |                         |

It will be seen that in about half of the eighty-three cases the adenoids alone were removed, as the children were very young and without obviously enlarged or diseased tonsils. Tonsillectomy would have added to the shock of the operation and, further, a rationale for their removal was lacking. The results were equal to those cases in which the tonsils had been removed as well. I believe that the free bleeding caused by the adenoid operation has relieved tubal congestion and hastened tympanic resolution in some cases in which the amount of adenoid tissue removed was smaller than one had been

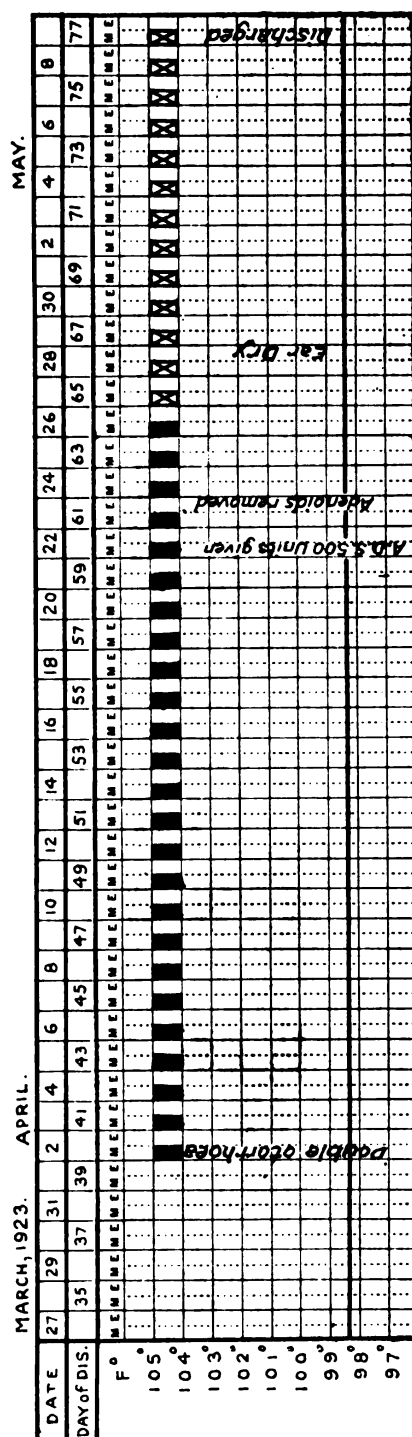


FIG. 5.

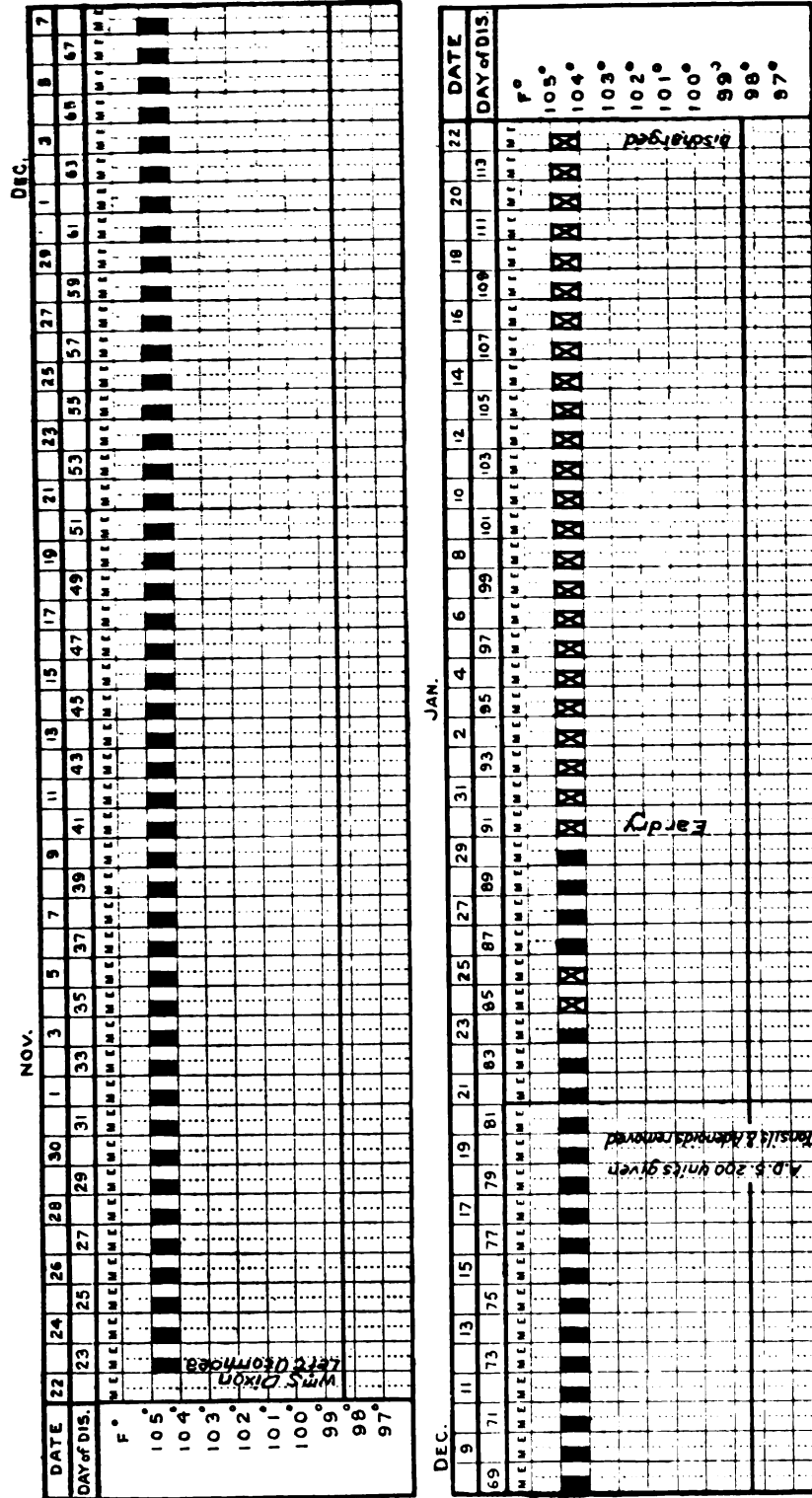


FIG. 6.

led to expect by previous digital examination. Kerrison in his latest edition supports this view.

It will be seen that the average duration of the otorrhœa after removal of tonsils and adenoids was fifteen days. This represents a saving of fifteen days in hospital over each case so treated, compared with those treated in a special ear ward.

In these diagrams the period of otorrhœa is black, and ☐ denotes that the ear is dry. It will be seen that the ear discharge ceases within a few days. These diagrams and charts show that the operation is effective in suitable cases where the operation has been delayed from unavoidable causes, such as the incidence of other complications.

The following table shows where the average has been spoilt and the supposed reason for the chronicity of the discharge.

TABLE II.

|                                                                                                                            | Duration of<br>Discharge<br>77 days |
|----------------------------------------------------------------------------------------------------------------------------|-------------------------------------|
| PARTLETT ... ..<br>Required paracentesis. She was not operated<br>upon for T. and A. until eightieth day of dis-<br>charge |                                     |
| BURROWS ... ..<br>Very dirty throat, after T. and A. suspicious<br>clinically of diphtheria                                | 43 ..                               |
| LAWLEY ... ..<br>Septic case of S. F. with breaking down of glands<br>both sides of neck                                   | 60 ..                               |
| BLOCKSIDGE ... ..                                                                                                          | 78 ..                               |
| BEDFORD ... ..<br>T. and A. removed on eightieth day of discharge<br>Membrane healed. Adherent                             | 66 ..                               |
| WHITEHOUSE ... ..<br>Impetigo                                                                                              | 37 ..                               |
| MASCHELL ... ..<br>Impetigo                                                                                                | 44 ..                               |
| KNIGHT ... ..<br>Restarted after removal of very septic tonsil                                                             | 47 ..                               |

The operation for tonsils and adenoids is performed in the lobby between the two wards, each child receiving 2,000 units of diphtheria antitoxin the day before. So far no case has developed diphtheria after operation. A headlight is used for operation and only three or four cases are done at a time, bleeding being stopped on the table. There have been no cases of delayed or secondary hæmorrhage. One child developed anterior poliomyelitis sixteen days after operation for tonsils and adenoids, which was performed in the tenth week of disease. I have no authentic record, but I have heard of one other such case.

Excluding cases which developed mastoid complications it would appear from the site of perforation of the membrane that the great majority of cases show evidence of Eustachian infection. An ionization apparatus has been available in the Ear Ward but on account of the favourable manner in which the infection has responded to the removal of adenoids, it has so far not been in great request. It has, however, been useful in helping to clear cavities remaining after mastoid operations and in the conservative and radical operations which it has been found necessary to perform.

Many of our cases come into hospital with a history of otorrhœa preceding the present attack. In a few cases of this type removal of adenoids has cured the condition. Similar cases account for the inclusion of the conservative and radical mastoid operations in my list. In future I shall perform a Schwartz

operation and leave the wound open to provide for counter-drainage, rather than do a conservative operation. The only advantage of the conservative operation would appear to be in the larger meatus resulting for the further treatment of the middle ear. All my radical mastoid operations have been performed on patients who have already been operated upon for mastoid disease.

Before being introduced to this work I had been impressed at a Deaf and Dumb Institute by the number of severely deaf children I had found there, presumably as a result of severe scarlet fever or measles infection in childhood. In my experience, during three years at this Fever Hospital, I have only seen one case in which the middle and internal ear have been attacked at the same time. This was a severe case of septic scarlet fever in which facial paralysis developed within six hours of the onset of the discharge. The child died within a few days of onset of the disease.

Variety is lent to the work by the fact of cases of persistent nasal discharge, either after scarlet fever or diphtheria, being reported to me with a request that I should do something to get them out of hospital cured. All such cases have cleared up after the removal of the patients' adenoids, with the exception of two cases; in one of these I found both maxillary antra infected and about the other I was doubtful. The condition of both patients cleared up after irrigation of their antra. It would appear, therefore, that in these diseases the sinuses are not often at fault.

All grown-up carriers of virulent diphtheria bacilli in their tonsils who defy local treatment are referred to me. In such cases a special anæsthetist comes to the hospital and the tonsils are dissected out. The results have been uniformly successful.

In conclusion it would appear that energetic treatment in the early stages of scarlet fever otitis is of value, as it is only when chronicity is established that the membrane tends to be widely destroyed. In only a small minority of cases have the patients left the hospital with perforations too large to be closed. All my patients who are attending school in Birmingham are followed by means of the card shown, and recurrences are reported to me at a special aural clinic.

TABLE III.  
PARTICULARS OF OTOLOGICAL CASE DISCHARGED FROM CITY HOSPITAL,  
LITTLE BROMWICH.

Registered No. \_\_\_\_\_  
Name \_\_\_\_\_  
Age \_\_\_\_\_  
Address \_\_\_\_\_  
School  
(if possible) \_\_\_\_\_  
Disease \_\_\_\_\_  
Complications \_\_\_\_\_  
Treatment \_\_\_\_\_  
Date of discharge \_\_\_\_\_  
Condition on discharge \_\_\_\_\_

*Medical Superintendent.*



If these results are approved by the Section, I would strongly urge that some provision be made for dealing with cases of measles with otorrhœa. From the greater incidence of measles during the first five years of life, when adenoids are in their most luxuriant period of growth, it may be safely argued by analogy with the foregoing results that timely treatment might prevent a great deal of wastage from ear disease in school and adult life.

I wish to record my deep appreciation of the help which I have received from Dr. E. H. R. Harries, the Medical Superintendent.

#### DISCUSSION.

Dr. KERR LOVE said that a week ago he attended at Newcastle-upon-Tyne a meeting of fever hospital superintendents, who discussed aural complications in scarlet fever. The impression he received was that if one looked round the medical profession for men who knew nothing about scarlet fever, such would be found among otologists; and that if a group of men were wanted who knew nothing of otology, they would be seen to exist among medical superintendents of fever hospitals. But now a new group was arising who might be termed "zymotic otologists," who would throw light on the subject of this paper. There seemed to be a certain amount of suspicion between the superintendent and the otologist, but each had much to teach the other. Otologists seldom saw scarlet fever cases in the acute stage, and superintendents of fever hospitals seldom saw the end-results of suppurative otitis due to scarlet fever, for they occurred years afterwards.

The conclusion seemed to be that no case of scarlet fever or measles aural suppuration should be allowed to leave the hospital until discharge had ceased, even if a mastoid operation were needed, which was seldom a radical operation unless in cases in which there had been suppuration before admission to the hospital. According to views expressed at the Newcastle discussion paracentesis was seldom necessary.

He had long been familiar with one class of case arising as a result of scarlet fever, namely that in which there was a total deafness though no suppuration had occurred in the middle ear of either side. The case might be a meningitis or a labyrinthitis occurring in acute scarlet fever or acute measles. The deaf and dumb schools contained many children whose deafness was due to scarlet fever or measles, in which suppuration had never occurred.

These cases without suppuration required further study, and he hoped members of this Section would use their influence, not only in increasing the number of these appointments in infectious disease hospitals, but also in furthering the care of these children who had been made deaf by zymotic disease.

He said Mr. Gilhespy's paper was very suggestive.

Mr. J. F. O'MALLEY said he had long felt that this question of otological treatment of cases in fever hospitals was one which should be taken up and urged by the Section and the specialty. The benefits resulting from the appointments of otologists that had been made were beginning to be in evidence.

Had the author made observations as to the degree of incidence of middle-ear suppuration in relation to the total number of scarlet fever admissions? Dr. Caiger, who was not an otologist, had stated the proportion to be 15 per cent., but it was desirable that the subject should be re-investigated by otologists. Dr. Caiger showed that the incidence was greatest from three or four to ten years of age, and that led him (the speaker) to the view that adenoids constituted a large factor in the production of middle-ear infection; and with that view Mr. Gilhespy had agreed.

Sir JAMES DUNDAS-GRANT referred to the author's statement that in a number of cases adenoids were removed and the tonsils left. There was, he (Sir James) said, much to be said for this, and he would like to hear what were the comparative results. Also, would those with experience on the point state what was the earliest stage, in a case of scarlet fever when it was practicable to remove adenoids. He had seen several cases of acute suppuration of the middle ear—not scarlatinal—in which the condition

hung fire until he removed the adenoids, and from that moment improvement took place. In one of the County schools with 1,000 pupils, 99 were reported to have large tonsils and adenoids. There was an outbreak of scarlet fever and the incidence among those with unoperated enlarged tonsils and adenoids was 1 in 12, and among others only 1 in 28. Of those who had had adenoids and tonsils removed, not one took scarlet fever. And recent statistics showed that in those who had no adenoids the disease had been of a mild type. The shock consequent on removal of tonsils was much greater than that from removal of adenoids.

Mr. H. V. FORSTER asked whether any attempt was made in cases of scarlet fever, measles and diphtheria to apply local treatment to the nose or throat, i.e., in all cases, not only in those who developed ear trouble. A nurse of experience from a fever hospital once remarked to him how frequently otorrhœa occurred in her ward where routine nasal lavage was employed for children. It was agreed that lavage and forceful spraying were risky in children, as ear disease was liable to occur.

The point brought out by Sir James Dundas-Grant concerning operation on the tonsils and adenoids in ear trouble was very interesting. He (the speaker) had been called upon to start a children's school clinic for ear, nose and throat troubles in a certain area near Liverpool, and he had operated on 1,000 cases of adenoid, or tonsil and adenoid trouble, since August, 1919. He acted on the principle mentioned by Dr. Kerr Love in his book, concerning the Glasgow clinics, that in children one should, if possible, spare the tonsil; with that he agreed partly, because the removal of the tonsil was a major operation—the adenoid operation being a minor one. He asked whether the condition of the tonsil had much influence on aural disease.

Dr. ALBERT A. GRAY said that Dr. Gardiner, in Edinburgh, had taken up the subject of operating on tonsils and adenoids in cases of scarlet fever.

Mr. H. J. BANKS-DAVIS referred to a case in which he performed a rapid mastoid operation on one side in a child with scarlatina. As the adenoids were large, he felt it on principle his duty to remove them. The child went on quite well for a fortnight, then developed acute mastoid trouble in the other ear; the parents refused a further operation and the boy died of meningitis. If he did a mastoid operation on a child he usually removed adenoids, but he left the tonsils alone. The raw area left after removing adenoids was considerable, and this should be borne in mind; if the tonsils were removed as well it was a more serious matter for the patient.

Dr. P. WATSON-WILLIAMS asked whether in these cases it was Mr. Gilhespy's practice to advise prophylactic measures, such as an antiseptic spray. There was good reason for hesitating to use douches, and hence many years ago in place of fine sprays he devised the coarse spray, which Messrs. Rogers made and brought out.

Though he had never had charge of a fever hospital, he nevertheless used to have many cases of the kind under discussion, and as a routine practice he used an antiseptic coarse spraying, and would now advocate such an antiseptic as collosol argentum, to prevent the gradually increasing infection which overwhelmed the lymphoid tissue of adenoids or tonsils or passed up the Eustachian tubes, and there became less under control.

He was interested in the conservatism of many colleagues with regard to tonsil removal, as it was not only in scarlet fever and measles that the tonsils should often be spared, at least when but moderately infected. To open up a large area which had the very free lymphatic and vascular communications, the tonsil would, he felt, tend to increase the danger to the patient. Would Mr. Gilhespy say how he differentiated between the cases in which he spared the tonsil, and those in which he removed the tonsils with the adenoids? The size of the adenoid hypertrophy had nothing to do with the degree of infectivity of the lymphoid tissue: often very small adenoids caused much more mischief than those greatly hypertrophied.

Dr. KERR LOVE (in reply to Sir James Dundas-Grant) said he did not know the number of scarlet fever cases occurring in hospitals for infectious diseases. In schools for the deaf and dumb, out of fifty cases of acquired total deafness, twenty

were probably due to meningitis which had not been associated with any other infectious disease, and twenty others were put down to scarlet fever and measles. Nearly all of them had sound tympanic membranes. He thought there was a metastatic labyrinthitis or meningitis.

Mr. MUSGRAVE WOODMAN asked what means the author had of dealing with impetigo in relation to ear disease and mastoid work. He himself had one case which impressed the danger on him. A boy had a radical mastoid operation done for severe ear disease, and went out with a healthy granulating ear. His family had impetigo, and he contracted it, and infected his ear with it; a fortnight later the ear was full of impetigo. He scraped and cleaned it out, but the case was a failure.

As to whether tonsils as well as adenoids should be removed, there were points both for and against the procedure. If the mastoid had been involved and had to be dealt with, the adenoid operation was sufficient for the patient to put up with. It seemed to help mastoid results to remove adenoids at the same time, but it depleted the tissues and opened the Eustachian tube, and the discharge ceased more quickly than if the operation were omitted. On the other hand in nearly all these cases the tonsils were simultaneously diseased, and the lymphoid tissue had very little power of regeneration and healing; it was therefore questionable whether many tonsils once severely infected ever completely healed and became healthy again.

In many of these cases there was enlargement of the cervical glands, and the quickest way to remedy this was to remove the tonsils. Recently he had seen two cases of enlarged glands in the neck with a recommendation from the doctor that they should be excised. He (the speaker) was not sure that they were tuberculous, but they were septic. He removed the tonsils first, being prepared to remove the glands if necessary. In both cases enlargement of the glands completely disappeared in ten days after removal of the tonsils, though previously treatment of tonsils had failed to influence the glands.

Many of these cases were Klebs-Loeffler bacillus carriers, and often the best way to clear away the nidus was to remove the tonsils. Each case, however, must be judged upon its merits. If the child was very ill, the tonsils should be left, but in most cases the best course was to remove them at the same time.

Mr. GILHESPY (in reply) said that what he had heard in the discussion would be of help to him in the future.

In answer to Sir James-Dundas Grant, he said that he only operated after the fourth week of scarlet fever; he did not think it was necessary to do so sooner, as the mastoid trouble had not developed. Delay before operation did not seem to influence the time at which the discharge would cease. In certain cases the discharge had been going on some time before the adenoids were removed, but it ceased immediately on this operation being done. He would not operate in a scarlet fever case when the whole naso-pharynx was intensely congested. He could not say when a tonsil was diseased. He thought that once the tonsils had been involved in an attack of scarlet fever, their structure became much altered, so that at some time they ought to be removed.

He believed 7·6 per cent. of scarlet fever cases having otitis was the correct proportion. Coarse spraying of the nose was not allowed by the superintendent of his institution; the nose was generally wiped out by cotton wool on which some antiseptic was placed.

## Case of Labyrinthine Vertigo due to Sphenoidal and Ethmoidal Sinusitis.

By P. WATSON-WILLIAMS, M.D.

PATIENT, a male, H., aged 45. For many years, since the age of 20, had been subject to attacks of vertigo about twice yearly. He would wake up in night with a sudden vertigo and vomit; pulse feeble. His doctor for a time suspected petit mal.

In August, 1922, the attacks, which had become gradually more pronounced, became very severe both at night and in daytime. He would feel as if hurtled through space, upwards and round and round, with inability to stand. This was sometimes followed by vomiting, faintness, and the sensation of objects looked at moving, duration usually two or three hours severe, with giddiness lasting twelve to eighteen hours. Had to avoid riding on tram-cars, driving his car for fear of sudden attack, and even dared not walk without help.

November: Hears watch, right  $\frac{2}{10}$ , left  $\frac{4}{10}$ .

T.F.C. 1. Right air —5" better than bone —7".  $A^{-5} > B^{-7}$ .

Left air normal better than bone —4".  $A^{=0} > B^{-4}$ .

Caloric test cold. Right: onset 1' 25"; nystagmus lasted 3' 15". Left: no response in 1½ minutes.

November 13: Operation: right sphenoidal sinus and posterior ethmoid cells freely opened; left sphenoidal sinus small opening for drainage. Culture of aspirated contents of sinuses showed *Staphylococcus aureus* in both sphenoidal sinuses and post-ethmoidal cells, the antra proving sterile.

November 17: No more vertigo till to-day—slight attack; and no more attacks till

November 25: Rather severe attack in waiting room, lasted two and a half hours, spontaneous nystagmus vertical and to left.

Up to December 8: Four more relatively slight attacks, but very greatly relieved, and remained so till

July 4, 1923, when he had seven attacks, and another July 8.

July 9: Explored sinuses. Antra both sterile. Sphenoidal sinuses, right and left, contained streptococci. Left posterior ethmoid cell sterile. Right posterior ethmoid cell contained streptococci.

Opened up afresh: Right sphenoidal sinus, right posterior ethmoid cell.

Ever since that operation no vertigo attacks, but frequently marked tinnitus in right ear, and has several times been poorly, stiffness in neck. He captained the village cricket team, drives motor anywhere, gets up 4 a.m. baking, and conducting his various businesses.

January, 1924: Caloric cold. Right optm. onset 10" lasted 1' 45".

Left optm. onset 42" lasted 1' 30".

*Additional Notes.*—At age of 14 had concussion from blow. About year 1912 quickly lost sight of left eye and could only see a mist, and was hemi-analgesic; hand and leg seen by Mr. Beaumont (Bath), went to St. George's Hospital under Mr. James, Mr. Harold Barwell, and Dr. Collier for the ocular, neural and nasal symptoms respectively.

November, 1922: Mr. Cyril Walker reported: Left optic disc pale? due to old retro-ocular neuritis in 1912. Has good colour vision and normal fields.

The vertigo appears to have been due to hypersensitive labyrinth, toxic, occasional for many years, becoming severe until the corresponding infected

right sphenoidal and ethmoidal sinuses were opened and drained and the sinusitis has not recurred for eight months at least, i.e., since the operation July, 1923.

#### DISCUSSION.

Dr. P. WATSON-WILLIAMS said he had been asked privately why, at the first operation, he opened up the sphenoidal and ethmoidal cells. The reason was that previously he did an exploration. He found *Staphylococcus aureus* in both the sphenoidal sinus and in the ethmoidal cells. He did not expect the results regarding the vertigo which had occurred.

Sir JAMES DUNDAS-GRANT said that the possibility of labyrinthitis ought to be remembered. The patient seemed to have had a vascular lesion in the right labyrinth, possibly it might be a toxic condition.

In this patient the cold caloric test took longer than normal to become evident on the right side, and it was difficult to reconcile this with Dr. Watson-Williams' attribution of the vertigo to hypersensitiveness of the labyrinth.

Mr. A. R. TWEEDIE agreed that the case was of great interest. He could not reconcile the statements that in August, 1922, the patient had apparently a functionless peripheral sense organ in connexion with the left vestibular nerve, whilst it was rather more sensitive than the other side in January, 1924. There was a hypersensitive condition suggested on the right side, but the apparent movement of objects was clockwise, whereas one would have expected them to have been counter-clockwise. He asked whether the man had a definite falling reaction corresponding with the vertigo and nystagmus. When seen to-day, the man had a transitory nystagmus to the right, of the third degree. He could not suggest a connexion between those of the nose and ear, other than by way of the Eustachian tube, but in the report there was no statement of the condition of the Eustachian tube on auscultation, and the result of inflation on the hearing, &c. There might, of course, have been some general disturbance from ethmoidal disease, and the cure of that might have cured the vertigo; in the same way as one could diminish vertigo by curing general anæmia or dyspepsia.

In descriptions of the cold caloric test, the number of cubic centimetres of water used, and its temperature, were not stated, nor what latent period was allowed to elapse before noting the onset of nystagmus; nor what was the duration of the resulting nystagmus. Also, was the galvanic test used?

Dr. P. WATSON-WILLIAMS (in reply) agreed that one could not reconcile the facts observed with one's usual results of the caloric test. He was not suggesting that this was a case of labyrinthitis, but he felt warranted in believing the labyrinth condition to be due to a toxic condition of the sphenoidal sinus or the posterior ethmoidal cell from which a variable sensitiveness of the labyrinth was induced. He did not know why the condition described was present in 1922. No galvanic test was applied, and the falling reaction was not noted. The man's symptoms were strongly indicative of pseudo-Menièrè symptoms, and it was apparently cured on opening up the posterior ethmoidal cell and the sphenoidal sinus of the corresponding side. True, in 1921, he had deafness and pain on both sides and he had some catarrhal deafness on both sides, although catheterization was done. The fact that the right ear did not improve from this, though the left ear did, he attributed to there being nerve deafness on the right side. The left eye remained as it was in 1914. He had never seen a case in which the patient was confined to bed and tumbled about on account of double catarrh, and had such pronounced attacks of vertigo from Eustachian tube catarrh. Had this been the cause the catheterization with free entry would have relieved it, whereas catheterization tended to increase vertigo and consequently had to be discontinued. The act of inflating his Eustachian tube sometimes apparently tended to excite the irritability of the labyrinth.

*Postscript.*—On July 27, 1924, he had a further severe attack of vertigo.—  
P. W. W.

## Two Cases of Chronic Otorrhœa treated by Zinc Ionization.

By A. R. FRIEL, M.D.

*Case I.*—M. B., female, age 11. Duration of disease, eight years. Condition on examination: Tympanum occupied by polypoid granulations. Treatment consisted in destroying these by zinc electrolysis. The sepsis was treated by zinc ionization and boracic acid.

It is interesting to see that the tympanum is now covered with smooth and dry mucous membrane.

*Case II.*—I. C., female, aged 9. Duration of disease, three to four months. On examination muco-pus was aspirated from a cavity at the posterior wall of the tympanum. This cavity was washed out with an attic cannula and then filled with zinc solution. The electric current was conveyed to it by the gelatine-covered wire previously described (November 3, 1923). The sepsis in the tympanum was treated by zinc ionization and boracic acid.

A rapid cessation of suppuration in both these cases was obtained by zinc ionization.

### DISCUSSION.

Mr. H. J. BANKS-DAVIS asked whether ionic medication would cure many cases after mastoid operations where discharge persisted in spite of the usual attention for years. Patients whom he had at one time sent to the Electrical Department did not improve very much. They often complained of giddiness, and did not like the treatment. It seemed to him that this ionic treatment should only be carried out by aural specialists who were acquainted with the actual conditions prevailing at the time.

Mr. A. R. TWEEDIE said that although the appearance of the child suggested the presence of adenoids, on inspection the naso-pharynx was seen to be perfectly healthy. He also noted that there was an almost total defect of the tympanic membrane, an apparently empty attic with free drainage, and no antral disease. He did not wish to throw any doubt on the success of this method, but such a case he would have hoped to heal with topical treatment without ionization.

Dr. FRIEL (in reply) said that before coming to the ionization clinic the child had had prolonged topical treatment. A few days ago he thought he saw a trace of secretion at the lower part of the tympanum, but on rubbing the white area with cotton wool he found there was none. The patient had had no treatment for a month before being exhibited.

Mr. Banks-Davis's question opened up too large a subject to be discussed at such a late hour of the meeting. He had regularly pointed out for what conditions the treatment was not suitable, but it was equally important to state those for which it was suitable. In his opinion for accessible areas the method was infallible.

## Tuberculosis of Nasal Septum and Middle Ear.

By F. J. CLEMINSON, M.Ch.

I FIRST saw this case on March 4 last. The history was that the patient had had increasing nasal obstruction and deafness since Christmas. I found the nasal mucosa heaped up on both sides of the septum, causing almost complete obstruction; it was dark red and granular in appearance and bled easily on being touched with a probe. Both tympanic membranes were reddened,

but not bulging. He could hear a loud voice at a distance of one inch on each side. He was admitted to the Middlesex Hospital at once. The Wassermann test was negative. A section was made of a small piece of mucosa removed from the septum, and the report was that there was stratified epithelium, beneath which were granulation tissue, fibroblasts and mononuclear cells; also there were several tubercular giant cells. Ten days after admission, right facial weakness was noticed, and I did a radical mastoid operation the same day. The report on the granulations was that giant cells were found. A week later the left side of the face twitched a little, and I did a radical mastoid operation on that side, and found the same condition of granulation without appreciable pus. I removed at the same time as much of the nasal septum as I could, but this week I notice that there is still some disease present on the anterior and upper parts of the septal remains. Hearing is about the same as it was before the operation. Dr. Beaumont found a lesion in the apex of the right lung, probably tuberculous in character. The sputum is not positive for tubercle.

Mr. A. R. TWEEDIE said that cases of adult tuberculosis in the middle ear must be very rare. He had a patient at the present time who had a large extradural abscess, following tuberculous otitis media, which was now getting better, and in which the progress of healing was apparently being assisted by the application of balsam of Peru.

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ROYAL SOCIETY OF MEDICINE

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SIR JOHN Y. W. MACALISTER  
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**VOLUME THE SEVENTEENTH**

SESSION 1923-24

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SECTION OF PATHOLOGY



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[Died, May, 1924.]

## SECTION OF PATHOLOGY.

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**November 20, 1923.**

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## SECTIONS OF MEDICINE, PATHOLOGY, AND THERAPEUTICS AND PHARMACOLOGY.

*(JOINT MEETING.)*

**January 8, 1924.**

DISCUSSION ON "THE USES AND LIMITS OF VACCINE THERAPY."

Sir ALMROTH WRIGHT (p. 1), Sir WILLIAM LEISHMAN (p. 3), Dr. WILLIAM GORDON (p. 4), Dr. A. P. BEDDARD (p. 5), Dr. NORMAN McCASKIE (p. 8), Dr. RICHARD ARMSTRONG (p. 11).

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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## Section of Pathology.

President—Dr. J. C. G. LEDINGHAM, C.M.G., F.R.S.

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### The Meaning of Fever—Illustrated from Typhoid.

By OLIVER HEATH, M.D.

#### INTRODUCTION.

THE problem of the meaning of fever involves us in a study of the separate events which are connected with or are part of this interesting phenomenon ; from which it follows that the main points of my thesis are three in number :—

(1) That the exciting cause of fever is the inactivation of antibody by the bacteria.

(2) That the purpose of the change indicated by the systemic symptoms of malaise and anorexia, as also of the specific immunity response, is in the main economy of body energy, and

(3) That the purpose of the rise of temperature is to accelerate the production of antibody, either normal or specific, as the case may be.

After studying typhoid fever in the wards for several years without result, I chose as a form of experiment to produce fever in myself by subcutaneous injection of typhoid vaccine, since by this method I was able to record at each experiment an additional known factor, namely, the number of bacilli operating ; in dealing with cases of typhoid one is quite ignorant of this factor, which no doubt varies quantitatively from day to day.

With regard to the validity of experimenting with vaccine and drawing conclusions which bear on natural typhoid fever, Hort and Penfold [1], experimenting with a large variety of bacteria, found that live and killed cultures produced almost identical rises of temperature, "immediate fever." Later, Besredka [2], using bacilli of the dysentery, typhoid, and cholera groups, found that doses (lethal in twenty-four to forty-eight hours) of either live or killed culture, caused death to the animal and exactly similar inflammatory lesions in the intestines. Thirdly, I myself, during the two hours after an accidental intravenous injection of 500 million typhoid vaccine, experienced two acute attacks of diarrhoea (with fever).

I think the best plan is to observe first what we can of the facts, and then consider the results in the light of previous hypotheses put forward ; I shall therefore describe first the experiments, then attempt a discussion ; but since each experiment includes an estimation of the bactericidal effect of my serum, it will be necessary first to outline the technique used.

Details of the preparation of material and of the working manipulations will be found in Wright's "Technique of the Teat and Capillary Pipette." A living typhoid culture was counted against red blood corpuscles, and the dilution found which was killed by all of a series of normal serums when

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50 c.mm. vols. of each ingredient are used; this was taken as the standard suspension and with my culture (1068) gave 100,000 bacilli to a c.c. of test mixture. As shown in the scale at the side of the charts, this standard suspension mixed with varying dilutions of serum was used to estimate bloods above the normal bactericidal power, while suspensions of 10,000 and 1,000 bacilli per c.c. were used for weak bloods. The mixtures were taken up in a large-bore throttled capillary pipette, which was then sealed at both ends and incubated for twenty-four hours at 38° C. Before incubation the mixtures are clear, but if growth occurs this is shown by well marked turbidity which can be proved, by plating out, &c., to consist of living typhoid bacilli; the clear vols. will be sterile. Dilutions of serum were made with 0.85 per cent. saline; but dilutions of bacillary suspension were made with broth in order to provide equal opportunity for multiplication in each mixture. The serums were tested when between four and eighteen hours old, and unheated. It is important to incubate the test mixtures as soon as possible after mixing or results will be erratic.

Bactericidin and complement co-operate to destroy living or dead typhoid bacilli, so that variations of either constituent may affect the results, and since the object of the test here was to determine the patient's power to kill typhoid bacilli (and not just the amount of bactericidin present) there was a distinct objection to using heated serum with guinea-pig complement in that we should thereby eliminate a possible natural variant and substitute a foreign one. On the other hand the variation of complement in the human normal is probably negligible; for two years I had occasion to do the Wassermann test with heated serums and titrated complement from a normal human [3], and using four people alternately I found that though one individual had more complement than the other three, each kept almost constant at his own level; this has been confirmed by Hayden [4].

The value of the test for comparative observations depends mainly on the standardization of the suspension. At the same time the use of normal controls is advisable. Before using the test in practice I made twenty-seven experiments on twelve normals during a period of weeks, and found that the killing power varied only from a dilution of serum of 1 in 2 to 1 in 8, and again, though individuals varied, each kept almost constant to his or her own level; in 122 subsequent tests on typhoid patients, and on myself after inoculation, results varied far above and below this range. I have also, where possible, tested the agglutinating effect of each sample of serum, and, in the series mentioned, bactericidal and agglutinating effects were found to vary in the same direction; agglutination is not a vital test but serves as a third method of control.

### (I) EXPERIMENTS.

(4) ON THE NORMAL SUBJECT.—In the intestinal lesion in typhoid, local inflammation brings an increased quantity of blood fluid into contact with the bacteria; this can be observed in process at the site of an experimental subcutaneous injection.

Now it is well known to all laboratory workers from observation of the absorption test that contact of bacteria and serum *in vitro* means inactivation of the normal or specific antibody present. This interaction occurs in infected tissue, and the continual return of inactivated fluid to the circulating blood causes a gradual diminution of the total blood content in antibody, an effect which can be shown *in vivo* to be associated with fever.

*Chart I.*—To take the simplest example, subcutaneous injection of 2,000 million bacilli caused fever lasting twenty hours, with a hundred-fold decrease of bactericidal power in my blood.

The next step was to see if this association holds good in fever, such as that of the first week in typhoid before appearance of specific antibody, and

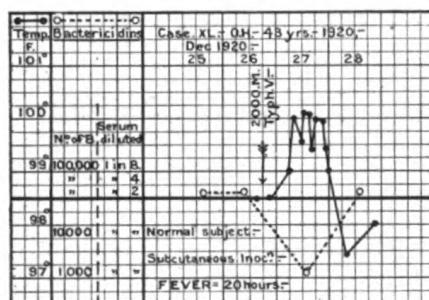


CHART I.

being unable to find cases in hospital at this stage I made the attempt to imitate the conditions in mild form in myself—full imitation would mean incapacity to record.

*Chart II.*—I must first point out that all through these experiments I have made no attempt to estimate the small blood variations which might occur

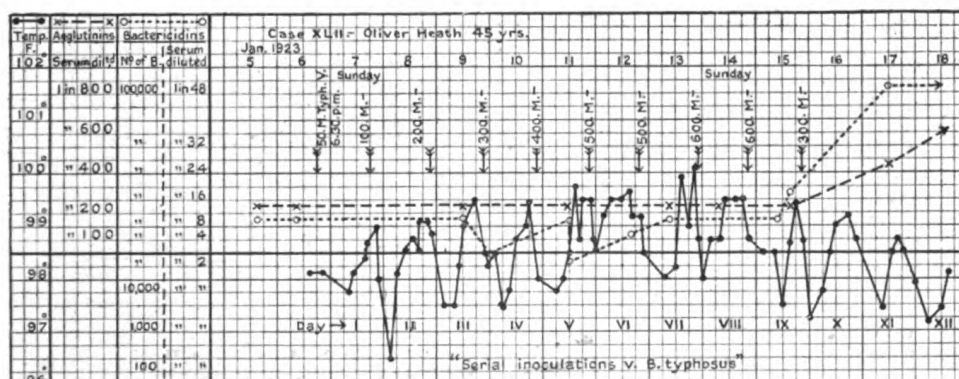


CHART II.

after small doses, because if we graduate any technique to too fine a point we immediately run into danger of exposing to view our own errors of working; the smaller doses were used only to feel my way.

From this chart it will be seen that a series of doses increasing daily from 50 million to 600 million caused an intermittent fever slowly rising to the seventh day. On the fifth and sixth days fever was continuous and the bactericidal power, twice tested, showed a decrease: another fall, occurring on the third day between 11 p.m. and 2 a.m. on a short rise of temperature, is interesting as suggesting that even a small rise of temperature at an hour when this should normally be falling represents a greater reactive effort than appears on the surface.

Now, if we may bring imagination to bear and increase both the size and

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continuity of the doses, and so of the effects produced, we should, I think, see a continued supernormal temperature and a continued subnormal bactericidal titre, and this, I believe, represents the conditions in the first week of typhoid.

In support of this view I would recall to your minds the high percentage of positive blood cultures obtainable at this stage, since we cannot imagine bacilli living in the blood unless antibody is very deficient.

(B) ON THE IMMUNE SUBJECT.—The possibility of a similar association, occurring in fastigium after appearance of specific antibody in the blood, opens

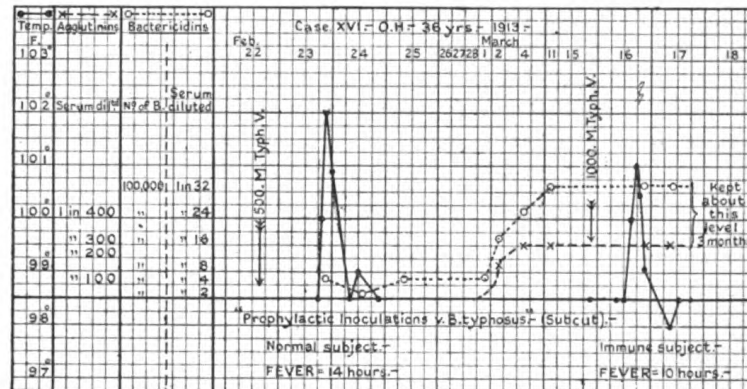


CHART III.

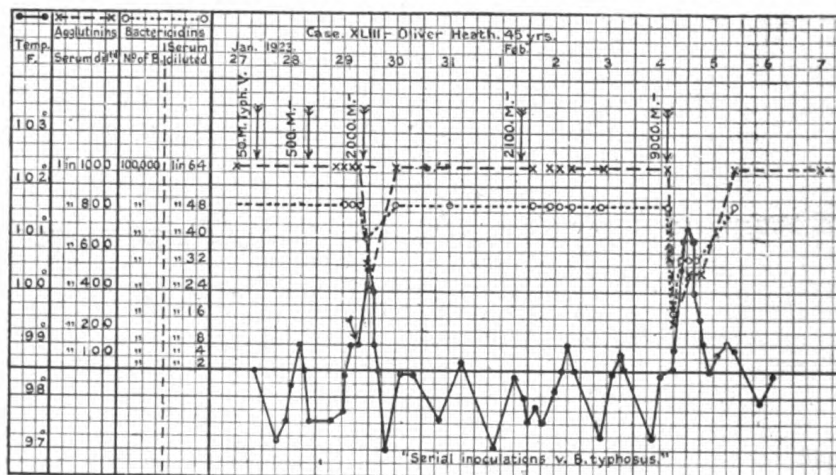


CHART IV.

up a problem which appears at first sight insoluble. If, as I am suggesting, depletion of antibody below normal is the cause of fever, how can fever continue (as we know it does) with an immunity considerably over normal? Though the proposition seems paradoxical, explanation is comparatively simple.

*Chart III.*—After inoculation in a normal subject, it is possible to demonstrate specific antibody in the blood at the eighth or ninth day. This increases slowly to a level well above normal. This level is maintained for a considerable time and is not, in my experience, raised by further doses of vaccine; in this

case it was unaltered for three months, but became normal again in nine months (the chart only covers a short period).

*Chart IV.*—This is directly continuous from No. II, and as the result of the ten doses we see again that a definite level of immunity is established which (where undisturbed by experiment) remained the same for two months, but was falling at six months. The old normal has been temporarily replaced by a raised "immune normal" which, excepting disturbances, remains stationary for some time.

Secondly, fever occurs only after doses and (discarding the smaller effects as too fine for our technique to detect) we find with fever a dip in the antibody curve; from which, I think, we may take it that some depression of blood titre below this "immune normal" is an essential factor in the causation of the fever. Applying this observation to the case of typhoid fever we can picture on the one side the patient's reactive forces working to establish the new normal, on the other side the bacilli, using antibody at a rate which prevents its attainment. In this chart, where there is fever, the blood titre falls; if

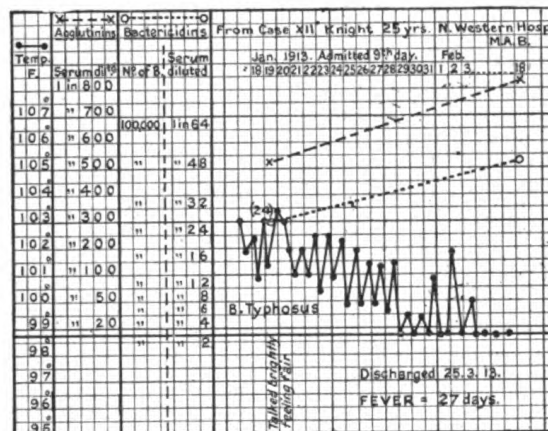


CHART V.

fever were continuous the titre would range continuously below the immunity level shown, though above the old normal: i.e., the demonstrable titre would rest between the old and the new normals somewhere round the lowest levels shown in the chart, while the steady level would exist only in mind as the unrealizable ideal.

This strongly suggests that the titre demonstrable during the fastigial period of typhoid represents just such a position of balance, and records covering defervescence commonly show results affording support to this view. I select two from fifteen.

*Chart V.*—This shows a defervescence of typhoid fever in which the rise of antibody occurred after the beginning of lysis, showing that the maximum titre was not in fact attained during fastigium.

*Chart VI.*—In this case a rise is shown to occur after the temperature had reached normal, so that "recovery" of the blood titre to the "immune normal" appears to extend well into convalescence. Topfer and Jaffé [5] record results comparable to these.

If this view be accepted, we have in fastigium, as in the first week, association of fever with diminished antibody content of the blood, but the titre



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here has failed to reach the ideal, and not, as in the first week, been depressed from a previously existing level.

In addition to this association, a direct relation can be shown to exist between the fall of blood titre on the one hand, and both temperature and symptoms on the other; but in this connexion cognizance must be taken of the fact that temperature in fever does not rise indefinitely; the mean is rather raised to a higher level, after which intensity of attack is more likely to prolong than increase the rise, while rest in bed may keep the temperature low but prolong the duration. The relation can be best shown by taking in order the factors which influence the rise.

(C) FACTORS WHICH INFLUENCE RISE OF TEMPERATURE.—Variation of sensitivity to the stimulus, or individual variation, does not come into view here.

(1) *Size of Dose.*—In Chart IV (omitting the second dose of 2,000 million) the first two doses were too small to cause appreciable blood variations, i.e., the fall of antibody was very small: 50 million caused a short rise of temperature to 99° F.; 500 million a similar but prolonged rise; and we can safely assume that more antibody was used and re-formed (I disturbed the end of this curve

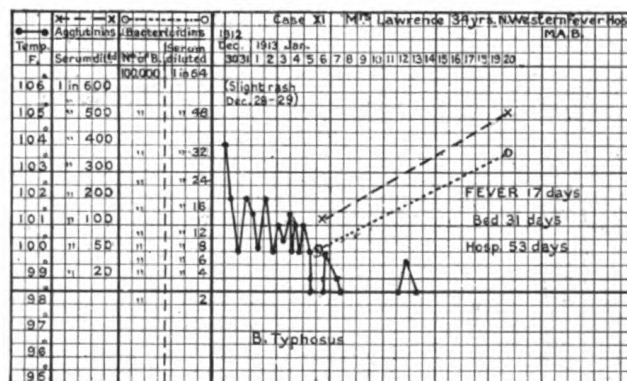


CHART VI.

by injecting the next dose). The first 2,000 million caused a fair rise of temperature and fall of antibody, while 9,000 million increased both effects.

This seems to show that the size of dose and amount of antibody used (which must be equal values), fall of blood titre and rise of temperature, vary together and in the same direction; but it can be shown in several ways that this form of presentation does not reveal the whole truth.

(2) *Rate of Absorption of Dose.*—Of the two doses of 2,000 million in Chart IV, where immunity is equal (also dose and inactivated antibody), the second (injected on a normal temperature) appears to have been absorbed very slowly over two days, disturbance of blood titre was inappreciable by my technique, and the temperature barely raised above normal: the first dose (injected on a raised temperature) caused a sharp reaction complete in eight hours, an appreciable fall in blood titre, and definite rise of temperature. It seems that the greater rate of absorption caused a greater fall of blood titre and consequent increased rise of temperature: but this again is not all.

(3) *Difference of Immunity Level before Injection.*—If we compare now the three doses of 500 million in Charts IV, II, and III, in order, we find that as immunity is lowered there is a marked increase of temperature, and from my experiences I feel safe in assuming that if there was any difference in rate of

absorption it was certainly more rapid with the high (and not the low) immunity, and cannot have been responsible for the increase of temperature.

It would seem, so far, that increase of the dose or rate of absorption cause more rise in temperature by increasing the fall of the existing blood titre; it is therefore only necessary to consider blood titre and temperature. Assuming that equal doses will produce equal falls of titre, this fall will be a larger part of a low than of a high immunity, and this may account for the increase of temperature.

(4) *Relation of Blood Titre and Temperature.*—For the last experiment in Chart IV I calculated (from body-weight and test results) that my total normal antibody is capable of dealing with (or equivalent to) about 400 million typhoid bacilli, and with an immunity of twenty-four times normal (Chart IV) with about (24 by 400 =) 9,600 million bacilli. To test this I gave myself a dose of 9,000 million bacilli the effects of which on temperature and blood are shown in the chart.

In the absence of an experiment showing the effect of 400 million versus normal blood, if we compare the rise of temperature with that shown after the dose of 500 million versus normal blood in Chart III, it will be seen to be roughly four-fifths of this.

This seems to show that if the dose bears the same proportion to the total immunity, whatever that immunity may be, the resulting temperature will be the same: and since, with a similar absorption time, any dose must cause a relatively proportional fall of blood titre, we can now say that the rise of temperature depends exactly on the proportion which the fall of blood titre bears to the total immunity.

To put this into mathematical form one would need first a technique which would show the exact blood condition over a period of hours and register the smallest variations, and, secondly, each temperature record would have to show a clean rise and fall. I do not think we possess a perfect technique, and I have therefore calculated from the dose (taking 400 million bacilli as the normal equivalent or unit of measurement), worked out the proportion of dose to total immunity, and put the results in the form of a table against the rises of temperature. Though I have selected the temperatures with the cleanest rises and falls, each of the first three show either a small rise on the second day or a somewhat flattened apex; I think both should be considered as having the value of say  $\frac{1}{2}$  to  $1^{\circ}$  F. added to the recorded rise; an addition of  $0.7^{\circ}$  F. to each of the first three temperatures would perfect the temperature series. Perhaps it would accord better with actual conditions to take the proportion against half the dose for subcutaneous and the whole dose for intravenous injections.

TABLE.

| Chart | Dose     | Immunity | Proportion of dose to immunity | Recorded fall of titre         | Temperature | Remarks                                                 |
|-------|----------|----------|--------------------------------|--------------------------------|-------------|---------------------------------------------------------|
| III   | 500 m.   | Normal   | 5 : 4                          | $\frac{1}{2}$                  | 102.0°F.    | Small rise on second day                                |
| IV    | 9,000 m. | 24 units | 5 : 5 $\frac{1}{2}$            | $\frac{1}{2}$ to $\frac{1}{4}$ | 101.3°F.    | Apex flat 2 $\frac{1}{2}$ hours and small rise next day |
| IV    | 2,000 m. | „        | 5 : 24                         | $\frac{1}{2}$                  | 100.5°F.    | Apex flat 1 hour, and slight rigor $\frac{1}{2}$ hour   |
| III   | 1,000 m. | „        | 5 : 32                         | —                              | 101.0°F.    | Clear rise and fall                                     |
| II    | 200 m.   | 4 units  | 5 : 40                         | —                              | 99.5°F.     | „ „                                                     |
| II    | 50 m.    | „        | 5 : 150                        | —                              | 99.0°F.     | „ „                                                     |
| V     | 50 m.    | 24 units | 5 : 960                        | —                              | 99.0°F.     | „ „                                                     |

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(D) FACTORS WHICH INFLUENCE SYMPTOMS.—The local discomfort or pain and the systemic symptoms of malaise and anorexia are personal sensations and not measurable. I can, however, say quite definitely from my records and memory of my experiences that they do not depend on the proportion of the total antibody inactivated: they appear to correspond with the level of antibody titre, increasing in severity as this is lowered. This explains how one can on one occasion be exceedingly ill with a temperature of 101° F, but on another, comparatively well with a rise to 102° F.

Though the number of experiments is perhaps not enough upon which to form the basis of a final conclusion on so important a subject, I think they show a sufficiently clear association of facts to support a provisional theory, as follows:

In certain bacterial infections with fever, the resulting inactivation of antibody causes a disturbance of blood equilibrium, and, as happens when volume is altered by hæmorrhage or the hydrogen hydroxyl balance upset, the body reacts to restore balance. Fever does not occur unless the existing normal or "immune normal" is depressed: when the blood titre is lowered, actually or potentially, the rise of temperature increases as the proportion of antibody inactivated (or dose of bacteria) to the total antibody content is greater, while the local and systemic symptoms increase in severity as the blood titre is lowered towards zero.

I think the evidence forms a very strong case in favour of considering the disturbance of blood titre as the actual exciting cause of fever.

### (II) DISCUSSION.

The number of hypotheses extant tells us that there is to-day no generally accepted view on the theory of the cause or purpose of fever [6 and 7].

(A) CAUSE OF FEVER.—Speaking generally, previous experiments have shown that fever follows injection of bacterial or other protein, and if the animal recovers, an antibody, specific to the protein used, is formed. Assuming that my experiments mean that the rise of temperature increases as the proportion of antibody inactivated to total immunity, this must be added as an important fact to be brought into line with any theory before it can be accepted.

*Suggested Causes.*—It is not now acceded that salt *per se* causes fever [8 and 9]; the prevalent idea is that the actual agent is of a protein nature, and in infections is bacterial.

*Foreign Protein.*—Friedberger [9] showed that in an animal sensitized to a protein other than bacterial, minute doses caused fever, but large doses caused collapse and subnormal temperature or even death. This was said to be due to a toxic product, anaphylotoxin. Vaughan [10] added the observation that specific proteolytic ferments were formed in response. Besredka [11] showed also that a minute second dose will protect from the shock which otherwise follows a large one.

Though it seems that protein fever must eventually be brought into line with bacterial fever, sufficient work has not yet been done on the condition of the blood to enable one to form a final opinion. Since these are not natural infections it is possible we have not yet evolved a form of defence comparable to that we possess against bacteria. The fact that immunity results, however, suggests analogy.

*Non-neutralized Bacterial Protein.*—If this were the active agent, fever would be in inverse proportion to the inactivation of bacteria by antibody: this is not so.

*Bacterial Endotoxin.*—Neither temperature nor symptoms are proportional to the dose of bacteria.

*Bacterial Exotoxin.*—This has not been satisfactorily demonstrated in connexion with bacteria which cause fever; and in pure diphtheria and tetanus, where toxin is known to act, fever is usually slight [12].

*Toxin formed by Bacteria from the Body Tissues.*—This idea has been strongly supported by Hort and Penfold [8]. These observers found that supernatant fluid from broth cultures causes fever, and argued that bacteria formed from the medium a "pyrogenic" substance which, however, they could not separate from the bacteria: it is more probable that the fluid contained autolysed bacteria. They next found that emulsions of pus or spleen juice from an animal recently infected and rich in bacteria did not, while a subculture from a septicæmic animal (grown in hæmolysed blood to represent bacteria from septicæmic blood) did cause fever; from which they argued that the pyrogenic substance was formed, but slowly, from body tissue: it is clear, however, that bacteria in tissue fluids will have been neutralized by antibody, and Broughton-Alcock [13] has shown that a vaccine of live sensitized bacteria does not cause fever: in fact, this and Broughton-Alcock's work strongly support the view I have brought forward, since a neutralized bacterium, being unable to combine with more antibody, will for this reason be incapable of causing fever.

*Protein Split-products.*—Probably the bacteria-antibody complex is neutral and innocuous. Vaughan [10], however, has suggested that this interaction results in a toxic split-product of protein which in itself causes fever. If this were true, fever would be proportional to the (amount of antibody or) bacteria neutralized. In addition to the contrary evidence already brought forward, my experiments show that 2,000 million and 9,000 million in Chart IV and 1,000 million in Chart III each caused less rise of temperature (and incidentally fewer symptoms) than 500 million in Chart III.

*Lowering of the Antibody Content of Blood.*—If the conditions described in my experiments are true the only possible explanation of the cause of fever is as follows: The circulating blood, impoverished in protective substances by interaction of the whole bacterium with antibody in the lesion, acts as a physiological stimulus to the nerve centre concerned and in this way causes fever. Fever will then be a defensive reaction of the body intended to restore the antibody equilibrium of the blood.

#### (B) PURPOSE OF FEVER.

(1) *Purpose of the Rise of Temperature.*—According to Krehl [14], who quotes Ziegler, Hildebrandt, and Filehne, of animals infected with several different bacteria, those which had the temperature artificially raised by puncture of the mid-brain suffered less severely than the controls. I have shown also [15] that the duration of a common cold may be cut down to three days by effecting a rise of temperature to between 101° F. and 103° F., whereas a rise to 100° F. or less does not prevent pus formation and prolonged trouble. There is therefore evidence of beneficial action.

Kolmer [16] quotes Graziani and Fukuhara to the effect that animals kept overheated produced typhoid lysins and agglutinins more rapidly and more abundantly than those kept cool, and Findlay [17] has shown that peritoneal exudate of pigeons had a greater bactericidal action on pneumococci when at 42° C. than at 37° C. Findlay, and Werkman [18] too, also found

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that lowering the temperature of animals by vitamin deficiency or administration of pyramidon (Findlay) made them susceptible to bacteria to which they were normally immune.

There is, therefore, clinical evidence that a raised temperature is of benefit in infection, and laboratory evidence that it affects antibody production and action favourably: in each case lowering the temperature artificially has the reverse effect.

The relation of the rise of temperature to the proportion of total antibody inactivated shows that as the danger to the organism increases so the temperature rises: i.e., it rises in response to the call for rapid production, and the two 2,000 million doses of Chart IV indicate a definite relation with rapidity of reaction.

I think the raised temperature accelerates antibody production as it does other organic processes.

(2) *Purpose of Raised Specific Immunity.*—The last experiment in Chart IV shows that with an immunity twenty-four times normal one can dispose of twenty-four times as many bacilli as with a normal blood, for the same expenditure of energy as shown by the rise of temperature. This is a very great economy of body energy in production of antibody, and at the same time each c.c. is more efficient owing to concentration of antibody.

(3) *Purpose of the Change indicated by the Systemic Symptoms.*—The systemic symptoms merely indicate inhibition of normal activities, and release of energy for defence; in other words, conservation of the total body energy.

### SUMMARY.

Fever associated with bacterial infection is due to the inactivation of blood antibody by the bacteria in the lesion; temperature rising as the proportion of antibody inactivated to the total antibody content increases: severity of symptoms increasing as the antibody titre falls towards zero.

The impoverished circulating blood, acting on the nerve centre, excites impulses which:—

(1) Inhibit normal activities and free energy for defence.

(2) In place of these, there start two (abnormal) defence processes which, (a) form fresh antibody, and (b) raise the temperature to accelerate this process.

Lastly, where specific immunity is exhibited this enables the patient to use his energy in greater comfort and more economical fashion, and increases the efficiency of the blood fluids as destroyers of bacteria.

### PRACTICAL CONCLUSIONS.

If this thesis be accepted I think one can draw the following conclusions:—

(1) Since even large doses after specific immunity is established cause no further rise of immunity, the usual second prophylactic dose of typhoid vaccine is useless, and vaccine during the fastigium of typhoid will only hinder the patient.

(2) Five hundred million (Chart III) produced an immunity of sixteen times normal, while 3,550 million (Chart IV) one of twenty-four times normal. If, as this suggests, there is some relation between attack and immunity response, the second prophylactic dose should be given within a few days of the first, and vaccine during the first week of a mild typhoid would be likely to increase the immunity response.

(3) Knowing the all-important purpose served by the rise of temperature, and the depressing effect of antipyretic drugs on defence shown by Findlay, the use of such drugs for the cure of fever should be put under the head of malpraxis.

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SECTION OF PSYCHIATRY



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## Section of Psychiatry.

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### Some Problems of the Future.

#### PRESIDENT'S ADDRESS.

By THEO. B. HYSLOP, M.D., F.R.S.E.

ON considering the question as to what should be the subject of this address, I came to the conclusion that it might be advisable to discuss some of the problems of the future, of which there is without doubt a superabundance.

In a presidential address, delivered before the Psychological Section of the British Medical Association at the Conference held in London, I attempted to deal with some of the problems of the evolution and dissolution of the mental faculties, and I endeavoured to draw a line of demarcation between legitimate hypotheses and fanciful conjectures as to the relationships between physical events and psychical events, and to correlate some of the phenomena known to science. Since that time the march of knowledge, and the reasonings based upon its data, have proceeded so rapidly and so diffusely that it seems advisable before outlining some of the problems whose solutions lie in the future, to give a brief summary of some of the problems which have never been solved and which by their nature are, and probably ever will be, beyond the comprehension of human intellect.

During the past decade the habitable portion of this world has undergone such a complete upheaval that the thoughts of nearly every human individual must perforce turn to the question as to whither does humanity tend. Science shows that evolution and devolution are constant throughout the universe, and that even worlds evolve and devolve. It is, however, characteristic of the human mind that it tends either to ignore possibilities, or even to claim exemption from such a possibility as that of its own devolution, and it nurses illusions which do not conform to reason obtained in the light of scientific fact. We, who are rationalists, by reason of our training, must recognize some of the limitations to the evolution of humanity. We note with wonder an amazing advance not only in mental capabilities but also in adaptability to an ever-changing physical environment. We note with complacency the history of the rise and fall, not merely of individuals, but also of nations and civilizations, yet we hesitate to become rational as to the outlook for humanity lest we should be dubbed pessimistic rather than rational. The destiny of this world is by reason of legitimate conjecture becoming disclosed. We know with

certainty that worlds are born, live and die. We also know that our world must conform to its allotted rôle: but our innate conceit with regard to this our habitation tends to render us blind to an inevitable prediction as to its ultimate devolution.

One of the greatest of problems is undoubtedly that of the possibility of a coextension of mind and matter in time and space. We note with acquiescence the infinity of both space and time, we grant the indestructibility of energy (i.e., that something from which mind, although collateral in its manifestations, cannot be evolved) and yet we hesitate to grant the possibility of mind being coextensive with them.

In spite of the fact that the human mind is known to us only as having its origin in connexion with a collateral series of physical phenomena, it would not appear unwarrantable to predict that human reason will ultimately lead to the conviction that mind *per se* has more than an earthly precedent, and that it is, in fact, coextensive with both time and space. Religion, which has as its object "to gather and consider," will then become based on the common ground of reason. I, for my part, and in the light of scientific reasoning, can see no legitimate alternative conclusion to that of a coextension of mind, matter and energy, through time and space, and it may be predicted that with the advance of human knowledge of the modes and manifestations of mental and physical data there will arise a fuller comprehension of the universe in its totality.

The human mind cannot be regarded, or spoken of, in terms of energy, nor can we view it as being a transformation of energy, for this would involve destruction of the fundamental dogma of science that energy itself is indestructible and therefore incapable of conversion into anything other than energy. Since parallel lines do not meet, it becomes inconceivable that either in time or space there ever was a beginning of the one apart from a beginning of the other—i.e., the philosophical conclusion derived from the data of physics and psychology is that mind in its ultimate essence is coextensive with energy. The question of the scope of personality in time and space involves the use of working hypotheses to pave the way for an explanation of telepathy, clairvoyance, double or multiple personality and also of the phenomena of genius. To Myers' doctrine of the "subliminal self"—a doctrine that we are, each of us, larger than we know, and that each one of us is only a partial incarnation of a "larger self"—we owe a flood of conjecture, some of which would appear to be legitimate. Sir Oliver Lodge endorses the belief that the individual, as we know him, is but an incomplete fraction of the larger self; i.e., a portion only of the whole self at any one period, in intimate contact with matter, and in close association with a material body. This problem involves the question of pre-existence in other collocations and forms, and with it the collateral problem of life itself enshrined in a germ with potentialities which will in the future require explanation. I, myself, agree with the view that the association of spirit with matter—the incarnation of something pre-existent—appears to be a reality, whether we understand it or not, and that a gradual incarnation, or utilization, of matter by life or by spirit may really occur. Primitive instincts, and the mechanism of race-memory, and pre-natal choice, also involve many considerations with regard to heredity which doubtless will become more clear in the future.

Before leaving this subject, I would urge that in dealing with these problems we might with advantage adopt a greater spirit of tolerance than some of us have done in the past. As Sir Ernest Rutherford says: "Experiment without imagination, or imagination without recourse to

experiment, can accomplish little; for effective progress, a happy blend of these two powers is necessary." In penetrating obscurities we cannot invoke the aid of super-men, but must depend on the combined efforts of a number of adequately trained ordinary men of scientific imagination.

In any attempt to bring physical and psychical phenomena into apposition we must take into account the advance in speculation as to the nature of the atom. For illustration, let us take an atom of uranium as representing the physical phenomena to be brought into apposition with psychical events. Sir Ernest Rutherford, in his presidential address to the British Association, described this atom as having at its centre a minute nucleus surrounded by a swirling group of ninety-two electrons, all in motion in definite orbits, and occupying, but by no means filling, a volume very large compared with that of the nucleus. Others have orbits of a more elliptical shape, whose axes rotate rapidly round the nucleus. The motion of the electrons in the different groups is not necessarily confined to a definite region of the atom, but the electrons of one group may penetrate deeply into the region mainly occupied by another group, thus giving a type of inter-connexion or coupling between the various groups. The maximum speed of any electron depends on the closeness of the approach to the nucleus, but the outermost electron will have a minimum speed of more than 1,000 kilometres per second, while the innermost K electrons have an average speed of more than 150,000 kilometres per second, or half the speed of light. When we visualize the extraordinary complexity of the electronic system we may be surprised that it has been possible to find any order in the apparent medley of motions.

I have quoted this theory in detail because these seem to be the physical phenomena with which psychologists are to bring psychical data into apposition. I, for my part, believe that the solution of such a problem is, and ever will be, far beyond the grasp of human understanding. The problem of the collateral relationship between physical and psychical events is without doubt becoming more accessible to science and reason, but that an ultimate solution of their intimate relationships is attainable is quite another matter.

Any attempt to proceed beyond the objective evidences of localization of motor and sensory functions to the localization of the parts of the brain subserving the subjective side of mental phenomena must necessarily be attended with great difficulty, and it must be confessed that the localization of subjective conditions still remains resting upon a most unsatisfactory basis.

Future endeavour to arrive at some conclusion as to how far we can localize even the simplest mental state does not receive much encouragement from knowledge gained up to the present. Phrenology has done little to help us. Experimental research has enabled us to determine certain physical relations between cortical areas and afferent and efferent impulses. To comparative anatomy we are indebted for much knowledge as to the differences of brain structure coexisting with differences of mental faculty in races and species of animals, and morphological data appear to warrant the assumption that quantitative differences in the brain trunks both in man and animals are dependent upon quantitative variations in the different parts of the fore-brain. Similarly, morbid anatomy, as exemplified in focal diseases, has to a certain extent added to our knowledge with regard to the localization of mental disturbances.

Certain areas of the cerebral cortex do appear to be concerned with the execution of certain functions of the mind; but we cannot regard the mind,

in its special relation to the brain, as limited to any point or small area of the cerebral cortex. Goltz, Munk, and Flourens agree in thinking that the most important cerebral functions, from which we conclude mental functions, cannot depend on definite sections of the cerebrum.

If we accept the doctrines that individual sensations or ideas exist only as members of a connected, conscious series, and that consciousness, therefore, can never be conceived as a mere sum or product, and if we believe, with Hume, that consciousness is a mere succession of ideas without inner bond or connexion, or that it is the series of our actual sensations (John Stuart Mill), it may be thought possible that there are individual nervous elements which possess isolated and distinct forms of consciousness.

From pathological conditions we do not appear to obtain evidences which may be termed conclusive; nor do such evidences prove to us that consciousness is confined to any supreme part of the cerebral cortex.

In the future we may possibly be able to answer the question, Are the so-called "motor centres" and "sensory centres" really the centres for pure motor and sensory events? or, in the words of Waller, "Have we reached any true dead end to knowledge in the conclusion that the cerebral cortex contains 'sensory centres' and 'motor centres,' and, if so, what signification do we attach to these final terms?"

We have ample experimental and pathological evidences, that the mind can see, feel, and will, in spite of physical and structural discontinuities of the brain-cortex. Destruction of any brain-areas is, so far as our knowledge goes at present, not necessarily attended by alteration in the subjectivity of the mind. We may destroy, extirpate, or sever the connexions of any of the cortical areas, and we can thereby cut off present supplies, or even render the results of former activities inert (memory); beyond this, however, we cannot go.

There is no way of evading the difficulty of adapting the subjectivity of the mind to anatomico-physiological data. When we advance in our knowledge of the data of consciousness, and the relations of the various structures of the brain, we may be able to formulate some more definite doctrines as to cerebral localization. In the meantime, however, we can offer no ultimate solution of the one great difficulty. If we localize determinate activities within restricted areas, we become responsible for an account of some supreme site, in which the mental correlates of these activities are viewed by the subject. Or if, on the other hand, we are indifferentists, we find ourselves confronted with the problem of having to account for the mode by which the subject obtains its view of the objects correlated with physiological activities in widely-apart localities. It is needless to say, that whichever view we take, our difficulty will be in reaching the truth, so far as the subjectivity is concerned.

The answer of cerebral anatomy and physiology to the question of localization of consciousness has, therefore, been of little value hitherto, and we are as yet unable to account for the phenomena of the subjective mind by the study of material structures. The mere fact that the formerly experienced contents of consciousness are rendered inert or incapable of being revived (as in cases of word-blindness and word-deafness, &c.) proves nothing, inasmuch as we are unable to prove whether the contents themselves are destroyed or only rendered inert through lesions of the tracts through which their causal or effectual activities have been, or should be, transmitted.

Let us take, for example, the conclusion of one experimental investigator,

that sensitiveness is the only specific energy common to ganglion-cells; and the inference drawn from this conclusion, that sensation is, in some way or other, the outcome or accompaniment of such a specific energy. Meynert and Munk both believe that—in order to explain conscious movement (volition)—it is sufficient to postulate sensations of innervation. Munk says “Intelligence is located everywhere in the cerebral cortex, and nowhere in particular,” and Meynert corroborates this view by adding that “Memory is the common property of all cortical cells and fibres which are able to receive and conduct external stimuli of all sorts.”

The word “motion,” in its broad sense, suggests itself to us as the executive operation which is correlated with the events of consciousness. The ultimate activity of that motion, so far as we can understand it, is intrabodily, and seen in the working of the elements or units of the cerebral cortex. The unit, predisposed by its inherent and fundamental capabilities, would require the motion to be specific, i.e., the mere dynamical operation of force irrespective of specific quantification is not sufficient to determine consciousness. The agitations set up must be of a certain fixed quantity. The rapidities, sizes and directions of the movements must be specific, not only for each sense, but also for every modification of the activity each organ conditions. The vibrations may be horizontal in some sense-organs, in others perpendicular. The rapidity of the vibrations varies greatly even in different fibres of the same sense-organ. No matter how much we discuss the intraquantifications of motion, we are forced to confess that they are ineffective for the conditioning of human consciousness; in fact, such forms of movement are antagonistic to those particular quantifications or specific movements with which consciousness is correlated.

The activity of our sense-organs is undoubtedly determined originally by cosmical operative motions outside the body; but, although these outer motions may tend to influence our physical organization, they have to be translated, changed, or modified, exoneurally, before they can determine consciousness.

Of the numerous modes of motion to which science has given the names of gravitation, heat, electricity, chemical affinity, &c., the quantity of force is a fixed sum, and even though the ideal limits of science were reached, and the statical and dynamical relations, supposed to connect the cosmical activities with organical operations, were estimated, we should still be without the slightest explanation of how the physical organization inherently develops the specific forms of motion upon which our consciousness is superimposed.

By the study of anatomy and physiology, we gain knowledge of an apparatus which is possessed of certain statical and dynamical relations, and we may further regard such an apparatus as being necessary for specific dynamical relations for the occurrence of consciousness—that is to say, we have to deal with three factors of extreme difficulty. First, we must comprehend, in detail, the structures which form a basis for the statical and dynamical activities; secondly, we must determine the specific rapidities, directions, &c., of motion, which, although not immediately determining consciousness, form the functional activities of our nervous organism; and, lastly, we must determine, over and above mere statical relations, the fundamental and specific mode of motion, if any, which is directly associated with consciousness.

Mercier has forcibly drawn attention to the fact that any attempt at a minute or detailed explanation of the physiological determinate conditions of consciousness will only result in “nonsense.” He says: “A process of change



in the nervous system cannot cause a change of consciousness; such an effect is unthinkable. Nor can a change in consciousness cause a change in the arrangement of the molecules of the grey matter; such an effect is equally unthinkable." We agree that such causal relations are unthinkable.

The prediction of Ladd, that some of the most widely accepted of the physical formulæ are destined to be thoroughly shaken up in the not-far-away future, can never be fulfilled, because (1) no physical formula is conceivable; (2) no data are afforded to us for the construction of such a formula; and (3) even though science should reach its ideal limits, and furnish us with such data, the construction of a formula would, and could, only result, in its conception by us, as a formula pertaining to the domain of physics. Mercier says that the student, when he has fully grasped the significance of the two notions—the "absolute separateness of mind and matter," and the "invariable concomitance of a mental change with a bodily change"—will enter on the study of psychology with half his difficulties already surmounted. The other half of the student's difficulties, we assume, will afterwards be found in elucidating the facts upon which the "invariable concomitance" theory is founded. Our conception of the inter-happenings of mental and physical activities is at present so vague that our powers must be devoted to their consideration, without regard to any conception which extends beyond our scope of inquiry. Science acknowledges the imagination as its most powerful helpmate and we are often forced to rest content with probabilities. Here, however, our part is merely to demonstrate the concomitance of the mental and the bodily events.

To comparative anatomy and embryology we may look for further data as to the origin and subsequent development of the primary oral ring of nerve substance and the subsequent evolution of the nervous system. It seems probable that the tube of the infundibulum was the original œsophagus and that the ventricles of the brain must have been the original cephalic stomach into which the œsophagus opened and became the canal of the spinal cord with its termination at the anus by means of the neurenteric canal. The non-nervous epithelial tissues lining the central canal of the nervous system in man would appear to be remnants of the alimentary system in lower forms of life, and as the brain mass has gradually evolved, the structures and functions of these non-nervous tissues have become modified. Although the mechanism of cerebral peristalsis differs from that of alimentary peristalsis, their origin and development, considered together with the intimate and causal relationships between visceral and nervous phenomena, open a wide field to conjecture, especially in dealing with abnormal mental states.

With reference to individual nerve elements and the mechanism of their stimulation, reaction, nutrition, metabolism and excretion, it must be confessed that our knowledge is still somewhat chaotic. Starling speaks of energies of living cells derived from energies of substances in solution as being measurable in terms of osmotic energy, when taking place in the interior of a cell, and of surface energy when occurring at the dividing surface between the living cell and its environment.

I, for my part, see in the nerve cell two distinct elements, namely: the neuroplasmic and the trophoplasmic elements. It appears warrantable to insist upon recognition that there are two distinct mechanisms in connexion with the nerve cell, i.e., a neuroplasmic element consisting of nerve fibrils within the axis-cylinders, which have, for their protection and nutritional processes of waste and repair, lining sheaths with nuclei within them which

constitute their trophoplasmic elements. The neuroplasmic fibrillæ are continuous, whereas the trophoplasmic elements are discontinuous at the nodes of Ranvier.

This fact alone should indicate the nature of the trophon, and instead of the neurons being isolated and distinct it is the trophon which is structurally and functionally distinct. This is further evidenced by the effects of alterations in the supply and demand between the neuroplasm and the trophoplasm. Stimulation of the neuroplasm demands increased activity of the trophoplasm, and cessation of stimulation is followed by cessation of activity, and this in turn means pathological variations not only in the neuroplasm, but also in the corresponding trophoplasm, and propagation in a progressive manner in the direction in which the molecular vibrations run. Similarly the trophoplasm may be influenced, not only by the molecular trophoplasmic demand from within, but also by chemical, metabolic, nutritive, or mechanical agencies from without, and when its trophic centre or nucleus becomes disintegrated or destroyed its structures become disorganized, and its functions cease. Deprived of its trophoplasm the neuroplasm then fails to perform its function of conduction.

I must confess that I see in each nerve cell nothing else than modifications of these two elements, the neuroplasmic fibrillæ and the trophoplasmic adjuncts. A careful study of the anatomy, physiology, and pathology of nerve cells has led me to the conclusion that each nerve cell is simply a station or junction where the neuroplasmic fibrillæ are structurally continuous, and have for their common use a comparatively large trophoplasm which possesses its nucleus, nucleolus, and plasmic prolongations for a certain distance along the fibrillæ. These trophoplasmic prolongations vary in their extent according to the arrangement of the neuroplasmic fibrillæ which they envelop. In the neuraxon, where the far extending fibrillæ are gathered together in the axis cylinder, the enveloping cellular trophoplasm is soon brought into contact with the trophoplasm of the fibre; in the dendritic processes, on the other hand, the trophoplasm extends along the neuroplasmic fibrillæ for some little distance, and where the fibrillæ leave the main clusters and branch off at right angles (as in the neuraxon) the myelin of the trophoplasm extends for some short distance along them, and constitutes (under hardening processes, staining, &c.) the so-called thorns or granules.

So far as the nerve cell itself is concerned the disposition and arrangement of its substance leads one to believe that the reticulated appearance of its outer surface is connected with the trophoplasm, and the elements of this reticulum are concerned with the intake and output of the trophoplasm's fluid constituents. The arrangement and appearance of the chromophil substance (Nissl) within the trophoplasm leads one to believe that it is probably (as almost conclusively proved by pathology and experiment), during the active life of the cell, of fluid consistence and having biochemical and possibly also mechanical, relationships with the nucleus and nucleolus. Within the body of the trophoplasm there is evidence of the presence of neuroplasmic fibrillæ which run from one process to another. Each of these bands of fibrillæ is probably protected (or insulated) from actual contact with the fluid of the trophoplasm by a thin covering of myelin. By careful study of the disposition of the Nissl bodies it would seem that each neuroplasmic bundle of fibrillæ contained within each trophoplasmic process of the cell divides on entering the body of the trophoplasm or cell, so as to send fibrillæ to each of the other main processes. If we construct a diagram on this assumption, the appearance and shape of the Nissl bodies would suggest

to our minds that they occupy the trophoplasmic lacunæ between the neuroplasmic fibrillæ, and, as previously stated, that during life they were of fluid consistency and essentially trophoplasmic in function.

Should these suppositions be correct, we may conclude that the long-continued discussion as to the individuality of the neuron loses most of its difficulties, inasmuch as we have in reality to deal with two factors, the neuroplasm, which is continuous through the cell and functions therein by continuity, and the trophoplasm, which is contiguous and functions autonomously by reason of its internal relation to the neuroplasm, or allonomously by reason of its dependence on biochemical and mechanical influences derived from without.

Nerve cells, viewed as trophons, like all other cells may lead more or less individual lives, i.e., they breathe, they assimilate, they dispense their own stores of energy, they repair their own substantial waste; each is, in short, a living unit, with its nutrition more or less centred in itself. I know of no functions other than trophic and protective in connexion with the trophoplasmic nucleus, nucleolus and plasma. That a nerve cell disintegrates and dies when it loses its trophoplasm, and that its death is followed by disruption of the neuroplasmic fibrils, are facts which merely go to prove that the trophoplasm is essential to the conductivity of the neuroplasm, and nothing more. Further, I know of no data which go to prove that the neuroplasmic fibrillæ in any of the cells of the cerebro-spinal system become structurally modified so as to indicate that they are anything other than lines for the conduction of waves of physico-chemical disturbance, nor do I find that, in any part of the course of the neuroplasmic fibrillæ, either in the cells or in the intercellular network, there is any evidence whatsoever that at this, or at that point or region, physical and psychical phenomena may come into apposition.

That elusive phantom "consciousness" evades us no matter where we search, and although functional pre-eminence has been given to the nerve cells of the cortex, I am totally unaware of any positive proof of the truth of the supposition that the nerve cell itself in some mysterious way not only serves as a conductor but also as a generator of impulses, and that to its internal activities may be ascribed all the phenomena of consciousness. Of recent years, and as an outcome of the neuron theory, the term synapse has been employed to represent the mode of nexus between neighbouring neurons. Whether there is homogeneous continuity of the neuroplasmic fibrils has never been determined. In the *Medusae* each nerve cell is said to join its neighbours much as at a node in the myelinated nerve-fibre, where the axis cylinder of each segment joins the next. The delay in interneuronic conduction of electrical stimuli, as is said to occur by some observers, is as yet uncertain in its bearings on the question.

To the psycho-physicist the phenomena of the transmission of stimuli to and from widely separated localities in the cortex, as must occur in association with mental processes, anything less than functional and even structural continuity is scarcely conceivable. In fact, the phenomena of nerve conduction and the phenomena of trophic energy, although mutually interdependent, have different modes of being. The former demands structural and functional continuity, whilst the latter implies structural and functional discontinuity. Each neuroplasmic fibril demands not only a means whereby its chemico-physical properties may be maintained, but also a means whereby its specific energies may be protected from disturbance at points other than at the periphery. The neuron theory has never been adequate to explain the facts derived from the study of the anatomy, physiology, and pathology of the

nerve-cell, and it ought, in my opinion, to be supplemented by the "trophon theory," which would, at least, provide a more intelligible explanation of many phenomena. The trophon is, to all intents and purposes, the structural and functional equivalent of a pulsatory lymphatic heart, deriving its activities and energies (1) from chemico-physical changes within the neuroplasm, and regulated in the discharge of its trophic functions by the chemico-physical or metabolic properties and controlling mechanism of the nucleus and nucleolus, and (2) from without, by reason of osmotic or other interchange between its internal fluids and its external surrounding fluid medium. These semi-fluid filmy trophoplasms or trophons are probably, during life, subjected to variations in pressure, derived not only from chemical and molecular physical influence, but also by mechanical influences acting on their walls, and due to the pulsatile movements derived from arterial systole and diastole. Lastly, as a protective mechanism, they not only provide a protective myelin capsule for the neuroplasmic fibres, but they serve as protective mechanisms against the encroachments of foreign bodies, which may be microbic, leucocytic, or merely in the form of detritus in the lymph lacunæ.

It would appear reasonable to conjecture that many morbid states of consciousness and reasoning are due to disruptions or short circuiting of currents in nerve tracts whereby their continuity or habitual service becomes disorganized. The phenomena of diverse mental abnormalities, ranging from multiple personality to states of delirium, are possibly due to such defects originating either in the trophons or the neuron.

When we consider the pathology of the nerve cell we witness in the phenomenon of vacuolation of the trophon—occurring, as it does in various regions, in connexion with epilepsy, chorea, and occupational spasms—a pathology which depends upon efficiency of the mechanism of osmosis. Certain it is that the trophoplasmic elements of the nerve cell must be affected by the pulsatile movements of the cortex, and doubtless the mechanism of osmosis will in the future become more clearly understood.

In the Presidential Address read before the members of the Chelsea Clinical Society, I devoted some attention to the "Intracranial Mechanism in Health and Disease," but it seems appropriate that I should here refer again to some of the problems awaiting solution, which I discussed on that occasion.

In dealing with the ætiology and pathology of disease one of the first considerations ought to be to determine, if possible, the existence of physical or mechanical factors of causation. Functional integrity of the various systems of the body depends on the maintenance of balance between stimulation, chemical change, metabolism, nutrition and excretion. For present purposes, however, I propose to exclude from consideration all problems of chemistry and metabolism, and of the rôle of the endocrines, and to deal merely with the mechanism of intracranial supply and demand.

First, we have to ask, is the brain a generator, or merely a transmitter, of energy? I will preface my remarks with the assumption that the nervous system is a mechanism or instrument for the transmission of force (the so-called *vis nervosa*) and is not a generator of force. The living organism can neither create nor destroy energy, and the nervous system can serve merely as a transmitter or transformer. The excitability and sensibility of living matter provide us with the first rudiments for adaptive reaction for the preservation of life in the substance stimulated. The stimuli themselves are derived from the external world and the reaction to them is derived from physico-chemical change. With regard to the intracranial contents it must

be granted that each and every content, be it liquid or solid, possesses both static and kinetic energy.

The transformations in connexion with biochemical and trophic changes, although offering vast and fascinating fields for research, involve considerations which are beyond the scope of this address. For present purposes we may assume that energy may be transformed from nervous energy to trophic energy, or vice versa, but its total amount remains the same. Each system has a definite amount of static energy, and any loss in static energy becomes compensated by kinetic energy. When speaking of heat, electricity, and magnetism as other forms of molecular energy into which the static energy of living nerve substance can be transformed, we must recognize that the problems now before us are concerned only with nervous matter, as such, and with its so-called "specific energy," i.e., we must exclude energies of exoneural origin which, although they may determine, and be propagated as, specific energies, are transmitted through nerve channels to muscles in which another mode of transformation takes place, with liberation of kinetic energy in non-nervous matter as the result.

I desire here to emphasize the proposition that nerve fibrillæ are purely conductory in function. The static unit of the nervous system has usually been regarded as existing in connexion with the neuron. The neuron is, however, a complex organization or symbiosis, and is something more than a static or dynamic unit, i.e., it possesses as we have already seen, organotrophic as well as nervous properties which must be differentiated, since each group has its own laws and methods of functioning. It is probable, therefore, that the nerve cell or trophon *per se* does not initiate energy but serves merely as a mechanism for the support, protection and nutrition of the fibrillar elements which pass through it and which serve as conductors of energies generated elsewhere. Morat ("Physiology of the Nervous System," p. 48) has stated very clearly that: "The body of the cell of the neuron is an organ necessary for the organization and conservation of the latter, but it takes no necessary and direct part in its power of functional activity, so-called."

The true static unit of nerve must, in common with other units, possess chemical, caloric, and electric force which undergoes transformations when the specific energies of the nerve units are called into play; but of the nature and conduction of these energies little is actually known. The experiments of Rolleston, Stewart and Boeck (who endeavoured to estimate by means of an apparatus sensitive up to 1-5,000th of a degree, the amount of heat given off by an isolated nerve trunk) gave no results. Many physiologists agree with Hermann that electric currents do not pre-exist; and that any currents which do occur have a chemical source and arise under conditions which are entirely artificial and exoneural. The various modifications of the body of the cell during repose and functional activity and the phenomena of chromatolysis pertain to the trophon and not to the static units of the nerve proper, which are relatively almost incapable of fatigue.

In connexion with the conduction of electromotive force through nerve tracts there is still much to be done. If two of the following bodies, viz.: carbon, platinum, gold, silver, copper, iron, tin, lead and zinc be brought into contact through the medium of a nerve tract, in one of them positive electricity, and, in the other, negative electricity can be detected. In the skin of the frog the outer surface is +, the inner is — (du Bois-Reymond); and the same is true of the intestinal tract (Rosenthal), the cornea (Grunhagen), and the non-glandular skin of fishes (Hermann) and molluscs (Oehler). Currents are also

manifested by glands. Lately I have endeavoured to obtain some data with regard to the conduction of electromotive force through nerve tracts, obtained for me by Walter Donne, and the instruments have been: (1) a unipivot galvanometer; (2) lamp and scale outfit.

(1) *The Unipivot Galvanometer* has a moving coil supported on one pivot only instead of two as is usual in moving-coil galvanometers. The unipivot principle was introduced by R. W. Paul in 1903, and by its adoption the pivot friction was found to be less than half that met with in double-pivoted instruments. The sensitivity attained is unique among pivoted instruments and they can replace reflecting galvanometers, e.g., a 1,000 ohm unipivot can be made to give one scale division deflection for 0'0000002 ampere, or i.e., it will measure up to 2,500 megohms at 500 volts. As a low-reading voltmeter one division can be made to correspond to 0'0002 volt.

The necessity for high sensitivity is obvious in measurements of insulation, of small electrical pressures, and in general testing and laboratory work. Unipivot voltmeters and millivoltmeters are constructed with high internal resistance (coil resistance 50 ohms) and practically no temperature co-efficient. A rise of temperature of 1° C. increases the current sensitivity by only 0'03 per cent.

Electrical and mechanical interchangeability is aimed at, e.g., the left hand or upper terminal is always positive, and the normal internal resistance of any millivoltmeter for 0 to 120 (or 0 to 150) millivolts is 500 ohms, and for the lower ranges proportionately less.

(2) *Lamp and Scale Outfit*.—A straight filament 4-volt lamp is used to project on to a mirror which refracts on to a scale of twenty-five divisions numbered 0 to 25 and corresponding to a range of 250 micro-amperes or 2 to 5 millivolts.

The nature and results of these experiments are as yet but imperfectly determined, but many points of interest have already been elicited, the chief of which are that + and — reactions do not always correspond in the male and the female, the amount and rate of conduction of the E.M.F. vary under the influence of muscular activity and fatigue, and the conduction is effected almost entirely through the flexor surfaces. The mechanism is of so delicate a nature that it becomes possible to estimate the conduction through individual nerve tracts and to facilitate thereby the localization of the resistance or obstruction to the passage of the E.M.F.

The following table of reactions as estimated by Walter Donne is somewhat interesting, but the explanation of the phenomena is as yet obscure:—

| Electrodes    |     |               |     | Reactions |     |        |                  |
|---------------|-----|---------------|-----|-----------|-----|--------|------------------|
| Left          |     | Right         |     | Male      |     | Female |                  |
| Copper ...    | ... | Copper ...    | ... | +         | ... | -      | + occasionally + |
| Aluminium ... | ... | Aluminium ... | ... | -         | ... | +      | „ various        |
| Lead ...      | ... | Lead ...      | ... | +         | ... | -      |                  |
| Carbon ...    | ... | Carbon ...    | ... | +         | ... | -      | +                |
| Silver ...    | ... | Silver ...    | ... | -         | +   | ...    | +                |
| Carbon ...    | ... | Copper ...    | ... | +         | ... | +      |                  |
| Copper ...    | ... | Carbon ...    | ... | -         | ... | -      |                  |
| Carbon ...    | ... | C. Leclanche  | ... | -         | ... | -      |                  |
| C. Leclanche  | ... | Carbon ...    | ... | +         | ... | +      |                  |

With two copper wires to the galvanometer the following reactions are obtained: Apple +, onion —, leek —, grape —, potato +, tomato +,

orange —, orange skin +. It is also to be noted that the reactions to roots and stems vary considerably.

The most recent work in this connexion is that of Dr. A. White Robertson, whose observations and experiments cover a wide field, and go to prove that under certain conditions electric currents are released from animal and vegetable tissues. That an insulating material exists in the skins of fruits does appear to be a fact, and doubtless the animal tissues have a similar conserving mechanism to prevent leakage both intra- and extra-bodily. How far this leakage goes to determine decay in both animal and vegetable tissues is yet to be determined, and it is probable that further experiments may elicit remarkable data with regard to local or even general processes of decay. Certain it is that the bodily mechanism depends upon its electrical activities for its proper functioning, and my own experiments go to prove that not only is insulation more complete on the dorsal or extensor surfaces of the body but also that the liberation of electricity is greatly influenced both by muscular effort and by fatigue.

Whether fresh foods are more active electrically than those which are preserved is a question which must be solved by further experiment, and, needless to say, this problem is destined to become one of vital interest to dietetics.

In seeking possible causes of disease some members of our profession neither balance proportions nor regard the problems before them in their entirety. The specific origin of some of the progressive paralyses of the insane is fairly well elucidated, and similarly the ætiology of tuberculous affections is made manifest. With regard to cancer, however, we witness much crude guessing with undue balance in the evidence adduced, e.g., in the latest theory its causation is assigned to preserved foods, regardless of the fact that to these foods protective measures against impurities and infections are afforded far exceeding those accorded to other articles of diet. It seems possible that in cancerous and other conditions the local and cachectic results are in great part either due to, or aided by, leakage of the vital electric forces which determine degenerative forces similar to those which occur in vegetable substances when the insulating mechanism has become inoperative.

Lugaro, in his memorable work on "Modern Problems in Psychiatry," says that he whose custom it is to indulge in fatuous enthusiasm and pretentious language will possibly, at every inaugural discourse—either on the occasion of a congress in psychiatry, or at the opening of a conference—hail with a flourish of trumpets the tremendous progress made and the results accomplished, and extol the pre-eminence of psychiatry over other branches of clinical medicine: but there can be no doubt that the progress of psychiatry has been slow, laboured and rather unsatisfactory. That the science of psychiatry has not as yet attained such satisfactory results as those of surgery and several departments of medicine is not to be wondered at when we remember that psychiatry is not only a science of extreme complexity in itself, but also embraces all the most intricate problems of biology and medicine. Lugaro fully appreciates this difficulty when he states that the intimate contact which psychiatry assumes with the most varied doctrines and with the most diverse fields of social activity raises a host of current scientific, religious, metaphysical, philosophical and ethical prejudices against it, which paralyse its efforts and hamper its progress. Clinical psychiatry has the two-fold task of considering not only the psychic symptomatology of mental diseases (and this must, of necessity, go hand in hand with the study of normal psychology).

but also the anatomical, physiological and pathological conditions which form the organic process or essential basis.

The history of clinical psychiatry consists in great part of the perhaps somewhat futile efforts of innumerable writers to bring within one comprehensive classification all the phenomena of mind and brain, taking note of the data of development, mental and physiological symptomatology, together with the various factors of causation. Little wonder is it, therefore, that each new classification, instead of leading to real progress and clearness of understanding, has merely added to the prevailing confusion and obscured issues of greater importance. Thus it is that many of the more recent works on clinical psychiatry, whilst possibly adding to the sum of our knowledge, have often been merely repetitions of data already acquired, but played out in new hands after a re-shuffling of the cards. To this tendency in modern psychiatry, the benefit to be gained is apt to be negated by the confusion and delay so occasioned. Our danger appears to be, when dealing with the human mind and body, that we should think in text-book terms and thereby tend to lose sight of processes. I would express this more tersely by stating that mere labelling does not imply delivery.

Within the last few years some of the riddles of the psychoses have been newly propounded through the medium of Freud, of Vienna, and undoubtedly his methods have in some instances led to a more thorough understanding of the origin and development of various abnormal mental states. To Bleuler, Jung and their co-workers at Zürich, and to many others of the Freudian school, we are indebted for further elucidation by analysis of many psychical problems.

When we review the data of physiology of the nervous system, we see how much we have been indebted to the destruction or excitation of macroscopical parts made accessible by animal experimentation. Microscopical anatomy has also advanced to such an extent that individual morphological types of nerve cells have not only been recognized, but have occasioned many vague conjectures as to their nature. The homologues of structure of the cells in the cerebral cortex have led some observers to conclude that in the nervous system there are not any two cells or any two fibres possessing absolutely identical function. That the cerebral cortex is a vast field for further pathological research no one will deny, and it is possible that in the future we may yet be able to draw more satisfactory inferences regarding the functions of single units than we should be justified in doing to-day.

It may be stated with truth, however, that up to the present the anatomical facts already noted, instead of elucidating the problems as to the physical basis of mental processes, tend rather to increase our difficulty in understanding the complex mechanism of the nervous system.

I wish it were possible to deal with many of the problems which meet the psychologist at almost every turn. It must suffice, however, to mention but briefly one of the more important of them, and I would without hesitation give precedence to the problem of the "origin of specific qualia," a problem which is to psychology what the "origin of species" is to biology. The ability to perceive qualitative differences between different sensations is a psychological fact; but by no process of the imagination can we construct even a seemingly valid hypothesis to account for the perception or discrimination between different sensations, and we are compelled to assume that the brain mechanism is, as an instrument, incapable of affording a solution to the problem. Sensations are qualitatively as distinct as the notes from a piano and one cannot be evolved from the other.



Science and religion are coming more into accord as our knowledge of realities becomes more manifest and it is not unwarrantable to foresee the possibility of creative and emergent evolution coming into complete agreement. Julian Huxley, in his "Science and Civilization," says: "Man is yet near the beginning of his evolutionary career, and has before him vast tracts of time to set against the vastness of his tasks." I agree with Dean Inge that these words are quite justified, and may form the basis of a rational optimism. As Huxley says: "God is not, in His inmost nature, involved in the time process: He is above it, in the realm of absolute and eternal values. Planets and suns 'shall wax old as doth a garment; and as a vesture shalt Thou change them and they shall be changed; but Thou art the same and Thy years shall not fail.'" Science itself needs this background of eternity.

## Section of Psychiatry.

President—Dr. THEO. B. HYSLOP, F.R.S.E.

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### The Treatment of General Paralysis of the Insane by Malaria.

By HENRY J. MACBRIDE, M.D., and W. L. TEMPLETON, M.B.

THIS paper is offered as a contribution towards the recorded preliminary results of the treatment of general paralysis by malaria, and is based upon a series of eighteen cases.

It is of interest to note that the method was suggested by Wagner-Jauregg as far back as 1887 [1] but was not systematically employed by him till 1917, when nine cases of general paralysis were inoculated with benign tertian malaria. The experiments were interrupted "in order to await results," and were not resumed until September, 1919. The improvement in the nine cases was so striking, that since this latter date all cases of general paralysis admitted to the Wagner-Jauregg Klinik have been treated by malaria, as soon as the condition has been diagnosed.

The principle depends on the fact that remissions in chronic disease may occur after an attack of an acute specific fever. Many methods of producing fever artificially have been tried with varying results. Wagner-Jauregg [2], according to Pilcz, after a thorough investigation of the various methods employed and of the cases of so-called "spontaneous" remission, has drawn up a scale of efficiency as follows: (1) Chemical substances, e.g., sodium nucleinate, (2) toxalbumins, e.g., tuberculin and staphylococci, (3) acute disease. The last group was found to be the most efficient.

The rationale of none of the various methods used is as yet known. In contrast to some, e.g., sodium nucleinate, and "phlogetan," in which the essential factor seems to be a neutrophil leucocytosis [3] with or without a pyrexia, it is to be emphasized, that in malaria a leucopænia and not a leucocytosis is the rule and this has been confirmed in many of our cases. It has been suggested that the improvement obtained by virtue of intercurrent disease or of artificially produced fever, is due simply to a quickening of metabolism with the removal of accumulated waste products. On the other hand it may be that high temperatures exercise some destructive influence on the spirochætes in the body, or again that the destruction of the red blood corpuscles, followed by rapid regeneration from the hæmopoietic centres, may lead, as suggested by Muhlens and Kirschbaum [4], to a non-specific antibody production. The

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biological relationship between the plasmodium and the *Spirochæta pallida*, both protozoa, must not be forgotten, and one might indeed formulate the hypothesis of a specific antibody production against protozoa. It is of interest to note in this connexion that in a series of cases of dementia præcox, treated by malaria (a full report of which will appear in a forthcoming publication) the mental improvement obtained was of a fleeting nature, in contrast to the more permanent and progressive results obtained in general paralysis; all of the dementia præcox subjects relapsed within three months.

As a method of producing fever, experiments have been made with benign tertian malaria, tropical malaria and relapsing fever. Of these three, benign tertian malaria was found to give the best results, no particular advantage being obtained from the higher temperatures of relapsing fever [1].

The method employed in the Wagner-Jauregg Klinik, in Vienna, consists in the withdrawal of from 2 to 4 c.c. of blood from the vein of a patient suffering from benign tertian malaria and in its subcutaneous injection into the general paralytic. The site chosen is the area between the scapula and the spine. It is not essential that the blood should be withdrawn during the febrile attack. By this method, the incubation period varies from one to three weeks, rarely less. In most of our cases blood was injected intravenously; to avoid clotting it is advisable to have the syringe lubricated with liquid paraffin. By this latter method the pyrexia developed in most cases on the second or third day, but ran a somewhat irregular course for a few days and then began to show typical rises every second day, with rigors, &c. In some cases after a varying period—twelve to sixteen days—daily rigors developed and continued. In a few cases, daily rigors occurred from the beginning, i.e., a quotidian fever developed, in this case double tertian. The advantages derived from quotidian fever, viz., that the course of treatment was accordingly shorter and that the fever reactions were more or less continuous, were, in our opinion outweighed by the fact that the patient was continuously prostrated.

Several strains of the malarial parasite have been used in the present series of cases (one strain has passed through ten different hosts), and we are of the opinion that the virulence tends to be increased with each successive transmission. This is in agreement with the findings of Grant [5], and we would consider it advisable, therefore, to renew the strain at every available opportunity from fresh untreated cases of malaria.

The patient is allowed to have, on an average, twelve high fever reactions, the actual number depending, more or less, on the patient's state of health. The fever is terminated by the administration of quinine. Wagner-Jauregg [6] suggests 15-gr. doses each day for three days, and in the fourteen following days half that dose daily. In our experience the first dose of quinine is, in most cases, sufficient to terminate the fever entirely. It has been suggested that the sudden cessation of the fever on the administration of quinine in artificially produced malaria is due to the absence of the more resistant sexual forms of the parasite.

Several cases failed entirely to develop fever, and a few recovered spontaneously without the administration of quinine. It is of interest to note that all of these patients had already suffered from malaria abroad, but there were others who, in spite of previous malarial infection, developed fever in the usual way after inoculation.

In all our cases, following upon the febrile attacks and running parallel with the quinine treatment, weekly doses of novarsenobillon or salvarsan were administered intravenously, six doses in all being given. Though directed

primarily against the syphilitic infection, these injections were remarkably efficacious in improving the profound anæmia which followed upon the malarial paroxysms.

During the course of the fever, epistaxis occurred in a few cases, and herpes febrilis was common. In a minority only was the spleen ever palpable. Acute retention of urine was a troublesome complication. Three cases had quite severe vomiting at the height of the individual fever reactions. Anæmia, of course, was profound in all of the cases, but recovery was rapid on the termination of the fever. Unfortunately we have to report three deaths which occurred during the course of the treatment. Other observers have reported a considerable number of deaths, but give no details. Gerstmann [1], commenting on twelve cases in which the patients died, states simply that in none of the cases did the post-mortem examination reveal any evidence of death from malarial infection. Grant [5], quoting six deaths in forty cases, does not state at what stage of the treatment these occurred. The details of our three fatal cases are as follows:—

*Case I.*—Male, aged 35, admitted October 6, 1923. He had malaria in 1916, but otherwise enjoyed good health till about one year before admission, when he became very irritable and hyperexcitable. Three months before admission he had a seizure, but felt quite well the following day. After an interval of two months he had two seizures within half an hour of each other, in which he bit his tongue and had incontinence of urine. During the few months before admission the patient often fell down owing to a sudden giving way of his legs, but there was no loss of consciousness.

On examination no abnormalities were found in the cardio-vascular, respiratory, alimentary or genito-urinary systems. The pupils reacted well to light and showed no abnormality; tremors were present in the lips and tongue; dysarthria was marked; pin-prick showed widespread diminution; motor power was good; tendon reflexes were absent; plantars both flexor; sphincters normal; Rombergism slightly positive; Wassermann reaction strongly positive in the blood and cerebro-spinal fluid.

Mentally, he was able to give a fair account of himself, and had a fair insight into his condition; memory poor; disorientated for time and place; emotionally unstable.

He was inoculated with 2 c.c. benign tertian malaria intravenously, October 13. Six hours after, he had a seizure, and his temperature rose to 107°, when he immediately went into a status epilepticus. From this he passed into a semi-comatose condition, and died October 19, in spite of intravenous quinine medication, &c.

*Case II.*—Male aged 33, admitted July 24, 1923. Patient complained of loss of memory. His wife stated that for the last year he had been mentally confused and had an attack, several months before admission, in which he lost his speech for twenty minutes without loss of consciousness.

On examination, the cardio-vascular, respiratory, alimentary and genito-urinary systems appeared normal. The pupils were unequal and did not react to light, but did on accommodation; pinprick was diminished generally; motor power good; knee-jerks absent; plantars both flexor; sphincters normal; Rombergism slight.

Mentally he was very confused and childish; memory bad; emotionally very unstable. He gave a very poor account of himself, and was a very poor witness. The Wassermann reaction was strongly positive in the blood and cerebro-spinal fluid.

On August 7, the patient was inoculated intravenously with benign tertian malaria. After a slightly irregular fever the true fever developed October 12. From this time the patient's mental condition deteriorated with each daily rise of temperature. He refused to take sufficient nourishment and was with difficulty fed per rectum. On October 18, an attempt was made to stop the malaria on account of the progressive prostration of the patient, but the patient died October 21.

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*Case XII.*—Female, aged 42. Admitted August 8, 1928. During the three years previous to admission the patient had been having progressive difficulty in her speech and was losing her memory. Walking was becoming difficult during the same period on account of weakness in the legs.

On examination, the cardio-vascular, respiratory, alimentary and genito-urinary systems appeared normal. The pupils were unequal, the right reacting, the left fixed to light; face and tongue were tremulous; dysarthria well marked; motor power fairly good; outstretched hands coarsely tremulous; knee-jerks brisk; plantars both flexor; sphincters normal. The Wassermann reaction was strongly positive in the blood and cerebro-spinal fluid.

Mentally, the patient was childish and completely disorientated for time and place; memory defective.

An intravenous injection of about 8 c.c. of benign tertian malarial blood was given August 7. A reaction occurred the following day, patient's temperature rising to 102° F. by the evening. The malarial rigors began on August 12, but in the five days previous to this the temperature was always slightly raised in the evening. Ten daily rigors occurred and the patient died after the tenth, in spite of the administration of intravenous quinine both during and after the last rigor.

During the period of treatment the patient emaciated rapidly. The trembling in the hands, which was noted previously, became more marked. The movements became jactitations, the finger and arm jerkings resembling the terminal clonic movements of an epileptic seizure. During the rigors there was considerable cyanosis. The spleen was palpable.

Post-mortem examinations were made in two of the cases, and in these evidence of marked splenic enlargement with bile-staining of the liver substance was found. It may be admitted, then, that these patients died of the malarial infection.

No autopsy was obtained in the other case, in which the patient died in a condition of status epilepticus. It may be that the intravenous injection of a foreign serum caused a certain degree of protein shock sufficient to drive the patient into this state. It is to be noted that in this case there is a history of seizures preceding admission.

Of 350 cases treated in the Wagner-Jauregg Klinik down to February, 1923, [7] complete observations were made in 296 only. Of these, no fewer than 202 showed remissions of varying degree, and 112 showed complete remission with the disappearance of the former mental symptoms, and a return of the former business capacity; 68 per cent. showed remissions, and 38 per cent. complete remissions. A recent personal letter from Dr. Gerstmann states that the treatment is being continued in the Wagner-Jauregg Klinik, with a very similar proportion of remissions.

Of the patients who had complete remissions, three treated in 1917 are still actively employed at business and show no sign of relapsing. These remissions are, therefore, of five years' duration. In seventeen cases, the remissions have already lasted two to three years, and in a further thirty-four, the remission has persisted for one or two years. Of the total of complete remissions three only have relapsed or shown any tendency to relapse.

Many of the advanced cases of the disease gave only very incomplete remissions, but even in those much benefit was obtained, particularly in the cessation of acute symptoms and in the non-progression or even regression of the dementia. Of those already in a profound state of dementia, many again became sociable and inoffensive and regained a certain degree of occupational ability.

The remissions do not follow immediately upon the febrile attacks and, indeed, Gerstmann states that the first sign of improvement may not be visible

for weeks or months after the termination of the fever. One must, therefore, be especially careful not to come to any definite conclusion on cases treated until sufficient time has elapsed. The course of remission is held to be progressive, without further inoculation, though this may be necessary in cases which remain stationary, say after an interval of six to twelve months. Cases which showed an incomplete remission, when re-examined months later, showed a full and complete return to the normal.

There is, apparently, no parallelism existing between the clinical and serological findings. Many who show a complete remission of symptoms, have still a positive Wassermann reaction in the blood and cerebro-spinal fluid, and a few of those when examined months later have shown a negative result in both. It is found that the blood becomes negative before the cerebro-spinal fluid, and that improvement is first seen in a reduction of the number of cells in the fluid. To bear out this absence of parallelism, Gerstmann quotes a case successfully treated by the tuberculin-mercury method in 1909, in whom a complete remission still exists. When this patient was examined recently, the report was as follows: Wassermann reaction positive in the blood and cerebro-spinal fluid, globulin test positive, cells 186.

*Case XVI* of our series, a case of non-progressive general paralysis, is of interest in this connexion:—

Male, aged 48. Admitted March 9, 1916. The disease was of eighteen months' duration on admission. On examination, the general systems showed nothing abnormal. Nervous system: Pupils unequal and Argyll-Robertson; speech slurred; hands showed a coarse tremor; knee-jerks brisk; plantars right extensor, left flexor; Wassermann positive in the cerebro-spinal fluid and blood. Mentally, the patient was exalted and had grandiose ideas.

July, 1923: The patient was weak-minded and childish; irritable, a-social and occasionally faulty in his habits. For about a year, his state had been apparently quiescent. Just before his inoculation with malaria, his blood and cerebro-spinal fluid were again examined, and found to be completely negative. In spite of this, however, the induction of malaria was proceeded with and he had twelve rises of temperature above 103°. During the fever paroxysms, he improved mentally, became more agreeable, more rational in conversation and clean in his habits. This improvement persisted for about six weeks only, and three months later he had completely relapsed.

According to the Vienna report [1] the results of treatment depend essentially on the stage of the disease—the earlier the stage, the more certain is the remission, and in cases of short duration Wagner-Jauregg claims that entire success can be predicted with almost absolute certainty. According to Gerstmann, the types of case giving the best results are those of simple dementia and tabo-paresis; Pilez [2] adds to this the cases with maniacal symptoms.

The development of acute mental symptoms during the febrile stage is regarded as a favourable omen, though in some cases they persist after the fever has terminated. This is especially true of auditory hallucinations, which may persist in spite of an otherwise complete remission.

The importance of an anatomical control of clinical remissions is obvious, and Gerstmann [8] quotes the histological findings in three cases which had shown complete remissions, the patient dying later of intercurrent disease. The histological changes in the three cases were so poorly developed, compared with the customary findings, that without knowledge of the cases the anatomical diagnosis would have caused some difficulty. He comments, too,

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upon the greater infiltration of the temporal lobes, as compared with the other cortical regions, and suggests a possible connexion between this fact and the frequent occurrence and persistence of auditory hallucinations.

Scripture [9], in a survey of the recent results from the same clinic, states that during a year and a half, of 141 cases treated, sixty-two were completely cured. These, he says, were the early cases, in which the disease had not been present for more than two and a half years.

Most of the German clinics have already made use of the method, with apparently good results. Weygandt [10] reported fifty cases with remissions in 88 per cent. and good remissions in 48 per cent. Kirschbaum [11] in fifty-one cases obtained 58·8 per cent. good remissions and 13·7 per cent. remissions of lesser degree—in all, remissions in 66·6 per cent. This same investigator, from a survey of cases admitted previous to the introduction of the malaria therapy, showed that on the average “spontaneous” remissions accounted for no more than 11·4 per cent. of the total. In this connexion, Robertson [12] has called attention to the remarkable decrease in the number of deaths from general paralysis within recent years, and this must be remembered in arguing from statistics as to the beneficial results of any form of treatment.

McAlister [13], reporting recently on a series of twelve cases treated at Morningside, concludes that: (1) Of nine cases, two only were conspicuously improved, the emphasis in one being on the mental side and in the other on the physical; (2) four other cases showed some improvement, although not so marked. The average duration of the disease in his twelve cases was two years, and yet, as the author says, “there is a complete absence of what, even when the term is generously interpreted, might be described as ‘cures.’” He discusses the question as to whether the infection with malaria exerts any particular influence upon the permeability of the choroid plexus to salvarsan.

Gerstmann [14], however, does not consider that the subsequent treatment with neo-salvarsan is at all essential; in fact, it has been repeatedly neglected for various reasons, without disadvantage to the progress of the remission.

Grant [5] has recorded his preliminary observations in a series of forty cases treated during the past fourteen months. Of these, three patients have been discharged to their homes and are now following their usual occupation. Three, formerly wet and dirty in their habits, have ceased to be so. Two patients, formerly confined to bed, have regained their strength to a considerable degree and are able to be up and about.

Of the present series of eighteen cases, subjected to the malarial treatment, two were cases of juvenile general paralysis. Of the remaining sixteen, eleven may be classified as early cases, the average duration of the symptoms being of ten months only; in five, the duration on an average was four years.

### EARLY CASES.

Cases I and II of this series have already been referred to as having had a fatal issue during the course of the treatment.

*Case III.*—A fairly robust male, aged 48. Admitted June, 1923. He complained of shooting pains in the left arm and leg for nine months previous to admission. A fortnight before he was admitted he collapsed while walking upstairs, but was not unconscious. His wife stated that his disposition had altered lately.

On examination, his general symptoms appeared normal. The nervous system showed the following: Pupils unequal and Argyll-Robertson; twitching of the face and

fine tremors of the tongue; dysarthria; diminution to pinprick down the left side; reflexes normal; Wassermann reaction strongly positive in the cerebro-spinal fluid. Mentally he was definitely grandiose in a childish way; memory was poor.

August 8: He was inoculated with malaria and allowed to have twelve rises of temperature. The reaction was severe and the spleen became palpable. After the first dose of quinine the temperature did not rise again.

November 15: The patient was a new man physically. He was stronger and his weight was 20 lb. heavier than before the treatment; speech was more normal; tremors not so marked; memory improved. He was still mildly grandiose.

*Case IV.*—Male, aged 53. Admitted August 9, 1923. For about eighteen months he had progressive weakness in his limbs with trembling; speech had become defective for some months. He had had malaria about twenty years ago.

On examination, the general systems showed nothing abnormal, except for some signs of chronic bronchitis. The nervous system: Pupils equal, Argyll-Robertson; tongue and lips tremulous; dysarthria present; power in the legs slightly diminished; knee-jerks, right brisk, left present; ankle-jerks both absent; plantars both flexor; difficulty in controlling his bladder; Rombergism present.

On August 25, the patient was inoculated with malaria and the first rigor appeared on September 1. He was allowed to have twelve fever reactions and after the first dose of quinine the temperature never rose again. Nearly a stone in weight was lost during the treatment, but this was shortly afterwards regained.

Two months later the patient was less tremulous and his speech had improved.

*Case V.*—Male, aged 54, admitted August 9, 1923. For several months before admission patient found difficulty in doing his work, which entailed a great deal of writing. During this same time his speech had become defective, and his memory impaired.

On examination the general systems appeared normal. The condition of the nervous system was the following: pupils reacted to light and were equal; motor power good; no sensory loss; knee-jerks brisk; plantars both flexor; occasional incontinence of urine.

He could not give a very good account of himself, and was slightly confused; memory defective; dysarthria present.

The inoculation with malaria was given September 14, and the first rigor occurred September 19. The fever remained true to type till September 28, when the rigors became a daily occurrence. After twelve rigors the rise in temperature was stopped with quinine. During the fever, towards the end of the treatment, patient became maniacal with each rise of temperature, was doubly incontinent, and had visual hallucinations. This all disappeared with the cessation of the fever.

Two months later the patient was discharged very well. His speech was very good; memory good and mentally clear. His physical condition was good and still improving. Writing had again become normal, and at present he is convalescing at the seaside before resuming his work.

*Case VI.*—Male, aged 42, admitted June 29, 1923. There was a history of a slight seizure about four months before admission, otherwise the patient felt perfectly well.

On examination his general systems appeared normal. The nervous system showed pupils to be Argyll-Robertson; reflexes normal; sphincters normal; motor power good; Wassermann reaction strongly positive in the blood and cerebro-spinal fluid. He showed marked euphoria, and was very loquacious; there was no insight into his condition; speech fairly good; no delusions.

On July 9 he was inoculated with malaria, and almost from the day of injection he had daily rigors. After twelve reactions quinine was given, when the rigors ceased. He vomited thrice, and developed retention of urine, but otherwise there were no complications.

About two weeks after the termination of the fever he developed marked grandiose ideas, and, shortly after this, hallucinations. His mental condition became such that he had to be sent to an asylum.



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*Case VII.*—Male, aged 50. Admitted July 11, 1923. For several months before admission the patient's manner had been changing, and he had been having difficulty in controlling his bladder.

On examination the general systems appeared normal. The nervous system showed pupils equal, left reacting to light while the right was immobile, but reacted on accommodation; dysarthria was present; motor power good; reflexes normal; sphincters: slight incontinence of urine; Wassermann reaction slightly positive in the blood and cerebro-spinal fluid. He was euphoric and had a poor insight into his condition; memory poor; slightly disorientated.

July 19: Inoculated with malaria, and was allowed to have twelve fever reactions, when quinine was given. During the fever there was marked retention of urine.

For about two weeks after the treatment patient's mental condition improved slightly, and his speech seemed better, but a cystitis set in, and the patient ultimately died November 5.

*Case VIII.*—Male, aged 56. Admitted July, 1923. For over a year he had been unable to look after himself properly, and had marked delusions and hallucinations. He was much troubled with insomnia.

On examination, no physical signs could be found except Argyll-Robertson pupils, and the Wassermann reaction was strongly positive in the blood and cerebro-spinal fluid. Mentally he showed marked euphoria, and had no insight into his condition; speech was fairly good; memory defective, and he had well-marked delusions and hallucinations. Generally he was not so strong as formerly.

He was inoculated with malaria July 19, and allowed to have twelve fever reactions, when the fever was terminated with quinine. During the treatment he slept better than he had done for months, and no hypnotics were required.

Four months afterwards he was a new man in many ways. He was physically very strong; mentally he was not confused, and could do business transactions under supervision; the delusions and hallucinations were still mildly present. He was still making progress when he left hospital.

*Case IX.*—Female, aged 53. Admitted February 1, 1923.

On examination, the general systems appeared normal; nervous system—pupils Argyll-Robertson, ataxic gait; speech slurred; Wassermann reaction strongly positive in the blood and cerebro-spinal fluid. Mentally she was exalted; delusions of grandeur, supposing herself to be Queen of England, &c. For four months after admission she remained in the same condition. Just before inoculation with malaria the patient began to lose some of her ideas of grandeur, but remained suspicious and querulous. She was inoculated on June 28 and was allowed to have twelve fever reactions above 103°. On recovery from the effects of the fever, patient became quite natural; her gait improved and she remained free from her delusions up to the date of her discharge in September. A later report states that the patient remains well.

*Case X.*—Male, aged 47. Admitted June 27, 1922. General systems were normal; nervous system showed the following: Double primary optic atrophy; tremors of the tongue and arms; ataxia; knee-jerks and ankle-jerks absent; Wassermann reaction positive in the cerebro-spinal fluid. Mentally he had marked delusions of grandeur, general exaltation and excitement. Up to the date of inoculation the patient remained exhilarated and deluded, but in a lesser degree than on admission. His memory was defective and he did not know how long he had been in the institution.

On March 31, he was inoculated with malaria and allowed to have seventeen rises of temperature above 103°. During the febrile stage he was unusually garrulous and quarrelsome, but on the termination of the fever this rapidly disappeared. He was then free from delusions; memory was very much improved. Physically, he was very much improved and was much steadier on his feet. The tremor of the arms had gone, though a slight tongue tremor remained. He was discharged September 28.

*Case XI.*—Male, aged 58. Admitted September 29, 1922. On examination the general systems appeared to be normal; the nervous system showed the following:

Pupils, Argyll-Robertson; tremors of tongue, lips, arms and legs; knee-jerks brisk; plantars both flexor; Wassermann reaction strongly positive in the blood and cerebro-spinal fluid.

Mentally the patient was exalted, aggressive and had marked delusions of grandeur. Up to the date of inoculation, he retained all his delusions and remained in the same physical state, as on admission, with the addition of considerable loss of motor power in the legs.

On July 29, 1923, he was inoculated with malaria, and was allowed to have eleven rises of temperature above  $103^{\circ}$ . During the course of treatment, the patient became quite confused, but only on the days of fever, and on the termination of the attacks, he made a rapid recovery.

During October, his memory remained good; he was free from delusions and had a wonderful insight into his previous illness; he was well conducted and agreeable; physically there was an almost complete absence of tremor and he was much stronger on his legs. This patient is awaiting discharge.

#### CHRONIC CASES.

*Case XII.*—This case has been previously described.

*Case XIII.*—Male, aged 35. Admitted August 7, 1923. Since 1918 he has been irritable and hyperexcitable, and during the last two years his speech has become affected. Trembling has been present since the beginning of his illness.

On examination, his general systems showed nothing abnormal; the nervous system showed the following: Pupils unequal, the left Argyll-Robertson; tremors of the lips, tongue and face; motor power good except for trembling in the hands; knee-jerks brisk, otherwise the reflexes were normal; sphincters normal. Mentally the patient was definitely grandiose in his ideas and was very garrulous; memory poor; speech distinctly affected.

He was inoculated with malaria, August 14, but was only allowed to have five rigors, on account of his condition of prostration. A few weeks after the fever the patient showed slight improvement, he took more interest in his fellow patients, but his speech remained unchanged.

On December 11, this patient came to hospital himself and appeared to have a fair insight into his condition. His speech had improved very slightly.

*Case XIV.*—Male, aged 46. Admitted June 12, 1923. General systems appeared normal on examination; the nervous system showed the following: pupils unequal and Argyll-Robertson; tongue tremulous; speech thick; knee-jerks absent; plantars flexor; Rombergism present; Wassermann reaction strongly positive in the blood and cerebro-spinal fluid. He was mildly exalted and his conversation foolish; memory was deficient.

On June 14, patient was inoculated with malaria and allowed to have thirteen rises of temperature above  $103^{\circ}$  F. During the paroxysms he was querulous and irritable.

Recovery from the anæmia was slow, but ultimately his physical condition reached a much higher level than on admission. His gait was much improved, Rombergism being almost absent. Mentally he was much more alert, but had acquired with his malaria very marked auditory hallucinations which remained months after the termination of the fever. His memory was certainly improved.

It is proposed to inoculate this patient again at the end of six months, if the improvement is not progressive.

*Case XV.*—Male, aged 60. Admitted March 19, 1923. General systems appeared normal; nervous system showed the pupils to be equal and to react to light; knee-jerks brisk; plantars, right flexor, left extensor. Mentally he was childish and confused; memory poor; delusions; habits filthy; Wassermann reaction strongly positive in the blood and cerebro-spinal fluid.

The patient was inoculated with malaria July 10. After seven rises of temperature above  $103^{\circ}$  F. the attacks ceased spontaneously. He showed no particular reaction during the paroxysms.

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Three months later the patient was, physically, much stronger; his habits were quite reformed and have remained so ever since the termination of the fever.

*Case XVI.*—This case has been already described.

*Cases XVII and XVIII.*—These were typical cases of juvenile general paralysis with the Wassermann reaction strongly positive in the blood and cerebro-spinal fluid of both. The patients were both inoculated with malaria in July, 1923, but after the termination of the fever, their condition remained unchanged.

### CONCLUSIONS.

These may be summarized as follows:—

(1) In the two cases of juvenile paralysis, no improvement was noted. This is in agreement with the findings of Weygandt [15].

(2) Of the chronic cases, five in number, one patient died during the course of the fever; four patients showed some degree of mental and physical improvement, one, however, quickly relapsed. In another of these cases, the auditory hallucinations which were acquired during the course of the fever still persisted after an interval of four months.

(3) Of the eleven early cases, two patients died during the course of the fever; three patients exhibited very marked mental and physical improvement. Only time will show whether or not these are to be regarded as real "cures." It is noteworthy, that of these patients, one became wildly maniacal with the access of each paroxysm, towards the end of the fever; and another in a similar way showed very marked confusion. In both cases these symptoms cleared up completely when the fever was terminated. Three patients showed considerable improvement, more marked probably on the physical than on the mental side; one patient showed a very slight degree of improvement, which has been maintained; one patient became decidedly worse after the termination of the fever and was removed to an asylum; one patient showed slight mental improvement, but his physical condition became steadily worse. This patient died four months later from intercurrent disease.

Excluding the two congenital cases, of a series of sixteen cases, three only showed an improvement in any way comparable to the numerous "cures" of the continental observers. Considering, however, that improvement is stated to be progressive, it is hardly fair to conclude that the cases slightly improved may not ultimately show a complete remission. We are of opinion that the continental workers are unduly optimistic, but only a widespread trial of the method over a long period and in a large series of cases, will enable us to confirm or confute their findings. Particularly must the treatment be employed in early cases of the disease. Most of the present series of cases were uncertified patients, admitted to the National Hospital, Queen Square, in a comparatively early stage of the disease. The record of the results obtained in these is, therefore, of the greater value.

We wish to express our thanks to the physicians of the National Hospital, Queen Square, and to the medical superintendent, City of London Asylum, Dartford, for their kindness in allowing us to publish the cases.

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## DISCUSSION.

Dr. CHARLES F. HARFORD said he would put forward evidence from a somewhat wide experience of malaria, showing the connexion of the disease with certain psychical phenomena which he believed would be worthy of consideration with reference to the present discussion.

His own personal acquaintance with malaria was chiefly with the malignant type, but he agreed with a previous speaker in believing that the difference in effect between the various forms of malaria was mainly one of intensity.

(1) One distinguishing factor in malaria attacks was the acute onset and the rapid decline. Thus a person previously in the most vigorous health was suddenly attacked with symptoms of the utmost gravity, yet when the paroxysm had passed away in the course of a few hours, it might be followed within a short period by a feeling of general well-being.

(2) They were familiar with two great contrasts, which were each a source of danger. On the one hand there were those who thought too lightly of the malarial attack and failed to take precautions through foolhardiness, while others might fall into a state of panic, with little justification, which in itself would lower the power of resistance.

(3) In some cases there might be violent delirium, suggestive of acute intoxication, and mental symptoms might develop giving ground for a suspicion of alcoholism. He mentioned a case in which he had been called into consultation in a base depot in France, where he was serving as an eye specialist. A colleague of his, who was in charge of the depot, in view of his (the speaker's) malarial experience, asked him to see an officer who had been arrested by the military police. He satisfied himself that the amount of alcohol taken had been quite insufficient to produce by itself intoxication and attributed the symptoms to malaria and the disturbed mentality arising from it. The members of the court martial concurred in this view and the officer was acquitted.

(4) He (Dr. Harford) had read a paper before the Royal Society of Tropical Medicine and Hygiene (published in the *Transactions* of that Society, for December, 1922) on "The Psychical Aspect of the Sequelæ of Malaria and Dysentery and their Treatment by Auto-Suggestion." This was based upon his experience as a specialist in the Tropical Diseases Clinic of the Ministry of Pensions. Finding his patients suffering from abortive malarial attacks, in which no parasites were detected, he regarded these as of psychical origin. He himself had suffered from migrainous attacks since blackwater fever thirty years previously, which had altogether disappeared as the result of treatment on the lines of auto-suggestion, and he had similar experience with many of his patients.

(5) He suggested that malaria produced a form of dissociation almost comparable to that due to hypnotism and that this might have favoured the disappearance of mental symptoms in general paralysis of the insane. Perhaps the malarial attack might be regarded as producing a mental trauma similar to the beneficial results of shock in certain cases of psycho-neurosis.

He considered that the inoculation of patients with so serious a disease as malaria involved considerable risk, but felt that the experience gained from the remarkable paper which had been read that evening, might suggest other methods by which similar beneficial results might be obtained.

Dr. TEMPLETON (in reply) said that he agreed as to the immense value of the close connexion existing on the Continent between the neurological and psychiatric sections, which in Vienna, at any rate, were housed in the same building.

He said that the question of leucocytosis during the course of the malarial paroxysms had been closely investigated in five or six of the cases described. Blood counts were made daily, often several times daily, during the course of the fever and at different

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stages, and at no period did the number of leucocytes rise above 5,000. He considered that this fact ought to be emphasized in view of the supposed importance of a neutrophil leucocytosis in sodium nucleinate therapy.

The intravenous route had been tried in Vienna, but discarded as offering no particular advantage over the subcutaneous method. In the present series, it was considered that the shorter incubation period—1-4 days, as opposed to 1-4 weeks—was of importance in the institutions where accommodation was overtaxed.

He believed that the feeling of wellbeing described by Dr. Harford, as engendered by natural malaria, in normal people, might to some extent explain the transitory effects of the infection in the series of dementia præcox patients already referred to.

Reviewing the results obtained, Dr. Templeton emphasized the fact that eleven of the cases reported upon were very early cases, the duration being on an average of ten months only. This, he said, compared favourably with the duration of the so-called early cases treated in the continental clinics.

## Section of Psychiatry.

President—Dr. THEO. B. HYSLOP, F.R.S.E.

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### The Physical Basis of Emotional Disorder.

By H. CRICHTON MILLER, M.D.

(Hon. Director, Tavistock Clinic for Functional Nerve Cases.)

IT has been claimed in the name of psycho-analysis that "every neurosis is caused by a psychic conflict." Without desiring to generalize on the pathogenesis of the neuroses, I propose in this paper to show how inadequate is such a conception. I submit that a theory of the neuroses based upon "the wish," or on the conflict between ego and social impulses, is narrow to the point of being misleading, however valid it may be in a partial application. Further, I suggest that, apart from the Freudian theory, in actual practice psychoanalysts tend to seek in emotional experience the causation of all but a limited group of the neuroses. It appears to me that, however important the psychogenic factor may be, we must recognize that somatogenic factors are rarely absent, and that to approach the problem of the neuroses exclusively from the standpoint of psychic conflict is to compromise our therapeutic opportunity unjustifiably. The neuropath brings life to a standstill, and the psychopath retreats from it, in each case because of his inability to orientate himself to the future. Is it not reasonable to suppose that much of this inability is based upon an awareness of organic inadequacy? Or, if we are determined to reduce our equation to terms of psychic conflict, let us frankly realize that one or other side of the conflict is frequently conditioned by factors of a biological nature, which are not beyond the reach of physical treatment.

I would ask you therefore to consider the value of the following formula as covering the ætiology of a large group of neuroses: "*Awareness of incapacity to meet a demand for adaptation present or impending.*" It will be convenient to discuss the subject under four headings: (1) alimentary; (2) vascular; (3) endocrine; (4) sexual. Note that these headings are not offered as a classification, as I am aware that they overlap at many points.

(1) *Alimentary*.—It is clearly within the experience of each one of us that our emotional feeling tone is influenced by the minor physiological disequilibria of an empty stomach, a loaded colon or a congested liver, and practical physicians are prepared to discount the emotional reactions associated with such states. But when a condition of this kind becomes chronic we may reasonably suppose that an equally chronic emotional distortion occurs which should preclude any thought of psychological treatment.

Only a few months ago I saw a patient (W. L. L.) who had undergone several months' treatment by an analyst for what he diagnosed as a mild

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anxiety neurosis. Being none the better, she consulted a surgeon who removed a portion of her colon. She now claims to be well. I have no wish to express an opinion on the expediency of either form of treatment, still less do I wish to identify myself with the view either of the analyst or of the surgeon—both of which are known to me—but I am prepared to say that, if both lines of treatment were indicated, the order in which they were carried out cannot be justified. I would go the length of surmising that the analyst must have spent a good deal of his time in attempting to resolve complexes which would have resolved themselves, had the patient's feeling tone been more normal than is that of a typical case of chronic coprostasis. In other words, I believe that the psychic conflict which caused the anxiety neurosis was based on a general awareness of incapacity to meet the demands for adaptation which her daily life imposed on the patient. If I am correct, then the proper procedure would surely have been to eliminate first the factor of intestinal toxæmia which produced this sense of physical inadequacy.

(2) *Vascular*.—It is probable that few of us are sufficiently alive to the influence of blood-pressure on emotions. The sensitiveness of the brain to high and low blood-pressure, and to instability in blood-pressure, whatever be its exact nature, is one of the factors most frequently responsible for emotional disorder. The patient with a consistent hypopiesis must inevitably experience a sense of mental, if not physical, inadequacy, and it may well be a matter of surprise to us when such a patient fails to show clear symptoms of emotional instability. There is a constant sense of inadequacy which may produce an anxiety state or else a constant craving for stimulating experience. Thus we have a considerable class of alcoholics whose craving for alcohol is fundamentally based on this sense of inadequacy due to a low cerebral blood-pressure. Doubtless they all have their conflicts which are bound to be exaggerated by the physical condition, but to say, as the psycho-analysts have said, that "all alcoholism is due to homo-sexuality," is gross misrepresentation, which can only lead to therapeutic puerilities. On the other hand, there is a school of thought which deals largely in exhaustion neuroses and psychoses. The term seems to me a very unfortunate one unless it connotes curability by rest alone. Many of these cases are blood-pressure problems, and mere rest is neither a certain nor permanent cure for disorders of cerebral vascularity.

Again, the mere fact of vascular instability has its emotional implications. The man of 60 whose arteries are beginning to harden, finds that at certain times of the day his concentration fails him or uncontrollable somnolence comes over him. Surely we are justified in regarding this as a cause for deep concern on the patient's part—the sense of his failing capacity to meet the demands for mental activity which his business involves?

(3) *Endocrine*.—It is impossible for us in the present state of our knowledge to have any exact conception of the influence of the internal secretions on the emotional life. But in regard to the thyroid at any rate we have some definite knowledge.

C. G. D., a woman, aged 32, consulted me for breathlessness, lassitude and generalized fear. She had a pulse rate of 120, slight exophthalmos and a greatly enlarged thyroid. I sent her to a surgeon, who expressed unwillingness to attempt surgical interference. The patient was therefore ordered rest, and a colleague of mine undertook her analytical treatment. A mass of repressions was revealed. She had a profound father-complex and a recent history of active homo-sexuality. She made some progress, but when she began to go about again her symptoms returned. The

surgeon then consented to operate and extirpated a considerable portion of the gland. She made a good recovery, went to work, and has been fit for her work ever since, so far as I am aware.

Now in this case I am open to conviction that the psychic conflict was the sole cause of the hyperthyroidism, though I suspect that an element of bacterial infection must at one time have been operative. Let us, however, grant that the condition was primarily psychogenic. It remains undeniable that the physiological implications of the emotional conflict had reached a point at which no redress of the emotional equilibrium could effect a remedy. In short, emotion had affected function and function had affected structure to a point from which there was no recovery by the path of the original onset. Or let us look at this case another way. Granted that the original psychic conflict centred round the parental and homo-sexual complexes—in other words that the original emotional disorder was due to emotional experience—I submit that the conflict was greatly exaggerated by the sense of physical incapacity, an incapacity which was successfully dealt with by physical means, and I add that until this secondary source of conflict was dealt with it was impossible for the patient to resolve that part of her conflict which centred round her sex life. Furthermore, the most striking result of the surgeon's work was the lessened capacity for fear, presumably due to increased stability of all her motor reactions. This case seems to me an illuminating example of the action and reaction between thyroid function and emotional disturbance.

Another case of some interest is the following :—

D. C.-H., aged 10½ years, brought to me for uncontrollable weeping. He was at a first-rate preparatory school where his environment was excellent. Both at home and at school he wept incontinently and without any apparent reason. His intelligence was said to be fully average. He was good at games and notably plucky. But all his masters reported the impossibility of putting up with a boy thus afflicted. During his interview with me he wept copiously and almost continuously. He said he cried because he thought of his father and mother. Now his father had been blown up at sea when the boy was about five. He was said to have seen little of his father and never to have been at ease with him. In due course the mother married again. The stepfather was particularly kind to the boy and the boy seemed fond of him. A year previously both mother and step-father were killed in a railway accident. The boy lived with his brother and sister in the care of an aunt who appeared to be a first-rate foster-mother.

The emotional situation was very intriguing, being complete down to the 'Hamlet touch,' and I venture to surmise that many analysts would have felt justified in treating this patient. On the physical side he was of normal physique, his pulse-rate was normal, but there was a definite thinning of the eye-brows on the external side. I put him on thyroid. In a week he was better; in six weeks he was perfectly normal, and I was afterwards able to confirm from my own observations the claim of his head-master and of his aunt that he had been completely transformed.

But without choosing definitely pathological examples we may recognize the constant influence of the thyroid in the balance of ideation and expression. The thyroid has been recognized as the great anabolic reservoir of the whole system; it has been described by Leonard Williams as the female gland *par excellence*; it has been regarded, probably erroneously, as the determinant of bacterial immunity; Langdon Brown has suggested that it corresponds to the extravert principle in psychology. This last view falls in with my own conception of it as the gland of creation. We can at any rate, without indulging in speculation, recognize in the activity of the thyroid the principal



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determinant of expression. Now the sub-thyroidic introvert is an under-expressed individual who cannot fail to have numerous repressions and who easily falls a prey to an anxiety neurosis. I recognize in him primarily a case for thyroid administration, being convinced that a great deal of his anxiety will clear itself up without any analytical treatment when the balance between ideation and expression has been redressed. His psychic conflict consists largely in that sense of inferiority which arises from his inadequate powers of self-expression. In other words the awareness of incapacity to meet life's demand for expression is the essential feature of his conflict and it is a feature eminently amenable to the simplest form of therapeutic aid.

But we cannot consider thyroid action exhaustively without discussing the adrenals as well. No correlation between emotion and physiological function is better established than the effect of fear on the "fight or flight" mechanism—that is, fear stimulates adrenal activity, this stimulates the sympathetic, the vagus-equilibrium is upset and the thyroid stimulated to increased secretion. Many of us, no doubt, recognized during the war the remarkable difference between what may be described as objective and subjective courage. One man would display the utmost heroism in the face of imminent danger who nevertheless was tortured before by the anticipation or afterwards by the memory of the same danger. Another would appear unaffected before and after, and yet display terror in the presence of the actual danger. It seems to me that these differences must depend primarily on the responsiveness of the adrenal system. With some, fears in phantasy are sufficient to determine adrenal reaction, while with others nothing short of objective danger produces this result. With some the response is biologically efficient and with others it fails to produce an adaptation which is biologically favourable. Hence, in the great class of psychic conflicts which centre round the instinct of self-preservation we may reasonably assume that an important element often consists in the awareness of adrenal inefficiency leading to the incapacity to adapt to situations of danger. I am not prepared to maintain that this factor, although it is largely a physiological one, is capable of being dealt with on a physical basis, but I am optimistic enough to hope that with greater knowledge of the subject this will come to be the case. I am encouraged in this hope by the undeniable effects of adrenal therapy in asthma. Here we have a condition in which analytical treatment has repeatedly revealed the psychogenic importance of fear complexes. It seems to me quite impossible to harmonize these two therapeutic methods of known value unless we picture the condition as a vicious circle in which fear promotes hyperadrenia and hyperadrenia promotes dyspnoea and dyspnoea promotes more fear and so on.<sup>1</sup> The asthmatic is therefore an individual aware of his incapacity to meet certain demands for adaptation of a respiratory nature, and this awareness constitutes, in my view, a part of his psychic conflict which already we can, to some extent, treat on a physiological basis.

But this problem of sympathetic stimulation is important enough to demand further discussion. The balance between sympathetic and parasympathetic activity must unquestionably be of no less consequence to the feeling-tone of the individual than cerebral vascularity. The sympathicotonic is neither more nor less incapable of meeting life's demands than the vagotonic. The first with his uncomfortable tension, his exaggerated response to stimulus, and his variable moods is as far from the mid-line of well-being as the second with his dull, relaxed, and apathetic lethargy that only intense stimuli can

<sup>1</sup> The factor of specific sensitivity to foreign proteins is omitted, not because I ignore it but because it seems irrelevant to the present discussion.

affect. The sympathicotonic fears life because he cannot tolerate it when it becomes unduly stimulatory, whereas the vagotonic fears it because he can only tolerate it under these conditions. I submit, then, that both types are alike aware of their incapacity to adapt to a world in which stimulus is necessarily inconstant.

Now, if we pass to a more general survey of the endocrine system, we are at once confronted by the antagonism between the glands of childhood and those of maturity. Our ultimate fate in life is so intimately bound up in this conflict that we are justified in surmising that it has far-reaching emotional implications. When and how the thymus and pineal yield to the gonads are the questions upon which turn not only our physical stature but also our mental and, in some measure, our moral stature. Analytical psychology is largely taken up with the persistence of puerile characteristics. It is surely impossible for us to contemplate the tendency involved in such regression apart from the physical side. The high-grade defective is no doubt immune from the attentions of the intelligent analyst, but there are definitely subnormal types, many of them characteristically thymocentric, whose thumb-sucking or other childish proclivities are apt to become the subject of solemn and arduous exploration. Without throwing any aspersions on the familiar psychological explanation of these manifestations, I venture to suggest that emotional immaturity might be approached—in certain cases at any rate—from the point of view of physical immaturity. We are all familiar with the inferiority sense of the high-grade defective, associated as it invariably is with a train of compensations in phantasy and behaviour. We recognize that in his case psychic conflict centres round his awareness of incapacity to meet the ordinary demands of life. We are prepared to admit that if any physical treatment could render him more efficient, his inferiority sense would be mitigated and its psychological implications reduced. I venture to suggest that there are many neurotic individuals who, while they cannot be classed as defectives, are yet immature psychologically and physically, and that their treatment should be regarded primarily as a problem of organotherapy, although a problem still unsolved.

(4) *Sexual*.—The question of maturity brings us naturally to the consideration of the sex aspect of our subject. First let me say that if we are to see this problem in its true light, we must recognize that the biological objective of every individual must, from an evolutionary standpoint, be parenthood. I think we are often apt to lose our way in this wood because of the number of trees—chiefly phallic, of course—which constitute it. The gratification of the sex-appetite is constantly referred to in terms which suggest that it is a *terminus ad quem*. This is surely superficial. The individual may, and constantly does, detach the hedonic from the procreative aspect of the function, but biologically the two are inseparable, and it is reasonable to suppose that unconsciously they are much more united than they are in consciousness. I think there is analytical evidence of this. If, then, we regard the achievement of parenthood as an impending demand for the male and the attainment of parenthood as an impending demand for the female, we see how readily psychic conflict may arise from any awareness of incapacity to meet this demand. Let us consider a few sex problems in this light. Let us first take masturbation. It is a mistake, I think, to regard it as mere gratification of an animal appetite. The capacity for sexual response, even of an auto-erotic character, is to some adolescents proof that they have reached a phase of development which is at least on the path to maturity and, as such, the practice has a reassuring value. Now, an adolescent, with normal self-confidence, neither

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requires nor obtains any such reassurance. If, then, some adolescents differ from others in respect of this self-confidence, I submit that one ground of difference is in gonad adequacy. It is true, of course, that much depends on the conscious recognition of the nature of the procreative demand and that when this recognition is premature the fear of procreative inefficiency is likely to follow, apart altogether from a sense of physiological inadequacy. But in certain cases the conflict that centres round masturbation derives its intensity from this awareness of incapacity to meet the impending demand to achieve or attain parenthood. Biologically, the aim of every boy is to make his father a grandfather. Thus, and only thus, can he satisfy himself of his biological maturity. Social and business promotions have little influence; the youth may become the active partner in the business and replace his father there; he may attain to the social status which has ceased to interest his father, but from the broadest point of view he has not satisfied his urge to displace him until he has also begotten a son as his father begat him. Now, regarded in this light the problem of dementia præcox becomes susceptible of a reasoned solution.

Let me take an example recently seen.

C. R. is a law student, aged 22. Father, an active and intelligent man who had forced the boy to ride when he was a small boy in spite of constant terror: subsequently he had found fault with his inefficiency at school and at the age of 14 had threatened to horse-whip him. The patient has been a masturbator since puberty, the practice being associated with masochistic phantasies. He now complains that he cannot concentrate on his studies. His mind is perpetually obsessed by insoluble problems and impossible phantasies of a masochistic nature. This hebephrenic tendency is passing into his general life, and he is continually assailed by such characteristic questions as "What would have happened if I had shut that door instead of leaving it open?"

On the physical side we find that the patient needs to shave only once in two days: puberty came on at 16; at 12 he had mumps. This latter statement may sound peculiarly irrelevant but I must be allowed to reiterate my conviction that mumps occurring between the age of 10 and puberty, even without orchitis, has some deep significance in relation to gonad inefficiency.

Now we see here a picture of a boy submitted from childhood to paternal repression and made to feel that maturity was an unattainable prospect and yet with every reason to wish to displace his father. But he feels physiologically inadequate; he regresses in phantasy more and more while deriving satisfaction, as a result of masturbation, from the assurance that he is at any rate better than a child.

If a motor-car fails to climb a hill it is always possible to say that the hill was too steep. But it is also possible to say that the engine was not sufficiently powerful. Unless we consider the problem in the light of the ratio of the power to the gradient, we get no nearer to a solution. That is why it seems to me that Mott's conception of dementia præcox as attributable to gonad inefficiency is just as unsatisfactory as the analytical theories which stress exclusively the emotional barrier causing the regression. In other words I believe that the patient just described might have made a normal adjustment to life in either of two cases: (a) if his view of adult life in general and of paternity in particular had been less menacing; (b) if his gonad efficiency had given him that confidence which would have enabled him to believe in his ultimate capacity to make his father a grandfather.

Now if this view be correct the therapeutic inference is obvious, namely, that it is our duty to attack the early case of dementia præcox not only from the

analytical point of view, but also from the physical, and it is fortunate that something can be done in such cases by organotherapy.

There is one sexual irregularity to which the psycho-analysts have attached ample importance. I refer to coitus interruptus. While I do not claim that the consequent neurasthenia can be classified under the general heading of awareness of incapacity to meet life's demands, I should like to say two things. The first is that out of a considerable number of cases with this causation I can only recall one in which there was not a perfectly definite focal infection. Hence I profoundly distrust the treatment of this condition that ignores the toxic side. The second thing I have to say is that, primarily, coitus interruptus disintegrates the endocrine system rather than the emotional life. The act consists in a volitional interruption of the most powerful chain of reflexes in our nervous system, and its effect is mainly to throw a strain on the adrenals which in course of time results in adrenal exhaustion. This is my second reason for distrusting the psychotherapeutic approach to such cases, at any rate as a first approach.

The menopause presents problems in every department of medicine, with the possible exception of orthopaedics. The menopause has necessarily the most profound significance from an emotional point of view for the woman whose maternal aspirations are unsatisfied. But in helping the patient to adjust to the cessation of her generative possibilities, we must not ignore the endocrine side, for the withdrawal of the ovarian hormone has a repercussion on both thyroid and adrenals, which is of the greatest importance. A temporary vagotonia ensues which if it replaces a previous sympathicotonia determines grave physiological changes. To ascribe the neuroses of the menopause to psychic conflict is to express a partial truth.

Finally, I would refer very briefly to the bewildering problem of cyclothymia and its less frequent but more obvious manifestation of manic-depression. I fancy there is no analyst of any experience who has not been fooled by a cyclothymic. Without recognizing the true character of the condition we begin an analysis of a patient who appears to be suffering from an anxiety neurosis. Sooner or later the patient begins to improve, and if the analyst is human he begins to congratulate himself until he recognizes the true situation either by the patient's undue exaltation or by an inexplicable relapse into depression. If we watch such a case over a period of several years, we recognize that the emotional reactions belonging to one phase have no relation to those manifested in the opposite phase: we note generally a fairly exact periodicity, and above all the fact that the transition from one phase to another is totally independent of emotional factors—favourable or adverse. Patients of this type are prone to be "cured" by whatever treatment happens to be in use at the time that the change comes. I cannot say whether the milder manifestations of cyclothymia are classed as neuroses by those who claim that every neurosis is caused by a psychic conflict, but I am very certain that the key to this problem will never be found in the realm of pure psychotherapy.

In conclusion, let me say that I have spoken, it may seem, in a derogatory way of analytical treatment. If I have done so, it is not because I am doubtful of its value or unconvinced by its successes. Rather do I feel that the therapeutic contribution of analytical psychology is too important and its possibilities too valuable to be exposed to criticism and even ridicule because of the apparent incapacity of certain analysts to correlate their psychological theory with physiological observation.

## 34 Crichton Miller : *Physical Basis of Emotional Disorder*.

### DISCUSSION.

Dr. C. H. CALDICOTT asked if Dr. Crichton Miller could give him any guidance in the treatment of mental states by organotherapy. With the exception of thyroid in hypothyroid conditions, substitution therapy did not seem to be very useful. He himself had charge of a good many depressive cases, and he felt that further knowledge of the internal chemistry of the body would go far to solve the problem of treatment in these cases. He had tried many glandular preparations of various kinds, but they did not appear to have much value. Did Dr. Crichton Miller know of any method of influencing the amount of any of the internal secretions by the use of drugs?

Dr. A. C. COURT said that he had been interested in Dr. Crichton Miller's attempt to associate endocrine deficiency in certain forms of mental illness. Certain puerile mental and physical characteristics did persist in cases, but he was doubtful of their ability to be altered by endocrine therapy. Dr. Swale Vincent, whose opening address at the discussion on the "Present Position of Organotherapy"<sup>1</sup> he had heard, had perhaps criticized that method of treatment rather severely, but he (Dr. Court) had also found endocrine therapy disappointing, and was inclined to the method of altering the patient's endocrine balance by another method, i.e., investigation of the life history of the individual to discover the conditioned reflexes which had been built up in association with his endocrine reaction; then by re-education, with added insight, the abnormality might be adjusted. He had seen such occur, but it was not to be expected that very good results would be obtained after violent and long continued endocrine disturbance. He had endeavoured to classify his patients on physical characteristics as endocrine types; he had read Berman's book, but he could not see types in the way Berman did; that writer appeared to have over-simplified the matter. The endocrine make-up of the patient seemed more complex, and for that reason he (Dr. Court) could not be quite so optimistic about the future of endocrine therapy as Dr. Crichton Miller seemed to be.

Dr. PHILIP CLOAKE said he understood that Dr. Crichton Miller's classification was not intended to be exhaustive. Dr. Crichton Miller's four classes of alimentary, vascular, endocrine, and sexual influences, might be said to be comprehended under the general heading of chemical, especially internal secretory, disturbances. But it had to be remembered that there was a large and important class omitted, a class in which the profoundest emotional disturbances might occur, and in which endocrine disorder had not been accorded any ætiological significance. He referred to that class in which emotional changes were associated with organic disease of the central nervous system. Such changes might be prominent in cases of disseminated sclerosis, syphilis of the central nervous system, chorea, encephalitis lethargica and many other diseases. It was probable that in many of these cases, if not in all, the emotional changes might be correlated with disturbance of the function of the basal ganglia and their cortical connexions. Here one had very striking instances of changes in conduct and feeling determined by lesions in the neurological basis of emotional expression.

Dr. T. M. LEGGE cited cases of manganese poisoning as an instance of physical conditions causing emotional disorder. The symptoms were the following: Stolid mask-like features, monotonous, whispering voice, ankle and patella clonus, spastic gait, and, occasionally, uncontrollable laughing or crying. In severe cases recovery was never complete. In only one case had a post-mortem examination been conducted, and in that case there was found some degeneration of more or less regular character in the longitudinal fibres of the pons, which ran with those of the pyramidal tracts.

Dr. HUBERT J. NORMAN said that the abstract conception of emotional changes—as of psychological processes in general—was unsatisfactory. It was satisfactory to see that Dr. Crichton Miller had laid stress on the importance of considering the physiological basis. Maudsley spoke of emotion as a "commotion" in the nervous system. Dr.

<sup>1</sup> *Proceedings*, 1928, xvi (Sect. Therap.), p. 7.

S. A. Kinnier Wilson, just a year ago, had discussed before the Section the emotional changes associated with gross lesions of the brain. It was interesting to note in this connexion the flooding of emotion that occurred in mental disorder dependent on arteriopathic changes and also in general paralysis of the insane. The problem of dementia præcox should be considered from both points of view—gonad deficiency and psychological stress. In some cases, especially where there was deficiency in the white cells of the blood, definite improvement had followed the administration of nuclein. Manic depressive insanity appeared to be associated with, possibly, cyclic changes in glandular secretion rather than with any observable mental stress. Berman's book on "The Glands Regulating Personality" was a fascinating work but the author seemed to let theory run much beyond ascertained fact: and in some ways it might be regarded almost as a contribution to the new American poetry!

Dr. NOEL BURKE said that physicians and surgeons tended to regard patients as merely bodies while psychotherapists tended to regard them as minds. The whole point of Dr. Crichton Miller's paper was that a patient should be considered as a mind and a body acting and reacting the one on the other. The paper was a plea for thorough examination, and for treatment of any organ or function that was defective. He believed that it was possible for a psychic trauma to so disturb function that permanent structural change could result, and he welcomed the support that he believed Dr. Miller had given to this view.

Dr. CRICHTON MILLER (in reply) said that he had studied manic-depressive cases with great interest but with a sense of helplessness. At the same time he believed that such cases would be susceptible of cure, at least in part, by organotherapeutic means. The only really bad manic-depressive for whose recovery he could vouch had been cured by marriage. He had a feeling that two groups of cyclothymics would one day be recognized—the first depending on thyro-adrenal instability; the second on thyro-pituitary instability. He admitted that this conception was at present purely speculative, and that his own attempts to cure this condition by organotherapy had been entirely unsuccessful.

With regard to Dr. Court's remarks about the classification of endocrine types, he felt there was room for more optimism than Dr. Court admitted. Despite the difficulties we could at least recognize with something approaching clinical certainty conditions such as myxœdema, acromegaly, and Addison's disease. If we had to-day succeeded in recognizing even a few extreme monoglandular derangements, it was reasonable to expect that in course of time we should be in a position to identify many more. He agreed, however, with Dr. Norman, that Berman's work was too speculative to be informative, though it was certainly suggestive.

One speaker had referred to the individual's personal history as determining his endocrine balance. Clearly the personal history could—and constantly did—alter the congenital endocrine pattern, but it was impossible to ignore that pattern as a very specific portion of the hereditary endowment of the individual. It was known, for instance, that stature was largely if not exclusively determined by endocrine factors, and, therefore, the transmission of stature characteristics implied the transmission of a certain endocrine pattern.

He was glad that Dr. Freeman had supported the view that bacterial immunity made for improvement of feeling-tone. He believed that general medical opinion supported the converse truth: that a positive feeling-tone contributed to improved immunity. If we sought a theoretical explanation of these views it probably lay in the fact that emotional well-being presumed a healthy and active thyroid and in the health and activity of the thyroid was involved the normal functioning of the parathyroids.

With regard to gonad inefficiency, he could only state that in his experience the worst masturbators were under-sexed and not over-sexed, although the opposite condition was often presumed.



## Section of Psychiatry.

President—Dr. T. B. HYSLOP, F.R.S.E.

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### The Relation of Alcoholism to Insanity and to Crime.

By W. C. SULLIVAN, M.D.

ALCOHOLISM is so hackneyed a question that I feel it incumbent on me to offer some apology for bringing it forward as a subject of discussion at this Section. There is, however, this excuse for my choice, that, like many other old problems, it has received a certain amount of fresh illumination from the experience of the recent war years, which may contribute to a clearer view of some of its aspects than has hitherto been obtainable.

I propose to discuss in this paper the relation of *alcoholism* to insanity and to crime—of alcoholism, not of alcohol. That is to say, my concern is solely with the relation to the two phenomena specified in my title of alcoholic excess, of the abuse of alcohol and not of its use. Whether the temperate consumption of alcoholic beverages has any influence on insanity or on crime, and if it has, of what nature that influence may be, whether its effects are good or evil—these are questions which lie outside the scope of the discussion. And as they are questions with regard to which we have no reliable data, any beliefs which we may hold about them belong rather to the realm of faith than to that of reason.

Before considering the proper matter of this paper, I will briefly recall to your memory some of the main facts regarding the action of alcohol on the body, and the bearing of these facts on the causation of alcoholism. The most important and elementary point to be noted in this connexion is that alcohol, in whatever form it is taken, is absorbed into the circulation very rapidly, but is consumed in the system very slowly. Its rapid absorption accounts for the readiness with which acute intoxication or drunkenness is produced by the taking of a large quantity of alcohol at once in a single dose or in a quick repetition of smaller doses. And the slow rate at which the drug is disposed of in the body explains why its more or less continuous consumption in relatively moderate doses, though it may never produce an appreciable degree of drunkenness, may nevertheless maintain the alcohol content of the blood at such a level as to exercise a constant toxic action on the tissues, which is apt in the long run to bring about the morbid changes comprised under the heading of chronic alcoholism.

Now these two modes of action of alcohol are not merely separable in theory, they are more or less distinct and contrasted in fact. Alcohol is taken primarily for the sake of its effect on the emotional tone; it is a mild narcotic which, by weakening inhibition and blunting sensibility to unpleasant stimuli—whether originating within the organism or occurring in the environment—



produces a more or less exhilarating effect on the drinker. The conditions under which this effect is ordinarily sought may be roughly divided into two main classes, corresponding to two main types of drinking, which, from the predominant element in each, may be conveniently distinguished as convivial drinking and industrial drinking. Convivial drinking, the character of which is sufficiently indicated by the qualifying term, is for the most part intermittent, and so, though it may run on to drunkenness, it does not tend to cause chronic alcoholism. In pure industrial drinking, on the other hand, where alcohol is taken in small but frequently repeated doses, as an aid to work, and especially to work of a fatiguing and unpleasant kind, there is no tendency to acute intoxication, but the constant presence in the blood of a noxious quantity of alcohol tends to bring about the tissue degenerations of chronic alcoholism. There is abundant clinical and statistical evidence to prove that, in the main, chronic alcoholism—and it is chronic alcoholism that we have chiefly to consider in the relation of intemperance to insanity and to crime—is due to industrial drinking. I cannot now refer to this evidence in detail, but it will be enough to point to one well-established and unequivocal fact, namely, the wide divergence in respect to alcoholic incidence between the several industrial groups, and the close relation which obtains between the degree of alcoholic proclivity and the conditions of work in each of these groups. This is immediately evident from a glance at the accompanying table (Table I) showing for certain

TABLE I.—COMPARATIVE MORTALITY FIGURES FOR (A) ALCOHOLISM AND (B) ALCOHOLISM AND LIVER DISEASES TAKEN TOGETHER IN CERTAIN OCCUPATIONAL GROUPS. (CENSUS OF 1891 AND OF 1911.)

| GROUP                     | 1891.      |                               |     | 1911.      |                               |     |
|---------------------------|------------|-------------------------------|-----|------------|-------------------------------|-----|
|                           | Alcoholism | Alcoholism and liver diseases |     | Alcoholism | Alcoholism and liver diseases |     |
| Agricultural ... ..       | 4          | 21                            | ... | 3          | 11                            | ... |
| Coal miners... ..         | 4          | 21                            | ... | 3          | 13                            | ... |
| Metal workers ... ..      | 11         | 40                            | ... | 5          | 19                            | ... |
| Dockers ... ..            | 52         | 78                            | ... | 26         | 43                            | ... |
| Publicans, &c. ... ..     | 94         | 268                           | ... | 50         | 152                           | ... |
| Clergymen ... ..          | 2          | 20                            | ... | 2          | 11                            | ... |
| Medical profession ... .. | 14         | 74                            | ... | 9          | 38                            | ... |

occupational groups in this country the comparative mortality figures from alcoholism alone, and from alcoholism and liver diseases taken together, liver diseases being the most usual synonym for alcoholism in the death certificates of paying patients. The two sets of figures given in the table are taken from the Supplementary Reports of the Registrar-General in connexion with the Census returns of 1891 and 1911; but the same characteristics are found in the reports referring to other Census periods—the actual figures differ, of course, between one period and another, but the order of the several industrial groups in the series is practically constant.

Now it is somewhat difficult to see how this regularly recurring relationship can be explained on any other theory than that of the dependence of alcoholic excess on the character and conditions of labour in these several occupations. How can it be interpreted for instance, by those fashionable hypotheses which attribute intemperance to an inborn craving for alcohol or to a repressed homosexual complex? Conceivably, no doubt, it might be suggested that the exceptionally high incidence of alcoholism among publicans and barmen is to be accounted for on the assumption that a special degree of inborn craving constitutes a vocational predisposition to the liquor trade, or that a strong homosexual instinct tends to find an outlet through the phallic symbolism

of the beer-handles. But how can we apply these interesting theories to other industrial groups? Why should the inborn craving or the homosexual instinct be so much more frequent in iron workers than in miners, in doctors than in clergymen? Until these difficulties are answered, we may be content to rest in the simpler conclusion that the main determining factor in industrial alcoholism, and therefore in chronic alcoholism generally, is to be found in the nature and circumstances of occupation.

After these preliminary remarks on the causation of chronic alcoholism, we now pass on to consider the relation of this condition to insanity and to crime.

First, then, as to insanity. Of course in a comprehensive psychological view, drunkenness is an insanity; the drunken man, as Maudsley aptly puts it, "in his drunkenness exhibits the abstract and brief chronicle of insanity." And the enfeeblement of intelligence and the perversions of mood which are ordinary accompaniments of chronic alcoholism are also in this sense insanities. But it is not to these states that people refer when they assert, as many people do assert, that alcohol is a frequent cause, or even the most important cause, of insanity. What is meant by insanity in such propositions is the insanity that brings its subjects into the lunatic asylum, insanity of the type of dementia præcox, manic-depressive psychosis, general paralysis or some other form of certifiable unsoundness of mind. What, then, is the relation of alcoholism to this certifiable insanity? Many years ago, when it was almost an article of faith to attribute a great deal of influence to alcoholic excess as a cause of every form of mental disease, it was pointed out by Sir Frederick Mott that it was difficult to reconcile this orthodox view with the fact that the pathological changes typically associated with chronic alcoholism were very rarely observed in the post-mortem examination of patients dying in asylums. Mott found that cirrhosis of the liver, for example, was present in 7·7 per cent. of autopsies at Charing Cross Hospital, but in only 1·8 per cent. of the autopsies at Claybury Asylum, and, further, that the asylum cases in which this diseased condition was discovered were mostly those of definitely alcoholic insanity, such as delirium tremens and Korsakow's psychosis. Again, when alcoholism and insanity were compared in respect of their regional distribution, it was impossible to trace any correspondence between the two phenomena; in districts where alcoholism was most prevalent the insanity rate was often very low and, conversely, areas of relatively light alcoholic incidence showed in many instances a very high rate of asylum admissions. In view of such facts, it seemed reasonable to suspect that alcoholism could hardly be so significant a cause of certifiable insanity as had been supposed.

Further and even more conclusive evidence in confirmation of this sceptical attitude has been provided by the statistical records of the war period. During that period, it will be remembered, the liquor traffic in this country was regulated by the system of physiological control devised by Lord d'Abernon. An essential principle of that system was the restriction and interruption of the hours of sale of alcoholic liquors, which had the effect not only of diminishing the opportunity of immediate excess, but also, what was still more important, of protecting the ordinary drinker from the danger of a persistent alcoholic action on his tissues. Since these restrictions, which were designed only to prevent excess, allowed ample facilities for reasonable drinking, they did not provide any adequate motive for an illicit trade in liquor, and they did not, therefore, lead to the growth of alcoholism which has been the natural consequence of such futile and mischievous systems as prohibition, local option

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and Sunday closing. The results of Lord d'Abernon's policy in the reduction of drunkenness, and still more in the prevention of industrial drinking and chronic alcoholism, are apparent from the accompanying table (Table II), showing

TABLE II.—ALCOHOLISM AND INSANITY (WOMEN ONLY), ENGLAND AND WALES, 1913-1921.

| Year | Convictions for drunkenness | Deaths from alcoholism | Deaths from cirrhosis of liver | Attempted suicide | First admissions to lunatic asylums |
|------|-----------------------------|------------------------|--------------------------------|-------------------|-------------------------------------|
| 1913 | 35,765                      | 719                    | 1,665                          | 988               | 9,372                               |
| 1914 | 37,311                      | 680                    | 1,773                          | 1,049             | 9,702                               |
| 1915 | 33,211                      | 584                    | 1,525                          | 816               | 9,078                               |
| 1916 | 21,245                      | 333                    | 1,163                          | 436               | 8,850                               |
| 1917 | 12,307                      | 222                    | 808                            | 383               | 8,702                               |
| 1918 | 7,222                       | 74                     | 579                            | 319               | 9,726                               |
| 1919 | 11,183                      | 84                     | 510                            | 499               | 9,888                               |
| 1920 | 15,246                      | 140                    | 562                            | 619               | 9,769                               |
| 1921 | 12,892                      | 139                    | 639                            | 291               | 9,823                               |

### CASES OF DELIRIUM TREMENS IN WOMEN.

(Certain Poor Law Infirmaries.)

|         |     |     |         |     |    |
|---------|-----|-----|---------|-----|----|
| 1913-14 | ... | 213 | 1917-18 | ... | 18 |
| 1914-15 | ... | 190 | 1918-19 | ... | 11 |
| 1915-16 | ... | 102 | 1919-20 | ... | 25 |
| 1916-17 | ... | 54  | 1920-21 | ... | 19 |

the movement of drunkenness, of delirium tremens, and of the mortality from alcoholism and from cirrhosis of the liver during the years 1913-21. To avoid the fallacy arising from the disturbing influence of military service on the male population, the figures refer only to women.

It will be seen from this table that the extraordinary decrease during these years in all the more generally recognized manifestations of alcoholic excess, has not been accompanied by any corresponding decrease, or indeed by any decrease at all, in the number of first admissions to asylum care. The specifically alcoholic insanity, delirium tremens—a form of insanity, however, which is only exceptionally treated in the lunatic asylum—declines almost to vanishing point, but the ordinary psychoses are apparently about equally frequent whether alcoholism stands at its high pre-war level or has fallen to 70 per cent. below that level. And in other countries also, in which the abnormal conditions incident to the war brought about a considerable restriction of the supply of alcoholic liquors and a consequent reduction of alcoholism, there was a similar absence of any corresponding decline in the prevalence of insanity.

It appears to be a legitimate inference from these statistical facts that alcoholism does not play any significant part in the causation of the mental disorders which make up the mass of certifiable lunacy, and that its direct influence as a cause of such insanity is confined to the group of psychopathological conditions which bear distinctive clinical marks of alcoholic origin, namely, delirium tremens, Korsakow's disease, alcoholic dementia, and alcoholic delusional insanity. So that, to put it briefly, it may be said that the only insanities due to alcohol are the alcoholic insanities. In other forms of mental disease, alcoholic excess may be, and in modern industrial communities very frequently is, a common antecedent, and may often be a more or less prominent symptom, but it is not an ætiological factor.

So much, then, as to the relation of alcoholism to insanity in the narrower sense of certifiable unsoundness of mind. We now pass on to examine its relation to crime. In this part of our inquiry, we have first of all to get clearly before our minds what it is that we mean by crime, for the risk of fallacy which attends the use of general terms is peculiarly great in connexion with

this very comprehensive word. A crime is an act or omission punishable by the law, so that the forms of antisocial conduct that come within the definition of crimes will obviously show considerable diversity in their nature and origin and in the degree in which they conflict with the moral standards of the community. It will be sufficient for our purpose to confine our attention to the offences which are regarded by the law as being of specially grave character, and which are accordingly liable to be dealt with by the more elaborate legal procedure of indictment. And these indictable offences we may further subdivide roughly, according to what we may presume *prima facie* to be ordinarily the main underlying impulse in each class, into three classes: (1) Crimes of acquisitiveness, (2) crimes of violence, and (3) crimes of lust. The only important indictable offences which do not fall within one or other of these classes are attempts to commit suicide, and malicious damaging of property, two forms of delinquency which have considerable affinity to crimes of violence. Now of these varieties of crime, crimes of acquisitiveness are, from the numerical point of view, overwhelmingly preponderant: they constitute more than 90 per cent. of the total volume of indictable crime. Crimes of violence and of lust, on the other hand, are relatively quite infrequent; but, though numerically insignificant, they are as a rule far graver in their antisocial character than crimes of acquisitiveness. This distinction is important for our present purpose because the influence of alcoholism as a cause of crime is practically negligible in crimes of acquisitiveness, but is very considerable in crimes of violence and of lust; in relation to crime, therefore, alcoholism is significant from a qualitative rather than from a quantitative point of view. If we take criminality in the mass, we find that alcoholism is not a very large factor in its causation, because the bulk of criminality is acquisitive; but this way of looking at the problem would obviously be very misleading, since it obscures the fact that in the other forms of delinquency which are of graver significance but which, being relatively infrequent, have no appreciable effect on the statistical movement of crime in general, alcoholism plays a very large part—at all events it did so before the war. To estimate the degree of its influence we have to rely mainly on the impressions of clinical experience, but, as the conclusions formed in this way are necessarily of qualified value and may be biased by the personal equation of the individual observer, it is a fortunate circumstance that they can also to a certain extent be tested and confirmed by the impartial evidence of statistics. To illustrate this point, we may first revert for a moment to the figures in Table II (p. 40). In addition to the columns referring to insanity and to the various phenomena of patently alcoholic origin, such as arrests for drunkenness, deaths from alcoholism and cases of delirium tremens, this table includes also a column showing cases of attempted suicide. You will observe that the figures in that column exhibit a close correspondence with the figures relating to alcoholism. Now it is a well-established fact of clinical experience—confirmed by statistical proofs—that, in this country, abortive attempts to commit suicide are due in a large majority of instances to alcoholism, and more particularly to chronic alcoholism or to drunkenness supervening on chronic intoxication. And the striking fall in the frequency of such attempts, coincident with the decline of alcoholism since 1914, as shown in this table, fully confirms this view. Attempted suicide is, in fact, perhaps the most frequent and most characteristic of the graver conduct disorders relating to alcoholism. If, then, alcoholism plays, or has played in the past, any large part in the causation of crimes of violence and of lust, as clinical experience would suggest, we should expect to find evidence of the fact in

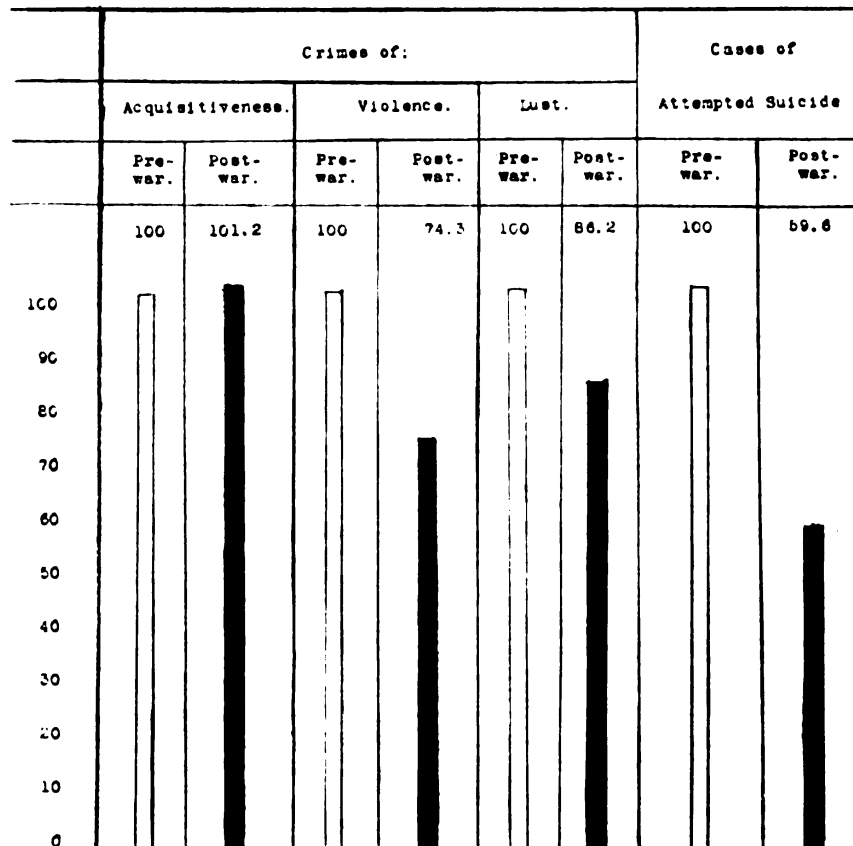
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a similar though, of course, less pronounced decline in these forms of delinquency. To ascertain whether this is so or not, we may compare the prevalence of the several varieties of crime in the years immediately preceding and those immediately following the war. The actual war years we may leave out of account, for crime is mainly a masculine activity, and the abnormal conditions affecting the male population in war time exercise a disturbing influence which would make criminal statistics for these years unreliable in the present connexion. The accompanying table (Table III) and

TABLE III.—CRIMES KNOWN TO THE POLICE.  
Proportion per 100,000 of Population in Years 1912 to 1921.

|                                                 | 1912   | 1913   | 1914   | 1915   | 1916   | 1917   | 1918   | 1919   | 1920   | 1921   |
|-------------------------------------------------|--------|--------|--------|--------|--------|--------|--------|--------|--------|--------|
| Crimes of violence                              | 5.79   | 5.43   | 4.68   | 3.87   | 3.53   | 3.06   | 3.11   | 4.12   | 4.11   | 3.69   |
| Crimes against morals (exclusive of bigamy) ... | 6.42   | 6.92   | 6.27   | 5.17   | 4.77   | 3.57   | 3.65   | 4.94   | 6.04   | 5.     |
| Attempted suicide                               | 6.54   | 6.71   | 6.74   | 4.79   | 3.64   | 2.86   | 2.53   | 3.63   | 4.66   | 3.     |
| Crimes of acquisitiveness ...                   | 256.35 | 242.33 | 220.62 | 202.93 | 219.66 | 251.21 | 249.71 | 226.23 | 249.61 | 255.38 |

TABLE IV.—PREVALENCE OF CRIMES OF ACQUISITIVENESS, CRIMES OF VIOLENCE, CRIMES OF LUST, AND CASES OF ATTEMPTED SUICIDE IN THE POST-WAR PERIOD (1919-21) IN PERCENTAGE RELATION TO THEIR PREVALENCE IN THE PRE-WAR PERIOD (1912-14).



the diagram based on it (Table IV), show the result of this suggested comparison between the pre-war and post-war prevalence of the three main classes of criminality and of suicidal attempts. As you will observe, crimes of acquisitiveness have returned to their pre-war level and have even slightly exceeded it; but crimes of violence and crimes of lust have both remained sensibly below their former rate of prevalence, showing a decline similar, though less marked, to that in attempts to commit suicide. In the absence of any other plausible explanation, it seems reasonable to conjecture that the reduction in homicidal and sexual crime, and in suicidal attempts, is a result of the coincident reduction of alcoholism, and thus confirms the clinical view which attributes to alcoholism a large part in the causation of these disorders of social conduct; while the absence of any corresponding reduction in crimes of acquisitiveness similarly supports the conclusion that alcoholism has no appreciable influence on the production of this form of delinquency.

We now turn from the statistical side of this subject to its clinical aspect, and inquire what are the characteristics and what is the mode of origin of the criminal conduct related to alcoholism. We shall first consider from this point of view the clinical features of "alcoholic murder." In this country, murder, apart from child murder, usually means the murder of women by men, and, in alcoholic cases, it generally means the murder of the wife or the mistress by the husband or the paramour. If we survey the alcoholic murders of this marital group, we find that they may be ranged in an unbroken series, at one end of which are the crimes committed in the dream-state of pathological drunkenness, with subsequent amnesia of the circumstances of the act, and with inability on the part of the murderer to assign any conscious motive for what he has done: while at the other extremity of the series are the instances in which murder is perpetrated with premeditation, is clearly remembered, and is associated in the consciousness of the murderer with definite delusional beliefs, including almost always delusions of marital infidelity. In between these two limits are cases intermediate in character—cases where the amnesia is only partial, so that the act may be remembered but not its motive, or cases again where the delusional rationalizations of the homicidal impulse may be fleetingly present only in phases of drunkenness and are not entertained, or at all events are not expressed in the sober intervals, and so on to cases where delusions of jealousy are more or less stable but vague and undeveloped.

All the cases of this series are alike in having a common toxic origin; they are alike also in the nature and direction of the homicidal impulse which characterizes them. The only differences they present are differences in the degree of associated intellectual disorder, and these differences, in their regular gradation throughout the series, are obviously nothing more than would result from differences in the stage of development of a morbid process. It is, therefore, a natural inference that the underlying mechanism is essentially similar throughout, and that alcoholic homicide, in such cases as we are here considering, must have a common organic basis. What is that basis? As a preface to this inquiry, and also in order to give clearer definition to the point we have to discuss, it will be convenient to take a concrete example of alcoholic homicide, and for this purpose it will be most illuminating to select a case showing the transition from the purely impulsive to the nascent delusional stage. The following observation illustrates very admirably this moment in the evolution of the alcoholic.

The patient was a man of 41 years of age, a fish-hawker by occupation. There was nothing noteworthy in his family history, and his long resistance

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to alcohol is presumptive evidence of a normally stable brain. He had been a steady drinker for some twenty or twenty-five years, and during the last two or three years before the crime which brought his alcoholic career to an end, he had several attacks of delirium tremens; also, without actual delirium, he had occasional hallucinations, tremor and insomnia. When under the influence of drink, he had latterly shown an increasing irritability of temper, especially towards his wife, and had expressed suspicions about her chastity; he would then accuse her of misconducting herself with unidentified men, would watch her movements, threaten and even assault her. When sober, he did not say anything about these ideas. On two occasions during this period he attempted to commit suicide.

For some months before the murder, which occurred early on Christmas Day, he drank very heavily. On Christmas Eve he had a violent quarrel with his wife. After this he stayed up all night, wandering about the house, talking to himself, and occasionally beating his head against the wall. Early next morning the woman went to a neighbour's house to ask the time, and the patient, who had been busy sharpening a knife, followed her, and stabbed her fatally. Immediately afterwards he said, "It is all over last night's affair: I saw it with my own eyes: I did it deliberately over that." Thirty hours later, he became mildly delirious and hallucinated: he talked about his wife's misconduct and about his own bodily condition, saying that his inside had been taken out, that half of his penis had been cut off. Later on, when this state of excitement had passed off, he was able to give a coherent account of the crime: he said that his wife, who had been committing adultery for years, brought a man to the house on Christmas Eve; resenting this brazen conduct, he went to bed, leaving the woman with her paramour: soon after, the door being partly open, he heard obscene talk between them, and, on looking out, saw them having connexion in the presence of the children. He could not remember very clearly what happened after that, but thought that he must have fallen asleep. Next morning, when he saw his wife going quietly about her household affairs and when he thought of the disgusting conduct he had witnessed in the night, his indignation overpowered him, and he killed her.

This case is a compendium of the characteristics of alcoholic homicide in a mature phase of the intoxication, the phase in which the evidences of disorder are extending from the sphere of impulse and feeling to the sphere of thought. The murderer was a confirmed alcoholic, in whom the reaction of the poison on the organism had been already manifested by attacks of delirium tremens and by impulsive suicidal attempts; ideas of jealousy developed in the course of the morbid process in the exacerbations of drunkenness; and in the delirious phase following the murder they emerged clearly in association with ideas of sexual mutilation and of visceral disorder, forming a delusional triad which is almost distinctive of chronic alcoholism. In less developed cases the homicidal impulse, usually with an accompanying suicidal impulse, issues in the murderous act without any explicit delusional symptoms; in more developed cases, the delusions of the wife's unfaithfulness, with associated delusions of poisoning and of interference with the sexual organs, attain a more stable degree of organization, persisting in periods of sobriety and providing the supposed "motive" for the murder.

Delusions of sexual jealousy are, then, the characteristic disorder of thought in cases of alcoholic murder with conscious motive. And such delusions, of course, are notoriously frequent in alcoholics in general; according to Krafft-

Ebing, they are present in 80 per cent. of the subjects of this chronic intoxication. Moreover, in forms of mental disease which do not ordinarily present delusions of marital infidelity in their clinical course, such delusions may appear and may seem to determine the direction of morbid impulse in cases where an alcoholic influence is operative to any considerable extent. This may be observed, for instance, in general paralysis when that disease develops in an alcoholic subject, or when there is a prolonged period of alcoholic excess in the initial stage of the malady; several of the few recorded cases of homicidal crime committed by general paralytics from motives of jealousy show this association. And it is also by no means uncommon in dementia præcox; when the onset of this degenerative process is accompanied, as it sometimes is, by excessive drinking, there may be a temporary development of ideas of sexual infidelity giving a semblance of conscious motive to the homicidal impulse.

To complete this short review of alcoholic crime, I may refer to the other category of grave delinquency in which alcoholism plays an important part, namely, the crimes of lust. In the more serious sexual offences it is necessary to distinguish two classes—sexual assaults committed on adults and sexual assaults on children. Both these varieties of sexual crime are frequently related to alcoholism, but they differ from one another very strikingly in the mode of this relationship, for while rape and indecent assaults on adult women are commonly connected with casual drunkenness, the violation of children, usually the children of the drunkard himself, is in a special manner a crime of the chronic alcoholic. In assaults on adults the sexual impulse, however vicious in its mode of expression, is in itself a normal impulse; it issues in an act which is brutal and criminal, and is, no doubt, to that extent abnormal, but its abnormality is in the inappropriate circumstances, and not in the instinct which inspires it. In the sexual assaults committed on children, on the other hand, the underlying impulse is of a perverted kind; it is a morbid impulse originating in a diseased state of the organism, and that is why crimes of this sort are so frequently met with in conditions of organic decadence, such as senility and chronic alcoholism. It is interesting to note in this connexion that instances are occasionally observed in which fully developed chronic alcoholism with characteristic delusions of marital infidelity and poisoning are associated, not with the murder of the drunkard's wife, but with the violation of his children. For example, one of my patients, formerly a heavy drinker, invalided from the Army with a diagnosis of shell-shock, became insanely jealous in the classic way of the chronic alcoholic; he accused his wife of misconduct under impossible conditions, he complained to the police of the nocturnal visits of her lovers, took her to a doctor to be examined for an imaginary syphilis which he fancied that she had passed on to him, had his food examined by a chemist to detect the drugs which he thought she put in it. The ordinary and pseudo-logical sequence of this state of things would have been the murder of the wife, and the patient did frequently threaten to react in this direction. Eventually, however, instead of doing so, he raped his 12-year-old daughter, and murdered her to prevent her from denouncing him. Obviously, in this case the crime and the accompanying delusions, though logically independent, had a common origin in the disordered state of the organic life.

Now, if we take a comprehensive view of all these clinical facts which we have been considering, and if we bring them into relation with what we know of the pathology of chronic alcoholism, I think we may be justified in drawing some fairly probable conclusions regarding the nature and mode of origin of the disorders of conduct and of thought in this morbid condition. The pathological



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changes produced by chronic poisoning with alcohol are generalized in their distribution ; they may differ in different cases in the degree to which particular organic systems are affected, but all systems are liable to be involved in some measure. There are, however, certain seats of election, certain systems where the morbid effects of the poison are commonly most marked, and of those, the most important, the earliest and most constantly affected, are the nervous and digestive systems, and the genital glands. The action of alcohol on the nervous and digestive systems is, of course, familiar and notorious ; and with regard to the sexual glands, the recent work of Bertholet and of Weichselbaum has shown that the alterations which these glands exhibit in subjects of chronic alcoholism, though not revealed by such obvious evidences of functional disorder are even more pronounced and more significant. We have, then, in chronic alcoholism, on the one hand, a diseased condition of the nervous system, and particularly of the higher levels of that system which subserve mental functions ; and, on the other hand, a generalized visceral disorder, affecting in a special degree the alimentary organs and the genital glands. The nature and distribution of these morbid changes thus present a remarkable and suggestive correspondence with the character of the associated disorders of feeling, impulse and thought. This correspondence would seem to be most readily explained on the hypothesis that the mental disorders of chronic alcoholism are the reflection in the weakened brain of the morbid bodily condition ; that they are the " psychic signature " of the diseased organic life. That is why the typical clinical picture is what we find—a pessimistic and malignant emotional tone with suicidal and homicidal impulses, and with a specific colouring of morbid impulse and morbid thought in harmony with the special incidence of the poison on the digestive organs and the genital glands. So that we have, as characteristic manifestations of this state, in conduct, suicide, wife murder and perverted sexual offences against children ; in thought, the delusions of poisoning, of marital infidelity and of sexual disease and mutilation.

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**VOLUME THE SEVENTEENTH**

SESSION 1923-24

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SECTION OF SURGERY

[Including the *Proceedings* of the SUB-SECTION OF PROCTOLOGY]



LONDON  
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## Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

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### DISCUSSION ON THE SURGERY OF THE HEPATIC AND COMMON BILE-DUCTS.

Dr. WILLIAM J. MAYO.

*(Rochester, Minnesota.)*

To the clear brain and clever hands of Lawson Tait, the father of modern abdominal surgery, who early made excursions into this field of biliary surgery, we owe much, but to the Leeds school, more than to all other influences, we owe the knowledge and methods which have made the upper abdomen, from a surgical standpoint, one of the best understood and most satisfactory regions of the human body. Robson's publications and his untiring work as an investigating surgeon made him the leader for his generation in surgery of the biliary tract, and in Sir Berkeley Moynihan, his successor, we recognize the ablest exponent of the principles and practice of surgery in this field.

#### THE MATERIAL OF THE MAYO CLINIC.

The President of the Section of Surgery of the Royal Society of Medicine has asked me to open the discussion of the subject of surgery of the biliary passages from the standpoint of the surgeon and to deal with the art, rather than the theory and the science of surgery. Perhaps I can best accomplish this by discussing the material from the clinic with which I am connected, outlining the viewpoint of the various surgeons working in the clinic, and the methods of treatment which they have found most effective. I shall discuss the common duct and the main hepatic duct because the pathologic processes with which the surgeon is concerned in this special field must be treated as a whole.

In the period from December 31, 1890, to December 31, 1922, there were 16,980 operations performed on the biliary tract for all conditions, acute, chronic, and malignant, by the surgeons on the general staff of the clinic, with an average mortality of 2·6 per cent. Of these operations, 1920 were on the common and hepatic ducts with an average mortality of 7·8 per cent. In the ten years from 1910 to 1920 the average mortality for operations on the common and hepatic ducts was 6·8 per cent. In 1921 the mortality for operation on the common and hepatic ducts was 5·6 per cent. In 1922, by reason of better pre-operative preparation of patients, the mortality dropped to 3·8 per cent. for operations on the common and hepatic ducts, and to 1·6 per cent. for 942 cholecystectomies.



## 2 Mayo : *Surgery of the Hepatic and Common Bile-ducts*

All patients dying in the hospital after operation, without regard to the length of time thereafter, or the immediate cause of death, were classified as having died from the operation. It may seem somewhat severe to classify patients as having died from operation, who, when operated on, had chronic nephritis, hepatic insufficiency from biliary cirrhosis, and secondary cardio-renal disturbance, the result of months of cholæmia and duct infection, and patients who had died in the hospital some weeks after operation from causes not connected with it. However, without an arbitrary standard of classification, it is difficult to secure comparable statistical data from different hospitals. Perhaps, too, there is a certain stimulation in holding ourselves to a high standard of responsibility.

Satisfactory improvement, so far as mortality is concerned, is manifest by these data. Improvement has been greater than would be apparent by a study of mortality alone; because of constantly increasing knowledge and improvement in technique more and more severe cases have been undertaken, and operations carried out successfully which, in the earlier days, were not attempted.

### THREE FUNDAMENTAL PRINCIPLES.

Certain fundamental principles which greatly affect the welfare of surgical patients must be evaluated. They concern: (1) mortality from the operation, (2) benefit from the operation, and (3) disability following the operation.

*Mortality from the Operation.*—On one occasion, to the great delight of an American audience, Sir Berkeley Moynihan said: "Statistics can be made to tell anything, even the truth." This is especially true of mortality percentages following surgical operations. The pride of the operator and his statistical skill in honestly juggling percentages make most astonishing apparent differences in statistics which are nearly identical. For instance, the early transference of the dangerously ill patient to the medical side of the hospital, because of a medical complication, is helpful from the standpoint of surgical statistics. If operations, rather than cases, are counted, and a number of operations are performed on the same patient, a small series of cases may make a large series of operations. Mortality estimated by cases is high, estimated by the number of operations low, although the number of deaths would be the same. Again, a slight operation which does not cure will be a test in a bad case. If the patient does not react well, the curative procedure entailing a major operative risk may not be undertaken: consequently the patient is not given the chance for cure which a primary radical operation would have given.

*Benefit from Operation.*—We study surgical tragedies and endeavour in every way to hold operative mortality at the lowest point, but the mere fact that a patient recovers from an operation is not in itself sufficient. He was alive when the operation started and if he does not receive sufficient benefit to warrant the risk of life, the pain and suffering from the operation itself, the expense, and the loss of time, he has just cause for dissatisfaction. On the other hand, if a more radical operation would have resulted in correspondingly greater benefit, an increase of primary risk might have been justified.

*Disability following Operation.*—The question of post-operative disability is important. A surgical procedure should be planned so that the patient, with the least possible risk and loss of time, will receive the greatest possible benefit. To-day industry is on a full-time basis and every unnecessary day that the patient is disabled is an economic loss. To perform several operations when

one would suffice, and thus attain an apparent, but not a true reduction in mortality, to use a type of incision not strictly indicated for the work at hand, or to use unnecessary drainage which will confine the patient to bed longer, or leave him with a greater liability to hernia, is unjust, and whether the patient is paying his own way, or is the occupant of a charity bed in the hospital, his own money or that of the hospital donors is being wasted. This economic loss is illustrated by a comparison, at ten-year intervals, of the hospital morbidity following operations on the biliary tract. The methods in use to-day, as compared with methods used ten years ago, save for each patient operated on in the clinic ten days of hospital time, or thirty-six years of the lifetime of one person each year. I will outline briefly the general plan of procedure that is used in our clinic in operations on the hepatic and common ducts.

#### GENERAL PLAN OF PROCEDURE.

*Incision.*—The incision used in the majority of operations on the biliary apparatus has been that introduced by Bevan in 1908, slightly modified. It is started high in the angle between the costal cartilage and the right side of the sternal notch and extends down not more than 1.5 or 2 cm. from the middle line to a point opposite the umbilicus. The method of McArthur in leaving undivided the posterior aponeurosis, the peritoneum, and the nerves in the lower third of the incision is followed, because these are sufficiently mobile to be drawn down readily by a retractor. I have not known hernia to follow in the part of the incision so protected. The upper and lower angles of the incision are made from 4 to 5 cm. longer through the fat and subcutaneous tissues, which have no retentive power, than through the external aponeurosis and the muscle, which enables good exposure with less muscle damage. This type of incision permits ready exploration of the entire abdomen and, if cholecystectomy or appendectomy is necessary, the gall-bladder and usually the appendix can be brought into view. A middle-line incision is very effective, especially if splenectomy is necessary at the same time. Occasionally the transverse incision is used. It is understood, of course, that in the individual case any type of incision for the better exposure of the operative field will be adopted.

*Exposure of the Common Duct.*—The exposure of the common duct is the most important step in the operation. Since that part of the common duct which is visible when the liver is retracted passes more or less to the left, the inexperienced operator may forget that when the duct passes behind the duodenum it turns sharply downward and becomes parallel to the long axis of the body, close to the middle line. It has been shown that in about two-thirds of the cases the common duct runs through the head of the pancreas, and in about one-third behind it, which determines the effect of coincident pancreatitis on bile drainage in gall-stone disease and other infections of the bile-ducts. The gall-bladder and cystic duct are the ready guides to the common duct, but it should not be forgotten that the cystic duct is not at the bottom of the gall-bladder, but to the inner side, and that the pelvis of the gall-bladder may overlap the cystic duct and cover the preduodenal common duct. The pelvis of the gall-bladder must be carefully separated from the common duct and drawn upward until the cystic duct is exposed. While ordinarily the cystic duct passes directly to the common duct, not infrequently it opens on the posterior or upper surface of the common duct, or runs parallel with it for a distance, possibly even to the duodenum. Failure to recognize such anomalies may lead to accidental injury to the common duct. A second not

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infrequent source of accidental injury, especially to the hepatic duct in the operation of cholecystectomy, lies in the failure carefully to secure the cystic artery, which usually lies behind the cystic duct, close to the liver in the gall-bladder notch. When cut, the cystic artery promptly reacts and gives rise to a sharp hæmorrhage. If the operator uses rat-tooth forceps in this bloody angle he is likely to bite out a portion of the hepatic duct, an injury which, if not promptly repaired, leads to permanent obstruction. When a cystic artery escapes, a little basket should be made by a non-biting curved forceps, grasping the tissue round the bleeding point until the artery can be lifted away from the hepatic duct in a manner to expose the vessel safely for ligation. As Brewer points out, the cystic artery occasionally comes from the superior pancreatico-duodenal, and runs up the face of the exposed common duct; if it is cut a very troublesome hæmorrhage results.

#### SECONDARY OPERATIONS.

Secondary operation on the common and hepatic ducts for the removal of stones, especially if the gall-bladder has been removed at the first operation, may be most difficult, particularly if dense adhesions bind the area in a confused mat, and a state of hepatitis or biliary cirrhosis makes the liver bleed at a touch. Ordinarily, in these cases, the common duct runs in the lower margin of the gastro-hepatic omentum with the portal vein and hepatic artery behind and above, but the portal vein may be displaced and opened accidentally. However, if the opening in the portal vein is grasped with the thumb and finger, the hæmorrhage can be easily controlled temporarily, and several light, curved compression forceps without teeth slipped under the fingers to hold the injured vein until it can be sutured with light catgut, as it is held and covered with fragments of the gastro-hepatic omentum. In secondary operations on the common and hepatic ducts following cholecystectomy, if the surgeon finds the place in the gall bladder notch from which the cystic duct has been removed, and follows this close to the liver, aspirating frequently with a glass syringe and a long fine needle until bile is discovered, he may really save more time than by quicker methods which may lead to troublesome accidents.

So far as the removal of stones from the exposed duct is concerned, it is not difficult ordinarily. But it is sometimes difficult to determine whether all the stones have been removed from the duct. Even when the duct is sufficiently large to admit the finger, for careful examination of its interior, stones may sometimes be left because they are high in the liver ducts where they cannot be detected, or they may be hidden in diverticula and the instruments pass by them into the duodenum. For this reason, after all the detected stones have been removed, an opening should be made by gentle instrumentation through the papilla into the duodenum, sufficient to permit undetected stones to pass into the duodenum later in any case. If the common duct approaches 1 cm. in diameter it should be explored, because stones, especially of the Fenger ball-valve type, may be present with very little physical evidence; in 30 per cent. of patients operated on for stones in the common duct, little or no jaundice exists at the time of operation. The importance of removing all stones from the duct cannot be over-estimated. In nearly one-third of the deaths which followed operation on the common duct for stone in our series, post-mortem revealed that all stones had not been removed. Since post-mortem examinations are made on more than 90 per cent. of patients who die in the hospital, this checking up has been of very great

importance in adding to our knowledge, although often humiliating to the surgeon. Perhaps some of the stones which are supposed to have re-formed in the common duct are "left-overs."

#### THE QUESTION OF BILE DRAINAGE.

*Duct Drainage after Operation.*—Davis, by animal experimentation, was the first to call attention to the fact that bile drainage following operation on the common duct is necessary in the jaundiced patient when marked duct infection exists. Kehr, whose large experience and high mortality following operations on the biliary tract, give him an unusual opportunity for post-mortem examination, especially insists on hepatic duct drainage. In a series of cases of this particular description it was found wise to use hepatic duct drainage by the Robson catheter method. Following explorations for removal of quiescent or latent stones from the common duct, hepatic duct drainage is unnecessary. The duct is closed with interrupted catgut sutures, and a soft rubber drain is carried down into the pouch, the early description of which by the distinguished surgeon of Newcastle-upon-Tyne has given it the name in the States of "Morison's pouch." If there is much oozing of blood, especially from lacerations of the liver, it may be wise to carry a strip of selvedged iodoform gauze over the injured surface of the liver where it readily becomes adherent, and as the portal blood pressure is but 30 mm. will usually check oozing from slighter injuries which do not require suture.

*Restoration of Normal Bile Drainage.*—In my experience, next to gallstones in the hepatic and common ducts, operative injuries during cholecystectomy are the most common cause for operations on the common duct. The most serious and difficult operations are those the purpose of which is the restoration of totally interrupted biliary connexion between the liver and the intestinal tract. When the common or hepatic duct has been injured and the fact has been recognized at the time, failure successfully to repair the injured duct has not occurred in a single instance. The tissues are normally pliable and movable to a considerable degree, and if as much as 2 cm. of the duct is lost, the severed ends can be drawn together by end-to-end suture and union takes place. If an anæmic jaundiced patient is operated on weeks or months after the injury the story is very different.

In a period of twenty-two years, 104 patients were operated on for restoration of function between the hepatic and common duct and the duodenum, with a hospital mortality of fifteen. The late mortality in all the years since the operation was nineteen. Twelve of these late deaths were due to progressive biliary cirrhosis, one to carcinoma of the pancreas, four and a half years after operation, and four to other causes not connected with the operation. Of the remaining seventy patients the present condition of sixty-three is known; forty-five are well, seven in fair health, and eleven continue to have more or less symptoms. A study of the operative methods employed in these cases, from the standpoint of ultimate results, indicates that in any case in which a piece of the duct has been accidentally removed, and the injury not discovered and repaired at the time, thus necessitating secondary reconstruction, direct union between the stump of the hepatic duct and the duodenum is the best operation. One patient on whom I performed this operation, in 1903, has remained well, raised a number of children, and has survived typhoid fever and pneumonia. The ability of others of these patients to withstand the vicissitudes of life has been equally

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well demonstrated. The best results were obtained when a muco-mucous union between the proximal end of the hepatic duct and the duodenum could be effected. The method of Walton was used in two of the successful cases. A rubber tube connexion after the Sullivan method, which was popularized by Wilms, gave good temporary results in a few very difficult cases, but eventually results were unsatisfactory. The use of a T-tube did not prove favourable, as its removal had a tendency to disturb the line of union. If a rubber tube is used, it is fastened in position with catgut and allowed to pass out spontaneously through the intestine, as it did eventually in our cases. When it is possible to bring the distal end of the common duct in apposition with the proximal portion by suture the use of a piece of catheter, with a cuff on the end inserted into the hepatic duct and the distal end passed through the common duct into the duodenum as advised by McArthur, gives excellent function. While the results in this series of cases have not been all that could be desired, they are not wholly unsatisfactory when the desperate pre-operative condition of many of the patients is considered.

### MALIGNANT DISEASE OF THE COMMON DUCT.

In a number of instances sections of the common duct have been removed for cancer without a single ultimate cure, although in several instances the primary results were very satisfactory, one patient living for three years. Cholecysto-gastrostomy in malignant obstructions, whether in the head of the pancreas or in the common duct, has proved a remarkably satisfactory operation. For benign disease cholecysto-duodenostomy is preferable theoretically and it is usually practised in the clinic. In several doubtful cases which proved to be benign, cholecysto-gastrostomy has given good symptomatic results after many years.

### PRE-OPERATIVE PREPARATION OF CHOLÆMIC PATIENTS.

Of the causes of death after operation, hæmorrhage and renal insufficiency, and infections of the bile-ducts are the most common. These conditions are directly related to the existing chronic obstructive jaundice, hepatitis, biliary cirrhosis, dehydration, and chronic under-nourishment. Walters reviewed the post-mortem records, for a five-year period, of patients who died following operation on the biliary tract, and demonstrated that in 58 per cent. of the patients *with jaundice*, who died within the first week after operation, there was more or less blood in the abdominal cavity, usually the result of oozing from slight injuries to the liver. The hæmorrhage alone was not always sufficient to cause death, but it was a contributing factor. Walters then made a direct comparison between the cases with jaundice in which cholecystectomy had been performed and those in which cholecystostomy had been performed in conjunction with operations on the common duct. He found that in the cases of jaundice with infected ducts the death-rate following cholecystectomy was much greater than that following cholecystostomy. In jaundiced patients in poor condition, unless definitely indicated, a cholecystectomy is not added to the risk of the operation on the common duct, because of the danger to the liver which adds to the possibility of slow post-operative oozing.

*Dehydration.*—As these patients are dehydrated, and usually unable to take much nourishment, an attempt is made before operation to introduce a quantity of water into the system to aid renal elimination. As a rule it is difficult to accomplish this by mouth; it is best accomplished by proctoclysis, or subcutaneously. Walters found that the jaundiced patients who died

following operation regularly had terminal nephritis with gradually increasing blood-urea before death, and that the result could be predicted as the blood-urea rose day by day.

*Hepatic Insufficiency.*—A third factor of importance in these cases is hepatic insufficiency, which runs parallel with renal insufficiency. In the presence of hepatic insufficiency, the blood-sugar may appear to be at the normal level when it is not truly so, because of the concentrated state of the blood from dehydration. Mann, in the clinic laboratories, has shown that if the liver is removed from a dog the animal goes into collapse in a few hours and dies. If, however, the dog just before death is given glucose in saline intravenously he is relieved at once, regains his strength rapidly, and in a few minutes appears to be restored. In such experiments, by continuing the glucose, dogs were kept alive for from twenty-four to thirty-six hours. Therefore, in the presence of hepatic insufficiency, 5 per cent. of glucose in plain water by rectum, or 3 per cent. in sodium chloride solution, is given subcutaneously. For subcutaneous administration the glucose must be sterile and chemically pure, to obviate late abscess formation. If the patient is very sensitive small amounts of procaine added to the glucose in sodium chloride solution, as recommended by Bartlett, will make it possible to give the infusion painlessly.

*Hæmorrhage.*—In order to check hæmorrhage, chloride of calcium, intravenously, has proved effective, and in certain cases blood transfusion is a remedy of remarkable efficiency. The blood contains prothrombin, which starts the clot, and antithrombin, which tends to prevent the prothrombin from clotting. When the tissues are cut, thromboplastin, which escapes from the tissues, unites with the antithrombin, and the prothrombin clots. The blood and tissues of the patient with hæmophilia are lacking in these three factors, and therefore his blood does not clot. Hæmophilia is transmitted through the female to the male; no thoroughly authenticated case of hæmophilia in the female has been noted. The remedy is blood transfusion; there is no cure. The second constituent of the clot is formed by the blood platelets. In purpura hæmorrhagica the blood platelets are deficient. The third constituent of the normal blood-clot is calcium. In the jaundiced patient the blood calcium is exhausted by combination with bile-pigments and acids, and this exhaustion may take place as early as three weeks after the onset of the jaundice, or, in some instances, calcium exhaustion may be delayed for many months. Failure of normal blood-clotting in the jaundiced patient is a specific indication of deprivation of blood calcium. Wright was the first to point this out. Cammidge and Robson added to the knowledge of the subject both experimentally and clinically. The difficulty was that the amount of calcium taken up either by the stomach or the rectum was always small, and in some patients was not observed. It remained for Lee and Vincent to give chloride of calcium in a 10 per cent. aqueous solution intravenously, with striking results. Sometimes, following a single injection, a clotting time even of thirty minutes would be reduced to nearly normal. Three injections on successive days usually are sufficient to restore blood-clotting to normal. The estimation of the blood calcium is made by a simple method described by Sanford. When the administration of calcium fails to reduce the clotting time to normal, blood transfusion will usually make a temporary reduction in the clotting time, sufficient for an operation to be performed.

The gradual development of these excellent methods of blood rehabilitation in the clinic was begun by Hallenbeck; later, Bell advanced the work, and it

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was carried to the present stage of efficiency by Walters. The careful pre-operative management of jaundiced patients has greatly reduced the mortality from operations. In two years not a single patient so prepared died from hæmorrhage following the operation.

Sir BERKELEY MOYNIHAN, Bart., K.C.M.G., C.B., F.R.C.S.

### **The Recurrence of Symptoms after Operation on the Common Duct.**

THE subject allotted to me in this discussion concerns those cases in which a recurrence of symptoms takes place after an operation upon the common bile-duct. About one-fifth of the total number of patients suffering from cholelithiasis upon whom I operate have previously been submitted to operation for this condition. Secondary, tertiary, or multiple operations upon the biliary system may be made necessary by a variety of causes, among which the chief are the nature and condition of this disease, and of its complications and remote effects; and imperfect primary operations.

#### *Factors in Stone Formation.*

A study of the nature and condition of the disease must chiefly have regard to the factors bringing about the formation of stone. These factors are dual: infection reaching the bile channels, and an increase in the cholesterol content of the blood. The great majority of the stones which require removal are formed in the gall-bladder; when stones are found in the ducts it is because they, or the small particles from which the larger stones develop, have escaped from the gall-bladder. But there are cases, happily few in number, in which stones are formed in the smaller ducts within the liver, or in the larger ducts which lead to the intestine. An infection may reach the gall-bladder from several directions—from the blood-stream; from the bile flowing downwards from the liver; from the lymphatic plexuses of the appendix, pancreas, liver, &c., which most freely anastomose with that of the gall-bladder; from the common duct, which in turn receives infection from the duodenum; and finally, by contiguity through adhesions to neighbouring organs. Though these are the direct and proximal routes, it is evident that the infection travelling along them is not necessarily a primary infection in the organ from which it is immediately derived. Infection coming from the liver may be primary elsewhere in the body, gain access to the blood-stream, mature in the spleen, and ultimately reach the liver by way of the splenic vein. A search for infective lesions must therefore involve a scrutiny of the whole body, so that the most lurking area of infection may be revealed. A failure to discover such an infection may be responsible for a recurrence of stones in the gall-bladder, the liver, or its ducts.

The cholesterol content of the blood is an important agency in the causation of gall-stones. We have still a good deal to learn with regard to cholesterin, and its presence in the blood in excess in certain conditions. Blood cholesterol determinations have been made for me by Miss Shiskin on a large number of patients suffering from various abdominal disorders, and one hundred consecutive cases have been scrutinized for the purpose of this review. In conditions other than cholelithiasis or cholecystitis, low or normal values

have been found to be the rule. In uncomplicated cases of cholelithiasis, on the other hand, the cholesterol content of the blood tends to be high. Thus in forty such cases examined the blood cholesterol values ranged from 0·133 per cent. to 0·409 per cent., the average value being 0·210 per cent. If we take 0·160 per cent. as the average normal figure, with 0·192 per cent. and 0·133 per cent. as the upper and lower normal limits, 60 per cent. of the cases are seen to be hypercholesterolaemic, and, although in the remaining 40 per cent. the cholesterol content of the blood is normal, it is to be noted that in three-quarters of these (that is 30 per cent. of the total) high normal values have been obtained.

In all the cases of cholelithiasis examined, a marked fall in the cholesterol content of the blood was observed shortly after the operation. This fall is not confined to cases of cholelithiasis. I have noted it in diverse other conditions after major surgical procedures, for example, after prostatectomy, gastrectomy, splenectomy, &c. In the cases under discussion the fall is to be attributed sometimes to the direct drainage of the biliary passages, but the effects of the anaesthesia, the slight elevation of temperature sometimes present after an operation, and, indeed, the disturbed metabolism consequent on operation, must all be regarded as contributing factors. Subsequent re-examinations show that after the initial fall there is a rise, in some cases a progressive one, so that the cholesterol content of the blood may soon reach the pre-operative figure. Such cases belong to the diathetic group; that is to say, they are more or less continuously hypercholesterolaemic, and even after operation for cholelithiasis the hypercholesterolaemia may persist. In the majority of the patients, however, the cholesterol content of the blood had fallen to normal and was found to have remained at the normal level when examined fourteen months after the operation.

The treatment of the patient after operation must therefore take into account this condition of hypercholesterolaemia. The following instructions as to diet are given to all patients after operation:—Articles of diet which must be avoided: Yolk of egg, brains, kidneys, sweetbreads, liver, goose and duck, pork and game, fat, stewed and fried meats, cream and cheese, fresh fruit and green vegetables (for three months), butter (only to be taken in small amounts, if at all). A recurrence of symptoms after an operation upon the common bile-duct may also depend upon the following circumstances.

#### *Imperfect Primary Operations.*

(1) Stones may be overlooked or left behind in any part of the extrahepatic ducts, including the ampulla of Vater. There are times when even a practised surgeon may have his doubts about the presence of a stone in the common duct. In cases when the gall-bladder is thick and shrivelled, and has ceased to perform its functions, the common duct may be found not a little dilated. After the gall-bladder has been removed the same change constantly occurs. What Rutherford Morison calls the “natural cure” of cholelithiasis consists in the closure of the cystic duct and the shrinking of the gall-bladder until it becomes a mere fibrous or calcareous covering for the stones remaining inert within its cavity. The common duct then may be obviously, even grossly, dilated. The question of how best to decide whether such a duct contains a stone is one that surgeons answer differently. The enlargement of glands along the duct and the presence of hard nodules in the head of the pancreas may cause the surgeon to feel almost confident that a stone is present. And in such circumstances the opinion is very generally given that the duct



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should always be opened and "explored." I disagree most confidently. Exploration of the duct with anything but the finger is a most uncertain method of discovering or locating a stone. A probe or a small scoop will miss a calculus, even one of good size, over and over again, especially if it happens to be in a little pocket aside from the main channel of the duct. Beyond any question the surest method of deciding whether the duct contains a stone is to pass the first and second fingers of the left hand through the foramen of Winslow and the thumb in front of it, and to palpate the duct and compress it between the fingers and thumb, as they travel gently upwards and downwards. By the expert fingers the tiniest fragment of stone can be felt as soon as the duct is empty of bile. The left hand of an assistant, if he, too, is expert, should carry out an independent investigation. During the last few years I have rarely opened a common duct merely for the purpose of deciding whether it contained a stone. The exploration of a sufficiently dilated duct can be carried out with the left forefinger passed inside it with the extreme probability that with adequate skill and unwearied patience the last fragment of stone will be discovered.

When a stone which lies concealed in the ampulla of Vater has been overlooked at the primary operation, the duodenum should be opened and McBurney's or Kocher's operation performed. After either of these operations the drainage of bile into the duodenum is very free and unimpeded. I am very fond of both these methods. They can be performed with the utmost daintiness and precision, with extremely little risk, and with the confident prospect of a perfect result. The making of a large opening at the lower end of a dilated duct, with, or more often without, the suture of the duct edges to the duodenal wall, will allow stones of even medium size to escape readily into the intestine. I feel sure that these operations are not practised sufficiently often. I am beginning to entertain a belief that they may be the safest of all operations upon the common duct, and those in which there is the most confident prospect of complete relief for the future. At least I can claim that their range should be greatly extended. Even when it is not necessary to open the duodenum the dilatation of the lower part of the duct from above is a most useful procedure. It may be effected by the passage of probes of the type suggested by Dr. Mayo, or by the passage of a rubber catheter through the duct into the duodenum. A later reference will be made to this procedure.

(2) Particles of grit may be left in the lower intrahepatic ducts, or in the hepatic or common duct, and inadequate drainage may not give them time to escape. The question of drainage after the operation of choledochotomy is one that still exercises the minds of surgeons, and certain surgeons who ought to know better are practising and advocating the immediate suture of the duct after every operation upon it. In every case of obstruction of the common duct by calculus, there is cholangitis, and in a large number of cases there is mud, or dust, or sand, in addition to the larger stones. When there is a large solitary stone of recent descent from the gall-bladder, the closure of the duct may sometimes be safely performed. I have done this a few times in years gone by, always with a feeling of anxiety during the few days following operation, and with no ultimate advantage of any kind to the patient. I make it a rule, and have done for many years, to drain the hepatic duct in all cases of multiple stones. Some of the patients are gravely ill, and in these cases the method of McArthur, namely, passing a catheter down the duct into the duodenum for the purpose of giving fluid nourishment, is invaluable. The

catheter acts by the method of "continuous dilatation," which Sir Henry Thompson originated and wisely advocated for the treatment of urethral stricture. Any narrowing of the common duct is, for a time at least, completely overcome, and during that time the smaller stones or the coarse dust and mud of the ducts above are able to escape into the intestine. I give through this tube a 5 to 15 per cent. solution of glucose, with a little carbonate of soda, by the "drip" method, interrupting the steady flow every three or four hours, to give a meal of peptonized milk, beaten eggs, &c. As much as 10 to 12 pints of fluid a day may be introduced in this way with great advantage. The tube remains for ten days to a fortnight.

When many stones and much mud have been removed from the common and hepatic ducts, small particles of grit may long continue to descend upon the gauze strips passed up into the ducts to clear them. In such cases I pass Carrel tubes up into as many of the intra-hepatic ducts as I can recognize. These ducts are sometimes so large as easily to contain the finger, and there is no difficulty in passing tubes into them. Around the common duct incision a barrier of dental rubber is placed. At the end of twenty-four to thirty-six hours, when the peritoneal cavity is sealed off, I start the intermittent irrigation by the method of Carrel, washing the ducts out with large quantities of sterile salt solution or plain boiled water. In one such case the nurse counted several hundred stones, weighed them, and finally weighed the total mass of tiny stones and particles of coarse dust washed down from the liver. She assessed their total number in this way, as "well over a million!" The operation took place four years ago, and there has been no recurrence of symptoms, though the operation I performed was the fourth attack made on the common duct. For the first ten days an abundance of fluid was given through a catheter passed into the duodenum. This case raised the question as to the origin of the infection responsible for the formation of the stones. It would appear that in such cases the most likely route of delivery of infective material to the liver is the splenic vein. In a later case than this, where the spleen was slightly enlarged, I performed the sixth operation upon a patient, drained the blocked hepatic ducts of many thousands of small stones, and then removed the spleen. The washing out of the ducts continued for six months; after that time the bile drained away through the wound, which has closed and broken down again several times since, but the formation of stone appears definitely to have ceased. I therefore advocate drainage of the hepatic duct in all cases of multiple calculi; drainage with irrigation of the ducts in many of the cases, especially the secondary ones, in which the ducts contain great quantities of mud, with the employment of the method of McArthur of catheterization of the lower part of the duct; and in cases in which the lowest part of the duct contains calculi, a more frequent resort to the operation of McBurney. If typhoid organisms are found in the bile the administration of urotropine by the "thump" method is necessary. Thirty grains of acid sodium phosphate in 1 pint of water are taken during the day, and at night a single dose of urotropine beginning at 30 and increasing to 60 gr. is taken. At the end of a week a respite of a few days is allowed during which alkalies are given. The administration of urotropine again begins and is again interrupted.

(3) In cases of primary cholecystectomy failure to divide the cystic duct at the proper place may be responsible for disaster. The variations in the mode of entry of the cystic duct into the common duct and of the variations in the vascular supply of the gall-bladder and ducts have not been sufficiently considered by surgeons in the past; the recent work of E. R. Flint in this con-

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nexion may be studied with great advantage. The cystic duct not infrequently runs for an inch or more side by side with the common hepatic duct, to which it is closely bound, until it enters with it into the formation of the common duct. The two may, indeed, not unite until they almost reach the duodenum. If a long piece of the cystic duct is left behind a stone may lie within it and there increase, or may quietly migrate from it and escape into the common duct. On the other hand, too much of the cystic duct may be removed, and with it a piece of the common duct, or of the hepatic duct. The pelvis of the gall-bladder is at times so intimately adherent to the common duct that when the gall-bladder is lifted up to enable the cystic duct to be cleared, the common duct may be stretched, and then so closely resemble the cystic duct that the greatest care is necessary to distinguish them. The inviolable rule in the surgery of the gall-bladder and of the common duct is this: "See exactly what you are doing, and until you see do nothing." On four occasions I have removed a part of the common hepatic or common bile-ducts, happily not to a degree prejudicing the recovery of the patient, because immediate repair of the duct was performed. If this tragedy should occur it should be redeemed at once. The parts are then all in view, and supple, and restoration is not by any means as difficult as it will prove to be in later weeks or months. It is the surgeon who bungles in this fashion who is then the only sufferer. He feels that the operation has not displayed that degree of ease and mastery in efficient action which stamps the artist; and he has the misery of knowing that he has given less than his best to his patient.

If too much of the cystic duct is removed, and with it a part of the common duct, a stricture of the latter may result. This stricture may itself be responsible for symptoms which closely resemble those of a floating stone, or may be the means of causing that degree of stagnation of bile which leads to infection and makes the formation of a stone easier. A few cases have come my way in which a complete segment of the common duct had been previously excised, and I have had to reconstruct the duct or to anastomose the hepatic duct to the duodenum.

### *Improper Operations.*

At the primary operation upon a case of cholelithiasis when stones are present both in the gall-bladder and in the common duct, the question as to the best procedure to adopt with reference to the gall-bladder will always arise. At a time when operations were performed with difficulty and attended by grave risk, at a time, too, when relief from symptoms after operations by competent surgeons was less frequent than it is now, it was felt that removal of the gall-bladder was unwise. If mistakes were to be made at all, it was safer to make the mistake of leaving the gall-bladder than the mistake of removing it; the former error could be made good, the latter was almost irreparable. But as the quality of surgery improved, and as by degrees we learnt that recurrence of stones in the common duct was so frequently due to recurrence first taking place in a gall-bladder which had only been drained, and that pancreatitis might arise—and often probably did arise—by extension from the walls of a diseased gall-bladder, the practice of the removal of the gall-bladder grew more common. A study of its walls often showed that they were filled with organisms, and that the elastic and muscular coats were so thoroughly disorganized, and the mucosa so much changed, as to render the viscus incapable of adequately fulfilling its functions either as a reservoir competent to discharge its contents on demand, as a regulator of the rate of

discharge of bile, or as an organ secreting a constituent which it was proper for the bile to possess on entry into the intestine or otherwise. The cases in which the gall-bladder is needed for anastomosis with the duodenum at a second operation are now so exceedingly few that this consideration need hardly weigh with the surgeon as he reflects upon the course he is to follow.

The present practice of most surgeons whose experience is large is to remove the gall-bladder at the time when the stones are removed from the common duct.

#### *Involvement of the Pancreas.*

There are conditions of the pancreas, its chronic or subacute inflammatory states, that may cause a very close mimicry of the symptoms of calculous obstruction of the common duct. The frequency of the association of disease of the pancreas with cholelithiasis is variously estimated by different observers. The pancreatic changes are not easily measured during an operation. It is difficult to gauge the size of an organ so long and so remote, with any approach to accuracy; the qualities of texture in respect of hardness, in one part or another, cannot be expressed in terms that make one surgeon's experience comparable with that of another. Removal of a portion of the pancreas is open to the fallacy that the area chosen may not be equally involved with the rest of the organ. But with all due and necessary allowance for these truths there can be no doubt of the frequency, and the occasional grave severity, of the pancreatic changes. The route by which infection reaches the pancreas is not necessarily the same in all cases. The cause of the cholelithiasis may be the cause of the pancreatitis, or the latter may be the consequence of the former. If the pancreatitis is secondary to cholecystitis the infection may travel from the gall-bladder down the common duct, and along the canal of Wirsung; or it may pass in the lymphatic plexus which is common to the two. Some clinical and all experimental evidence (especially Nordmann's) suggests that invasion by the ducts is the more likely; but there is evidence, too, derived from operations, that lymphangitis may be the chief factor in causing the changes in the pancreas. Whichever may be, in any special case, the correct explanation, it is evidently a safer plan to remove the gall-bladder than to drain it. Cholecystectomy is beginning to displace cholecystenterostomy in the treatment of chronic pancreatitis, as a result, chiefly, of the advocacy and practice of Deaver.

The typical symptoms of calculous obstruction of the duct may occur in the absence of calculus. In operating upon such cases it is a little disconcerting to find a distended common duct in which no stone is present. The head of the pancreas, especially, will be found enlarged, hard and œdematous. The duct lies in such intimate relationship with the gland, that a swelling of the gland compresses the duct, causing stagnation of bile, and consecutively a degree of cholangitis that may be very severe. In such cases the T-shaped tube of Kehr may be used to drain the hepatic duct intermittently, or, as I prefer, two catheters may be introduced, one draining the hepatic duct, the other passing downwards into the duodenum and affording the channel by which abundant nourishment may be given.

#### *Operations on Deeply Jaundiced Patients.*

In all operations upon deeply jaundiced patients two questions are raised: one is that of hepatic insufficiency and its avoidance, if possible, by an adequate preliminary preparation of the patient; the other is that of post-operative hæmorrhage.

## 14 Moynihan: *Recurrence of Symptoms; Secondary Operations*

The symptoms due to the failure of the liver are apt to come quite unexpectedly. The operation has perhaps presented no unusual difficulties, and the patient leaves the operation theatre in good condition. For a few days all goes well; then one or two events attract serious attention. The amount of bile discharged may suddenly diminish or almost cease; jaundice deepens slowly; the pulse becomes slower; the mind is clouded; nourishment is sparsely taken or refused, when it is well taken vomiting begins; the amount of urea in the blood steadily augments, the patient grows rapidly weaker, and dies after a period of coma. In these cases, as Walters has shown, it is renal failure consecutive to hepatic failure that causes death. Or the picture so well described by Walters and Parham may be seen.<sup>1</sup>

*"Clinical Picture of Hepatic Insufficiency.*—Usually the first two to eight days after operation the course is uneventful; biliary drainage and urinary output are normal, fluids are taken well, the pulse and temperature are normal. One of the first signs of the onset of trouble is paling and thinning of the bile with marked increase in the flow. This profuse cholerrhagia does not continue long, but the bile remains thin. The patient begins to grow weaker, the pulse loses volume, and the temperature becomes subnormal. Restlessness and irritability develop, with great fatigue and muscular weakness. Soon regurgitant vomiting follows intake of liquids by mouth and little taken by rectum is retained. Jaundice does not deepen markedly, but there is an increasing pallor beneath the icteric blue, the face becomes drawn, and the eyes anxious. The patient grows progressively weaker and finally dies. The urinary output remains proportional to the fluid intake throughout, and the urine reveals little or no evidence of marked nephritis. The blood-urea remains persistently low."

Post-operative hæmorrhage is due to the reduced power of coagulation of the blood, and this probably depends upon a lack of calcium salts. The deficiency of these is made good in two ways, by direct transfusion of blood, or by the intravenous administration of 5 c.c. of a 10 per cent. solution of calcium chloride on three successive days, as advised by Walters, and by Lee and Vincent. The giving of calcium salts by the mouth even in colloid form has little effect—when it has any—upon the calcium content of the blood. The coagulation time of the blood may be brought down to the normal or to the near neighbourhood of the normal by the intravenous administration and the operation carried out at the appropriate moment. In all these cases it is necessary to administer as much glucose as possible by the mouth and by the rectum, and at the operation to take every care to ligature the smallest vessels. It may sometimes be prudent to postpone the removal of the gall-bladder because of the slow but unceasing weeping of blood from the denuded surface of the liver which sometimes follows upon cholecystectomy.

Secondary operations upon the biliary system are among the most difficult tasks in surgery. To obtain the best results all the highest qualities of the surgeon may be called into play: courage, resource, patience, accuracy and rapidity of judgment, the finest craftsmanship, and a tranquility of mind and action that nothing can disturb.

<sup>1</sup> *Surg., Gyn., and Obst.*, 1922, xxxv, 605.

Mr. JAMES SHERREN, C.B.E., F.R.C.S.

**Stones in the Common and Hepatic Ducts.**

THE presence of stones in the common and hepatic ducts is usually significant of the absence of efficient treatment in the particular patient in whom they are found. It denotes failure in carrying out the only satisfactory treatment of gall-bladder disease—early cholecystectomy—which would have effectually prevented their entrance into the ducts. This failure is due to errors in diagnosis as well as to inefficient treatment. Patients are still too often treated for various types of dyspepsia; that is to say symptoms only are treated, and even after the onset of colic we still see attempts made to avoid operation. There is no medical treatment known at present that will remove the cause of the stone formation or dissolve stones already formed: all attempts at such treatment must fail and the ultimate death-rate far exceeds that of the curative surgical procedure. The relative number of cases operated upon at the stage when this complication has arisen must vary somewhat with every district according to the skill and bias of its practitioners and the skill and reputation of its surgeons. The percentage in Sir Berkeley Moynihan's private patients as given at Leeds last year, 21'96 (67 out of 305), differs little from mine, 18'5, which includes both hospital and private cases (113 out of 600).

It is obvious from my opening remarks that, excepting certain rare pigment calculi associated with disease elsewhere, I do not believe stones form primarily in the ducts. They may, however, increase in size and in number when infection is present and extend in a continuous column from the ampulla of Vater to the liver and may even fill the intra-hepatic ducts. It follows, therefore, that if stones are found in the common duct after a previous operation for gall-stones, they must have re-formed in the gall-bladder, if this was not removed, or they must have been overlooked. They are easily overlooked unless systematic search is made for them and even in that case they may be missed.

*Difficulty of Pre-operative Diagnosis.*

Pre-operative diagnosis is only possible in the presence of typical attacks of colic associated with jaundice, often with rise of temperature. Jaundice is usually present at some time during the progress of the case; thus, in the 113 cases upon which these remarks are based it had occurred in seventy-eight, but only in thirty-three was it severe and present at the time of operation. Many stones may be present in the common duct without producing jaundice, and, on the other hand, a transient jaundice may follow an attack of biliary colic and the ducts at operation will be found clear and the correctness of this confirmed by the subsequent history. In most instances the patient was operated upon for gall-bladder disease and the stones were found in the ducts, not by accident but as a result of a definite exploration. In two of the thirty-three cases in which marked jaundice was present at the time of operation it had never been associated with pain; as a distended gall-bladder could be felt the diagnosis made was that of carcinoma of the head of the pancreas. In the majority there was a history of abdominal symptoms, extending over many years, for which all the recognized forms of medical treatment had been tried.

In most of the cases operated upon the patients were between the ages of 45 and 55; the oldest patient was 76, the youngest 23 with a three months' history. The first symptom in this last case was a typical night attack of biliary colic followed by jaundice, and before I saw the patient she had had six further attacks. I removed five small calculi from the common duct. The patient is now aged 30 and has had no recurrence. It is my impression that stone in the ducts is more often met with in women than in men, probably because of the earlier date at which men seek treatment. In my common-duct series there were twenty-two men. For comparison with this I have taken my last 113 consecutive cases of cholecystectomy in which stones were found in the gall-bladder only; forty-one of these were men. I do not think it is extravagant to prophesy that operations on the common duct will become rarities within the next generation of surgeons, as the students educated in the modern methods of treatment of abdominal disease go out into the world.

#### *Treatment.*

With regard to treatment: this consists in removing the stones and in carrying out cholecystectomy to prevent further stone formation. I shall deal first of all with those cases—and they are the most common—in which operation is undertaken for gall-bladder disease but in which the symptoms give no indication of stones in the ducts. A shrunken gall-bladder, a dilated cystic or common duct, may at once point to the probability. In all these—and in those in which there is a history of jaundice, after removal of the gall-bladder—the common and hepatic ducts should be explored through the stump of the cystic duct, the opening being enlarged, if necessary, by cutting downwards and removing calculi if found. Before the opening is sutured a pair of fine forceps should be passed and the ampulla dilated. The incision should be closed with a continuous stitch of hardened 00 gut and a drain left to the site of incision. This is, in my experience, the most frequent type of operation. Drainage of the ducts is imperative when infection is present or if much manipulation has been necessary in removing the stones. If calculi become impacted they do so in the ampulla, but it is very rarely that any other approach but that mentioned is necessary for their removal. In only two cases have I been compelled to open the duodenum to remove such a stone, and on one occasion I mobilized the duodenum and removed an impacted calculus. All these cases terminated successfully. It has been a common teaching that the gall-bladder is shrunken in most cases of stone in the common bile-duct: this is usually so in late cases with obstructive jaundice, but in only forty-four of my series was there a small gall-bladder; in five of those with jaundice it was reduced to a fibrous cord without lumen. A dilated gall-bladder does not exclude the possibility of stone in the common duct.

To pass now to the cases in which stones can be palpated in the ducts: I still remove these when possible, by cutting into the common duct from the stump of the cystic duct. To be *certain* that the ducts are clear is impossible. On occasions after removing with forceps and scoop what I considered to be all the calculi present, I have been able to syringe several others from the common and hepatic ducts. When faceted calculi or putty-like masses are found in the ducts this syringing should always be done. Dr. Mayo has told us that in nearly one-third of the deaths which followed operation for stone in the common duct, post-mortem examination revealed stones which had been overlooked. Among my cases in which the patients died at varying times

after operation were five in which stones that I had overlooked or been unable to remove were found post-mortem; that is, in half the number of cases in which this examination was made. It is instructive to note that in four of these the operation had been undertaken for obstructive jaundice, and I believe that had I been content with drainage only, leaving thorough search to a second operation, at least two of these lives would have been saved. In two of these cases the stones which I had felt in the duct could not be discovered; in two others stones were removed but small calculi were overlooked in the ampulla. In all the cases stones in the common duct were diagnosed. In only one had the common duct not been explored, that of a female patient, aged 39. I removed a gall-bladder containing stones, but did not explore the common duct and have a definite note that "no stones were felt." The patient died of acute dilatation of the stomach the same night. Post-mortem examination revealed a small calculus in the ampulla. Had I passed a probe, as I should do now in a case with a similar history, I should have discovered it but could not have prevented her death.

To pass on to the treatment of the most serious group of all, those with obstructive jaundice and—usually—cholangitis. I believe that it is unwise to operate in this stage unless forced to do so. I mean that the patient should be watched for a day or two, except in cases with rigors, when immediate operation is necessary. While waiting, saline and glucose should be given per rectum. If the jaundice persists, so making operation imperative, it is well to operate in two stages; this plan is safer and renders stones less likely to be overlooked. If the gall-bladder is distended and the cystic duct patent, cholecystotomy should be carried out; if not, temporary drainage of the common duct, with removal of such stones as are present. These patients stand prolonged manipulation badly, and by this method I have been able to bring to a successful issue cases that would inevitably have been fatal if I had attempted to deal with everything in one stage. At the second operation cholecystectomy is done, the stones are removed and the ducts thoroughly examined in a way that would be unwise in very ill and deeply jaundiced patients.

#### *Prognosis.*

With regard to prognosis: there were eleven deaths in the 113 cases, nine of the patients were among the thirty-three who were operated on for obstructive jaundice, and in four, stones were overlooked. In two the ducts were filled with colourless fluid, the so-called "white bile." Both recovered, and the excretion of bile was established within thirty-six hours. It is obvious that delay increases the mortality; the longer the period of jaundice the greater is the risk. In none of these cases did death result from hæmorrhage, and I have not, therefore, found it necessary to resort to pre-operative treatment by blood transfusion or the administration of calcium. Three patients died of lung complications, one under the anæsthetic, the remainder from the tenth to the eighteenth day, from hepatic insufficiency. The two deaths among the eighty patients who were operated upon at a time when they were not jaundiced, were due, one to acute gastric dilatation, the other to pneumonia. These figures illustrate the fact that the risk depends on the condition of the patient with regard to jaundice and infection; cholecystectomy for stones, when these are confined to the gall-bladder, has a death-rate of less than 2 per cent.

What is the ultimate outlook in the case of patients who have had stones



removed from the common ducts? The cases in which I carried out cholecystectomy and in which cholecystotomy had been performed are almost equal. In the first group (fifty-seven) there has been no recurrence of symptoms sufficiently severe to need operation and only two in which post-operative colic occurred, and in both this was within a few weeks of operation and did not recur. In one, however, a stone was overlooked and found post mortem. In seven of those cases in which the gall-bladder was left, further operations were necessary for removal of calculi from the common duct and in one for chronic cholecystitis. The original operation was done by myself in only one of these. The number of stones removed from cases in the first group varies, but even after the removal of hundreds—and in one case the amount was 5 oz.—no recurrence has taken place.

In conclusion, I should like to emphasize the fact that stones in the ducts constitute an entirely unnecessary complication, and that when the correct treatment for disease of the gall-bladder is carried out we shall cease to have to discuss and carry out operations for the removal of duct calculi.

Mr. G. GREY TURNER, M.S.

### **Injury to the Common Bile-duct and the Technique of Operations on the Ducts.**

AS a contribution to this discussion I shall refer briefly to four of my own cases of surgical injury of the larger bile-ducts as they so well illustrate the difficulties and pitfalls connected with this unfortunate accident.

*Case I.*—A female patient, aged 56, on whom I operated for gall-stones in September, 1919. The case was complicated by the presence of a large diverticulum springing from the second part of the duodenum. When the abdomen was opened the gall-bladder and duodenum were found to be covered by a mass of adherent omentum. During separation of the omentum, the lumen of some part of the bowel was opened and this proved to be the end of the unsuspected diverticulum. The hole was temporarily clamped while the investigation of the gall-bladder was undertaken. The latter was shrivelled and thickened and apparently of hour-glass form, and contained stones. The viscus was readily separated from the liver and what was supposed to be the cystic duct was exposed, clamped and divided; but there was still a duct from which bile escaped. This escape of bile suggested that the common duct had inadvertently been divided, and this proved to be the case. The ends were identified and held with forceps while attention was turned to the diverticulum. This was found to be as large as a thumb and originating from the posterior area of the second part of the duodenum. A process of the pancreas extended into its wall and many large vessels had to be divided before it could be isolated and removed, leaving a large opening into the duodenum. With considerable trouble the bowel was repaired but this process caused considerable narrowing. The pylorus was therefore occluded by a strong catgut ligature, tightly tied and buried by Lembert sutures, and posterior gastro-enterostomy was performed. Attention was then again turned to the bile-duct. Although at least an inch had been removed there was no difficulty in getting the ends into apposition but the

lower segment was much smaller than the upper which was just at the junction of the hepatic ducts. Two-thirds of the circumference were easily repaired by interrupted sutures of fine catgut and a soft rubber drain was brought from the suture-line to the surface. Unfortunately the patient developed an uncontrollable duodenal and pancreatic fistula from which she died four weeks after the operation. At the necropsy the parts were found much altered as a result of auto-digestion but the bile-duct was intact. There was another diverticulum springing from the anterior wall of the second part of the duodenum and there were four of much smaller size in the upper three feet of the jejunum. Upon a cursory examination of the removed gall-bladder it appeared as if the cystic duct was entirely absent, the gall-bladder apparently opening directly into the common duct. A more critical inspection showed that the cystic duct was present but so much shortened and thickened as a result of inflammation as to be almost indistinguishable among the inflammatory scar tissue.

There was no difficulty about the immediate repair of the duct and I have no doubt it would have been permanently successful had it not been for the other complicating factors.

*Case II.*—The patient was a man, aged 42, on whom I had operated for gall-stones in August, 1921. He was big and muscular, and the operation of removal of the gall-bladder, through an incision in the rectus abdominis, was exceptionally difficult and was complicated by bleeding troublesome to control. Contrary to expectation—as what was supposed to have been the cystic duct had been ligatured—there was a free escape of bile during convalescence, but when the patient was discharged from hospital three weeks later the wound had healed. After his return home bile again escaped from the wound. This escape ceased at the end of a month but was immediately followed by jaundice and severe pain. Four months after the primary operation the patient was readmitted to hospital with deep jaundice and intense skin irritation, but without pain. After a few days of preparation the abdomen was re-opened. Some adhesions between the omentum, stomach and liver obscured the region of the common duct: these were easily separated, but it was very difficult to identify the common duct with certainty. Eventually the lower end was found and opened just as it was disappearing behind the duodenum. A probe passed easily into the bowel but was arrested in the opposite direction. The upper end of the divided duct was extremely difficult to find. By puncturing with a hypodermic needle it was ultimately identified, in the midst of a mass of dense scar tissue, in the situation of the hepatic duct, level with the liver and widely separated from the lower end. During this part of the dissection the cystic artery was torn and had to be caught in a clip which was left *in situ* at the completion of the operation as the artery could not be safely ligatured. On opening the proximal end of the duct a large quantity of "white bile" escaped. Once identified, the ends were fairly easily approximated over a rubber tube (No. 12 catheter) after the method of Sullivan. A gauze pack had to be left in the neighbourhood on account of oozing. The patient stood the operation quite well but death occurred, forty-eight hours later, from hæmorrhage. At the necropsy a large quantity of blood was found free in the peritoneal cavity and the cellular tissue about the hepatic omentum; the duodenum and the head of the pancreas were diffusely infiltrated with recent hæmorrhage. The liver was intensely bile-logged. The junction of the duct was satisfactory but the tube had been passed too far upwards and lay in the left hepatic duct in the centre of the liver.

It was obviously a mistake to attempt the anastomosis in the presence of such deep jaundice. The main indication was to open and drain the hepatic duct, the question of its repair being left for a subsequent operation.

*Case III.*—The patient was a short, stout female, aged 41, who had undergone an operation for recurrent gall-stones, in May, 1921, when cholecystectomy and gastro-enterostomy had been performed. A bile fistula had persisted for ten weeks and after it closed the patient had never been quite comfortable. She became thin, her skin developed a pale lemon colour, and she passed light clay-coloured stools. Occasionally she had pain with pyrexia and an increase of the lemon colour amounting to definite jaundice. At these times the stools were lighter than at others. Nevertheless in the intervals she was fairly well, could take food and was able to do some housework and to enjoy a holiday. As time passed the diagnosis of an increasing stricture of the common duct gradually became irresistible and a further operation was carried out in April, 1923. As in Case II, the common duct was difficult to identify but was eventually found and opened below the site of obstruction. The lumen at the latter point was so narrow that it would only admit a fine probe but, curious to relate, the duct on the proximal side was not appreciably dilated. The stricture was divided and the duct reconstructed over a rubber tube as in Sullivan's method. Bile was discharged from the wound for a fortnight but to my disappointment, after this discharge ceased, the bile did not at once find its way into the intestine in any considerable quantity. Three months from the date of operation, however, the patient's condition greatly improved and the fæces were normal in appearance and colour though the tube had not made its appearance. The most remarkable feature in this case was that the patient had maintained fair health for two years in spite of the extreme stenosis of the duct demonstrated at the last operation.

*Case IV.*—The patient, a male, age 36, was admitted to hospital in February, 1922, for an immediate abdominal operation. The gall-bladder was enormously distended, much inflamed, and full of stones, and there was extensive right-sided peritonitis. The gall-bladder was cleared of stones as far as possible, but the patient was not fit for any prolonged manipulation and the operation was completed by drainage. Though recovery followed, this man almost immediately began to have attacks of gall-stone colic with slight jaundice. In May, 1922, the abdomen was re-opened. The gall-bladder was adherent to the parietes over a considerable area; it was still large but not distended, though the walls were thickened owing to the comparatively recent inflammation. The region of the neck was much obscured by adhesions of the infundibulum to the duodenum and the common duct. The patient was a big, muscular man, and the parts were difficult to expose, and this difficulty was increased by the fact that he remained rigid throughout the operation. Cholecystectomy was carried out, the duct being first isolated and divided. It was recognized that a small portion had been excised from the wall of the common duct, and this was not then, but subsequently, confirmed by an examination of the specimen. The opening in the duct was isolated, and was used for the removal of several stones from its lower end. A suture was used to bring the edges of the opening into apposition, and when the loin support was removed the parts fell so well together that I had very little anxiety about the after result. Drainage was provided, and there was free discharge of bile for three weeks. At the end of this period the discharge ceased, bile was found in the fæces, and the patient felt and looked well, and was able to leave hospital just four weeks after operation, apparently cured. Shortly after returning home he

felt ill, shivered and became slightly jaundiced ; the wound re-opened and discharged bile, but in spite of this the jaundice persisted. He was readmitted, ten weeks after the last operation, on account of the biliary fistula and because the bile had disappeared from the faeces. After three days' preparation the abdomen was again opened. There were dense adhesions between the under surface of the liver, and the situation of the bile-ducts and the sinus had to be traced through these to the upper end of the common duct. This had become obliterated at the site of the injury, and all the bile was finding its way through the fistula. In this case the lower end of the duct could not be identified, and the upper part was therefore anastomosed to the duodenum, which had to be mobilized to allow the union to be made without tension. The crescentic opening in the wall of the bowel was at least half an inch in length, and mucous membrane was united to mucous membrane. A small rubber drain was brought to the surface from the neighbourhood of the union. Two days after the operation there was distinct jaundice, but this soon passed off, and in three weeks the patient was very well, the wound was healed, and the faeces contained a normal amount of bile. Two months afterwards he had a slight attack of pain with transient jaundice. These attacks were repeated, and five months after operation he had a severe one accompanied by a rigor. This unfortunate man was accordingly readmitted in January, 1923. He was then slightly jaundiced after a recent attack. Because of the intermittent nature of the seizures and the comparatively good condition between them, the question of the possibility of further stones in the common duct was raised, and yet another operation was performed (February 3, 1923). The region of the duct was exposed with difficulty. No stones were felt, and the duct was not sufficiently dilated to with certainty be identified or to be incised. The duodenum was therefore opened, and, after great difficulty, the new opening of the common bile-duct was found. It was very small, only admitting a fine probe, this was followed by bougies, until the opening easily admitted a No. 10 catheter, a portion of which was left *in situ* and fixed by a stitch. An artery was divided, and could only be secured by clips left *in situ*. During convalescence there was slight bile leakage, raised temperature and a tinge of jaundice. Early in June he was very well, but there had been a slight shiver. There was no news of the tube.

In the most modern view medicine in its highest aspect is a preventive science, and such has always been and must always be the case with surgery. It has therefore a direct bearing on the surgery of the common duct to emphasize the need of great care in the performance of cholecystectomy. The cystic duct should never be divided until both the hepatic duct and the common duct are seen, and *during* every such operation the removed gall-bladder should be carefully examined, so that if there is any suspicion of the main duct having been injured the site can be exposed and the damage repaired while the lesion is fresh. Though there may be fortuitous recovery from some such injuries with restoration of function, my cases show that the tendency is towards obliteration or stricture. For some time past surgical literature has been filled with accounts of operations for restoration or reconstruction of the bile-ducts, a fact which suggests that these surgical injuries are more frequent than the published cases would lead one to suppose. It is only because I have been able to furnish some part of the sequel of my cases that I have ventured to bring them before you, as it is only by watching these cases over as long periods as possible that we can discover the best

methods of dealing with them. Injuries due to non-surgical violence must be treated on the same principles, if for any reason the abdomen is opened. It has been claimed that lacerations of the duct may heal spontaneously—the effusion of bile into the peritoneal cavity either absorbing or yielding to drainage only—but the evidence on which this statement is based is very slender.

In dealing surgically with the larger bile-ducts, certain facts have to be borne in mind : (1) The ducts are elastic, and not only do they retract when divided but their ends actually contract, thus favouring the process of obliteration. (2) Except when fixed by inflammation they lie loosely among the tissues, which form a natural sheath and protection for them. Blood and extravasated bile tend to be confined or conducted by these natural sheaths, and the confinement aids the obliterative process. (3) The mucous membrane of the ducts has great recuperative power, and tends to grow out along the duct, and is very helpful in bridging a gap or making up a deficiency. (4) Ducts do not atrophy appreciably, or become obliterated, as a result of disuse ; these are processes which depend on inflammation.

In the light of these considerations it is clear that the most important point in the repair of ducts is to secure approximation—but not necessarily exact apposition—of the ends. Too many sutures and too much nicety in suturing are very apt to cause narrowing. The suture line should be the widest part of the duct, and in order to secure this it may be necessary to slit up one end of the duct slightly, especially if there is any disparity in size. Whenever possible, mucous membrane should be brought in contact with mucous membrane, even if that can only be done at one part of the anastomosis. It is better to avoid the contact of any foreign body with the suture line, and drainage should, if possible, be made from the proximal side of the union. When the ends cannot be approximated, some type of anastomosis or reconstruction will be necessary.

There are other points about operations in cases of injury to the ducts which may be usefully borne in mind. The separation of the ends is often exaggerated by the additional aids to exposure which have to be employed, and alarming or undesirable tension often disappears when the loin support is removed and the rotated liver is allowed to slip back into position. The identification of the duct in the midst of a mass of dense scar-tissue is often very difficult. Great help may be got from puncture with a hypodermic needle but it must be remembered that in cases of long-standing obstruction the bile is often clear.

These operations are difficult and tedious and their immediate success is surprising ; probably this is partly due to the fact that the peritoneum is immunized to traumatism and to infection. In operations on the bile-ducts in general all the aids to exposure must be employed. Any type of vertical incision through the rectus abdominis will give a sufficient exposure, provided it is carried right up to the epigastric angle. This upward extension is of far more value than twice the amount added to the downward length of the cut. The oblique incision of Kocher, carried from the middle of the costal angle right through the rectus muscle, gives a very good exposure and I am glad to see that Mr. Ralph Coyte, of the Surgical Professorial Unit at St. Bartholomew's Hospital, has confirmed observations made about the absence of nerve trunks in the upper four inches of the rectus sheath. In my experience this incision gives a good sound scar. The use of the Robson position has become well established, but the reversed Trendelenburg posture

does not seem so well known though it has proved very helpful. My experience has taught me that when there have been symptoms pointing to an obstruction or infection of the common duct the latter should always be opened. Small stones, soft stones and detritus may very readily be missed if dependence is placed on external palpation alone. With patience, care and gentleness, the supraduodenal portion of the duct can be opened in the great majority of cases, and in 119 operations I have only once had to employ the retroduodenal operation and only three times the transduodenal route. Gentle downward stripping of the duodenum is the greatest aid, but the proper use of suitable forceps also helps. Almost since the inception of common duct surgery the finger has been regarded as the only reliable probe, but surgical progress has led surgeons to operate so much earlier than formerly that in many cases the duct is not sufficiently dilated to admit the finger.

For these cases the Desjardin forceps are at once the best probe and the readiest means of removing smaller stones, &c. I appreciate what Sir Berkeley Moynihan said about overlooking small stones or fragments; I learned at his clinic, years ago, never to conclude an operation on the duct without passing a sound through the duct into the duodenum; I prefer the female bladder sound with a diameter of  $\frac{3}{16}$  in. Of course this is not done with the object of forcing any fragment into the bowel but because any stone or fragment likely to be missed after a careful search will not be of larger size than will pass through an ampulla which has been thus dilated. The single attack of colic which not infrequently follows the re-establishment of the normal bile channel is probably evidence of the safe negotiation of the duct by some such fragment. I was interested in the remarks made by Dr. Mayo about suture of the duct after the removal of small stones or in quiescent cases, as my own comparatively limited experience would lead me to say, "when in doubt, *drain*." I may just mention a rare complication which has a bearing on this question of drainage. I opened a normal-looking common duct and removed a small stone. The opening in the duct was not sutured but a tube was brought from the vicinity though I was not satisfied that it was well placed. The patient was never quite well after the operation and four weeks later I re-opened the wound and evacuated several pints of bile-stained fluid from the lesser sac. Recovery eventually followed. Since then, when the tube is not passed into the duct, I have taken care to split the end so that it lies over the incision in the duct, and, if necessary, I fix it to the margin of the duct with an unknotted suture. The greatest improvements, however, that I have witnessed in the surgery of the ducts have not been in matters of technique but in the choice of the time at which to operate, the preparation of the patient and the after-treatment. Recent great advances along these lines and the results of these advances have been mentioned by Dr. Mayo. In dealing with very toxic and deeply jaundiced cases I am sure that a two-stage operation will sometimes save life. Though it will, perhaps, be less frequently called for in the future than in the past it may still have a useful place as a preliminary to difficult operations on the ducts.

I have recently reviewed all my operation figures for gall-stones. I find I have operated on 502 cases with thirty-one deaths—a mortality of just over 6 per cent. Of these, 119 were common duct cases with thirteen deaths—a 10.9 per cent. mortality. In reviewing this latter group I have been surprised at the number of cases in which there has been coincident pancreatitis. All the deaths have either been sudden and attributable to toxæmia or due to late hæmorrhage or to pancreatitis. At the present time the number of common

duct cases coming for operation does not bear the same relationship to the whole as formerly. Earlier recognition of the presence of gall-stones—in this country largely due to the propaganda work of Sir Berkeley Moynihan—and the more general acceptance of the need of operation while the calculi are still limited to the gall-bladder, will continue to diminish this number. I have for long taught that jaundice ought to be looked upon as a complication of, rather than a symptom due to, gall-stones, and that calculi in the common duct, with infection, really represents a systemic and not a local disease. This, I think, is a useful conception for the practitioner who has to deal with these cases in the first instance.

I will now just refer to one or two rarer conditions represented in the wealth of specimens collected for us this evening. One is the congenital cystic dilatation of the bile-duct. The specimen I show is from a woman, aged 40, the mother of three children. She had enjoyed good health until five months before the operation. She then suffered from pain, vomiting and jaundice followed by the development of a localized swelling below the right costal margin. On operation this swelling proved to be a large cystic dilatation of the common duct containing two pints of altered bile. Drainage was carried out but the patient died of uncontrollable hæmorrhage four days later. The specimen shows a kind of valvular obstruction in the duct and I assume that the pathology is akin to that type of hydronephrosis which remains for a long period limited to the pelvis, and which follows some similar obstruction in the ureter. Most surgeons must be familiar with cases in which the common duct is as large as the small intestine and in which the other large ducts are similarly affected. In these patients there may be many calculi, but the symptoms have often been slight—very slight in view of the gross pathological conditions found. I would suggest that the dilatation is probably congenital and allied to the enormous dilatation of ureters—or colon, or œsophagus—which are sometimes found in infants and in which no obstruction can be demonstrated. The last condition I wish to mention is bile leakage from the dilated—but apparently intact—duct. I first came across it in association with pancreatitis but have since seen it without that complication.

The large ducts still deserve the attention of the anatomist; the keen young surgeon-anatomist is likely to make the most of the investigation. We are all grateful to Flint, of the Leeds school, for his admirable work on the so-called anomalies. I suggest that casts should be made of the common duct distended with gelatine, or by some other means, in order to see if, in any proportion of cases, lateral pouches—or even diverticula—which surgeons constantly imagine as hiding places of what Dr. Mayo has called “left-overs,” can really be demonstrated.

Mr. A. J. WALTON, M.S.

### Reconstruction of the Ducts.

I PROPOSE to limit my remarks to that difficult group of cases in which some form of reconstruction of the common bile-duct is necessary. Although relatively rare, these cases—now that so many operations are being performed in this region—are becoming sufficiently common to fall to the lot of most surgeons, and, since the conditions necessitating this step are often met with unexpectedly and may be the result of some accident, it is well that a definite

line of treatment should be accepted and practised. There are several conditions for which such an operation may be required:—

(1) *Accidental Injury to the Common Bile-duct.*—Pathological lesions of the gall-bladder are so frequent, and so commonly lead to changes in the normal anatomy, that injury of the common bile-duct is becoming more frequent also. This is more especially the case because congenital abnormalities in this region are common and because the operation of cholecystectomy, which is now more widely advocated, necessitates careful dissection in the region of the cystic duct. Many papers have been written recently on this subject and it is now realized that there are several factors predisposing to such an injury. The variations in the origin and course of the cystic artery and in the length and position of the cystic duct constitute one of the chief causes of error. If the cystic artery arises, as it frequently does, from the hepatic main trunk, it may pass in front of or behind the common bile-duct and lie superficial instead of deep to the cystic duct. If care be not taken in the dissection of the cystic duct, the artery is unexpectedly divided and, retracting beneath the free edge of the gastro-hepatic omentum, may bleed furiously and obscure the normal parts. An artery forceps when being passed into the depth of the wound may then (as shown in fig. 1) pick up the common bile-duct with the artery and completely crush its walls. In such a case the injury may be noticed immediately, or the duct may be tied and may not give way until several days after the operation is completed.

The position of the cystic duct is prone to considerable variation. If it runs parallel with, and closely adherent to, the common bile-duct, the common duct may be pulled up with and mistaken for a portion of the cystic duct as seen in fig. 2, and the two structures divided as indicated by the dotted line. At the termination of the operation, the opening to the common bile-duct may be seen and the accident remedied. If it be overlooked bile will leak into the peritoneal cavity and peritonitis result, in which case a reparative operation will be much more difficult.

In the days when it was more usual to commence a cholecystectomy at the fundus of the gall-bladder, the common bile-duct might be pulled up in a loop by the tension upon the cystic duct as indicated in fig. 3, in which case the line of section would pass through the adjacent parts of the common duct and completely resect a portion of it. Here again the accident might be noticed at the time or might be overlooked until symptoms of obstruction or leakage occurred.

As the result of many inflammatory adhesions, Hartmann's pouch of the gall-bladder may be drawn down and firmly attached to the side of the common bile-duct. An incomplete dissection will reveal the common duct apparently occupying the position of the cystic duct, and it may then be cut right across (see fig. 4). The only way to avoid these accidents is to make a definite rule that in performing a cholecystectomy an incision is first made in the upper part of the gastro-hepatic omentum and that nothing whatever is divided until all three ducts, namely, the common hepatic, the cystic and the common bile-ducts, are exposed to view (fig. 5). It is remarkable how great is the temptation, even after much experience, to divide the cystic duct as soon as it is exposed, but by carefully observing this rule I have, on several occasions, been saved from cutting the common duct which I had felt certain was the cystic duct. It is an equally important rule that the cystic artery, or any other vessel which may appear to be abnormal, should be ligatured before it is divided, so that any possibility of its retracting beneath the common bile-duct is avoided.



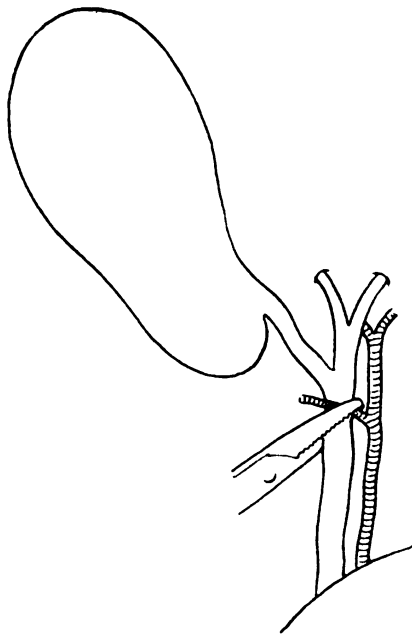


FIG. 1.

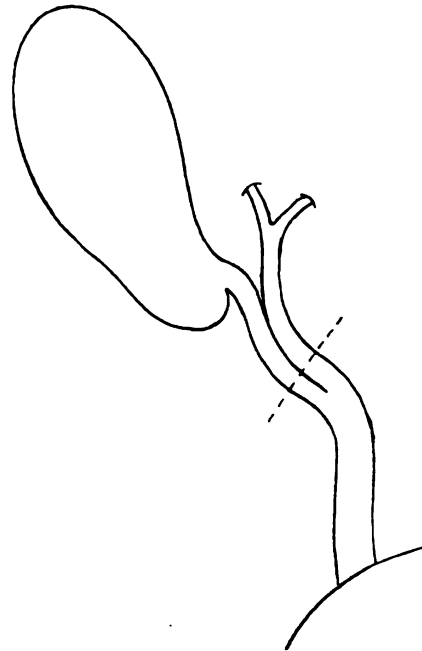


FIG. 2.

FIG. 1.—Injury of common bile-duct in clamping abnormal cystic artery.

FIG. 2.—Division of common bile-duct with an adherent cystic duct.

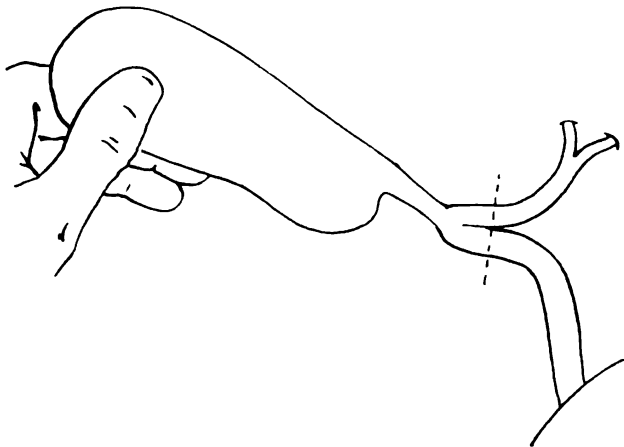


FIG. 3.

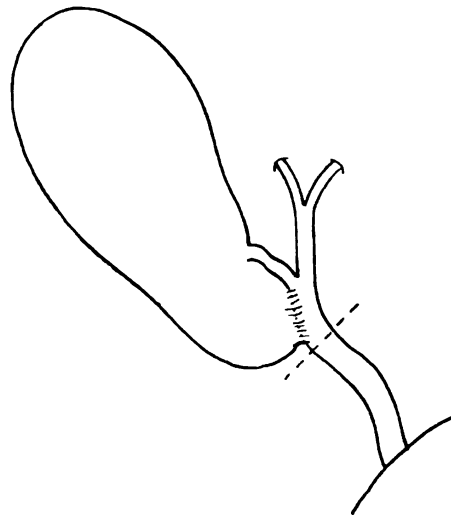


FIG. 4.

FIG. 3.—Tension upon gall-bladder causing a loop of the common bile-duct which is divided in mistake for the cystic duct.

FIG. 4.—Common bile-duct adherent to Hartmann's pouch and mistaken for cystic duct.

(2) *Congenital Absence of the Cystic Duct.*—Complete congenital absence of the cystic duct (fig. 6) is a very rare condition, although not infrequently this duct may appear to be absent owing to inflammatory adhesions between the gall-bladder and the common hepatic and common bile-ducts. I have reported a case in which the duct was congenitally absent, and under such conditions cholecystectomy will of necessity be associated with a removal of a portion of the common duct. This complication should, however, be recognized if care is used in dissecting the structures before removing the gall-bladder.

(3) *Early Pancreatic Obstruction.*—There is a group of cases in which upon operation the gall-bladder is found to be the seat of chronic inflammatory changes or even to contain gall-stones. The common duct is not dilated but there may be some difficulty in passing a probe to the duodenum. In some cases, indeed, the probe will not pass at all. Danger arises from the fact that

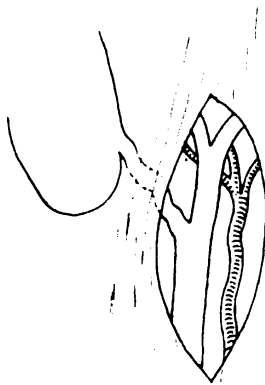


FIG. 5.

FIG. 5.—Method of exposing all three ducts before division.

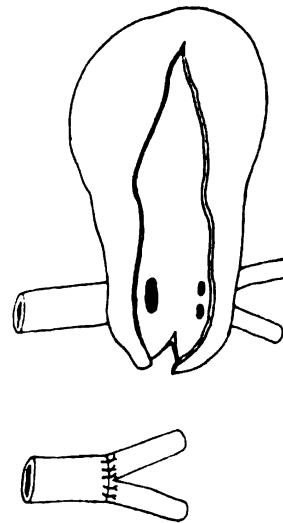


FIG. 6.

FIG. 6.—Congenital absence of the cystic duct.

the operator may fail to recognize that there is a slight obstruction lower down. He may remove the gall-bladder and close the common bile-duct, and, later, the line of suture may give way and a permanent biliary fistula may result. At a second operation it will be found that the lower portion of the duct is collapsed and atrophied or may be so fibrous that it cannot be discovered, and some operation will then be necessary to unite the patent upper end to the duodenum.

(4) *Advanced Cases of Chronic Pancreatitis and Carcinoma of the Head of the Pancreas.*—As a general rule these cases are associated with profound jaundice, a dilated gall-bladder containing thick green bile, and a dilated common duct. The pancreas feels hard, enlarged and nodular. In certain cases, however, long-continued obstruction is associated with absorption of the bile pigment, so that the bladder and passages are filled with a mucoid-like fluid. My own experience has differed from that of Judd and Lyons in the

fact that on several occasions I have found that this white bile was present even when the gall-bladder freely communicated with the common duct, and, apart from the dilatation, appeared to be normal. The danger in these cases lies in the fact that if the gall-bladder is aspirated and clear fluid withdrawn the condition is likely to be mistaken for an obstruction of the cystic duct and the common duct may be opened in order to decide the diagnosis. If such a possibility be kept in mind a mistake of this sort is not likely to be made. If the common duct has been opened it is probable that the opening, even when sutured and when the pressure is relieved by a cholecystenterostomy, might not hold, and a permanent biliary fistula would result. In such cases—as in the last—an operation to cure the fistula would show that the lower end of the duct had become atrophic and fibrous and the proximal opening alone would be left.

(5) *Combined Disease of the Gall-bladder and Common Duct.*—Carcinoma of the common duct may sometimes be associated with carcinoma of the gall-bladder, or with calculi; or a chronic pancreatitis may be present and may give rise to obstruction while the gall-bladder is the seat of an acute cholecystitis. In other cases there may be a small localized growth situated at the junction of all three ducts, or yet again there may be a growth of the common hepatic duct well above the junction of the cystic duct. Carcinoma in these positions is not uncommon. In my own series of 262 cases of gall-stones there were seven examples of carcinoma of the gall-bladder, ten of the common duct alone, and five of carcinoma of both common duct and gall-bladder. It is important to remember that the growth may be associated with stones, otherwise it may be overlooked when the stones are found. In my own cases stones were associated with carcinoma of the common duct in 55.5 per cent. as compared with 85.7 per cent. in which they were associated with carcinoma of the gall-bladder. Carcinoma of the ducts may occur at the ampulla, in which case it is most likely to form a fungating mass which may reach the size of an orange and project into the lumen of the duodenum or widely infiltrate its walls. In the higher portions of the duct it commonly appears as a small, hard, fibrous growth, which may be limited to the duct below the junction of the cystic duct, in which case the gall-bladder is dilated and the condition can be relieved by cholecystenterostomy; or it may be situated at the junction of all three ducts, in which case also the gall-bladder is dilated, but only with mucus, and the condition will not be relieved by cholecystenterostomy; or, again, it may be limited to the hepatic ducts, and in this case the gall-bladder will be collapsed. If small, and of this nature, the growth may be difficult to distinguish from an inflammatory mass, so that in all doubtful cases a resection of the duct should be performed if possible. Owing to the fact that the growth is often small and localized this is frequently feasible, but unfortunately the results of operation for such conditions are unsatisfactory, and the growth commonly recurs. When the gall-bladder is diseased or collapsed it will be impossible to utilize it in performing a cholecystenterostomy, and the only operation which is feasible, to afford relief, is that of anastomosing the duct directly with the duodenum.

(6) *Obstruction of the Duct by a Scar.*—It is frequently stated that the routine performance of a cholecystectomy is objectionable, because if the duct were obstructed after the removal of the gall-bladder, it would be impossible to perform a cholecystenterostomy and thus afford relief. This danger is more imaginary than real, for obstruction under such conditions is extremely rare. Very occasionally a stone may become impacted at the junction of all

three ducts, and by ulceration may lead to a fibrous stenosis. In such cases a cholecystenterostomy would afford no relief.

#### OPERATIVE TECHNIQUE.

The operations which have been from time to time performed in these varying groups of conditions are very numerous, and there is no one operation which is the most suitable for all cases, since, manifestly, the technique to be employed will differ considerably, according as to whether the duct is dilated or leaking, and as to whether the distal end can be isolated or not. The operations of choice may be considered as follows:—

(1) *Direct Suture*.—The wider recognition of the fact that the common bile-duct can be easily injured at operation has led to a more careful investigation and frequent recognition of the lesion at the time the duct is injured. Under such circumstances, and sometimes also after a primary growth of the duct has been excised, approximation of the two ends can be readily secured, and an end-to-end suture is then the operation which is usually performed. Theoretically this appears to be the operation of choice, but it is often associated with considerable difficulties. If the duct has been injured the two ends are relatively small, and if a growth has been removed the upper end may be dilated and be of much greater calibre than that below. For these reasons the operation is sometimes performed by suture around a tube, but this method has the drawback that the removal of the tube is associated with considerable difficulty, and is often followed by fistula formation. Even when no tube is used the operation does not appear to be ideal. Leakage is not uncommon and stenosis is likely to occur. In the only case in which I have carried out this operative technique—that is the one in which the cystic duct was congenitally absent—the patient has continued to have recurrent attacks of pain associated with a temporary jaundice. These attacks occur at relatively long intervals, and only last for a short period, and hence no further operation has yet been performed, but it is possible that the stenosis may increase and interference be required at a later date.

(2) *Lateral Choledochenterostomy*.—In those cases in which the obstruction is low down and the duct is considerably dilated, approximation of the side of the distended duct with the duodenum is quite feasible. This is the operation specially indicated in the rare conditions of congenital cystic dilatation of the common bile-duct, for here the lumen of the duct may be enormous, the cyst in some cases containing several pints of fluid. In the varieties of obstructive dilatation such as are met with in chronic pancreatitis or carcinoma of the head of the pancreas, the operation will only be performed after the gall-bladder has been previously removed or is so far diseased that it is no longer available for a cholecystenterostomy. Even under these circumstances my own experience of it has not been entirely satisfactory. In order to perform the anastomosis it is necessary to cause a considerable amount of angulation of the duct, so that there is considerable stress upon the line of junction. Moreover, the opening is freely patent, and thus there is danger of the duodenal contents passing up into the common duct and leading to an infection of this structure.

(3) *Re-formation of the Common Duct*.—It will be manifest that in a large number of these cases there is a very considerable gap between the lower and upper portion of the duct. This gap will be present whenever resection has been carried out for the removal of a localized growth of the duct or of a fibrous stricture. Another large group consists of those cases in which an

injury has been overlooked at the first operation, or where an obstruction below has been associated with a fistula. It frequently happens that in this group not only are the structures much obscured by inflammatory changes but the lower end of the duct, not having had bile flowing through it, is so small and shrunken that it cannot be found. It is thus necessary to bridge the big gap between the upper end of the duct and the duodenum, and in order to do this a new path must be made along which the bile can enter the intestine. Of the many methods which have been used for this purpose only a few have survived. Attempts are still occasionally made to form a new duct from an autogenous graft taken from the fascia lata or some other convenient structure. These operations are, however, associated with considerable technical difficulty and a portion of the graft is very likely to fail. To-day practically every case can be treated by the application of the methods of (a) direct or (b) indirect implantation into the duodenum.

(a) *Direct Implantation into the Duodenum.*—Wherever possible this is unquestionably the operation of choice. It was first performed by Dr. W. J. Mayo; since then several cases have been reported either for the repair of an accidental wound or after resection of a neoplasm. The proximal end of the duct is brought down and anastomosed to an opening in the wall of the duodenum. This opening must be so devised that the entrance is valvular. This has been accomplished by several methods: in Mayo's operation a somewhat circular incision is made which gives rise to a flap-like formation, while in others the wall of the duodenum has been folded over the duct so that the latter runs an oblique course before opening into the viscus. Where the duodenum is mobile—that is to say in those cases in which the reconstruction is done as a primary operation—this method will probably be found suitable in most instances. If, however, there has been a long-continued fistula, with many inflammatory changes, it will often be found that the duct can at best be brought into apposition with the upper surface of the duodenum, and there is insufficient material to allow of the formation of a valvular opening, which seems to be essential, otherwise there would be a grave danger of infection passing from the duodenum and leading to a suppurative cholangitis. In certain cases this difficulty has been overcome by making the anastomosis to a loop of small intestine which has been drawn up through the mesocolon. This method, however, has the drawback that the loop of intestine has to be drawn far up and will exercise a considerable drag upon the anastomosis.

(b) *Indirect Implantation.*—The original method employed to form a new bile-duct was that of Sullivan. A tube was inserted into the common duct at one end, and into the duodenum at the other end, and was then wrapped round the omentum in the hope that the fistulous tract would be formed around the tube and that the tube would be passed. Several such operations have been reported in the literature, but, as might be expected, they have not been entirely satisfactory. The tube, lying in a mass of inert tissue such as the omentum, may fail to pass into the intestine, and so, acting as a foreign body, may cause considerable inflammatory change, or lead to the formation of calculous deposit. Should the tube be passed, there will be a tendency for the fatty tissue of the omentum to become fibrous and constrict, so that ultimately there is obstruction again, together with a septic cholangitis. These drawbacks and difficulties can be overcome by forming a new duct out of a flap of the duodenum, and this method may be utilized either when the duct is completely divided, or for the formation of a new lateral duct when the original passage is simply dilated above an obstruction.

When the division is complete, the upper opening of the common duct—or, if the division has been very high, the openings of the two hepatic ducts—are freed from the surrounding inflammatory tissue. A catgut suture is passed

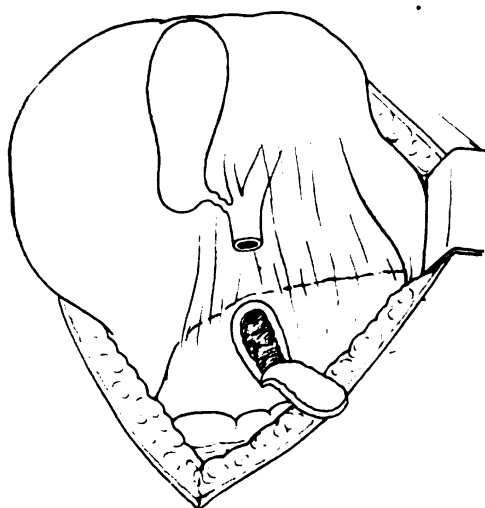


FIG. 7.

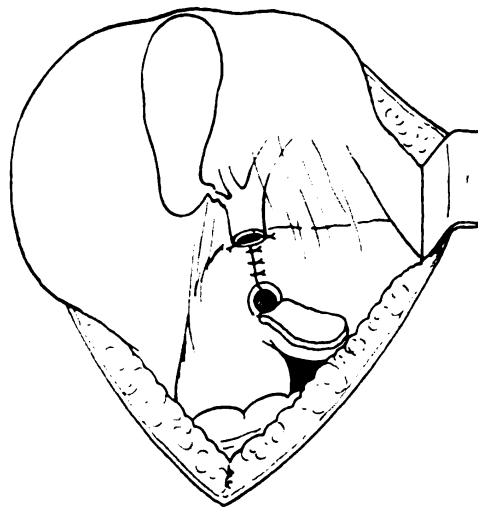


FIG. 8.

FIG. 7.—Formation of flap in anterior duodenal wall.

FIG. 8.—Suture of cut duct to upper border of duodenum and partial closure of duodenal opening.

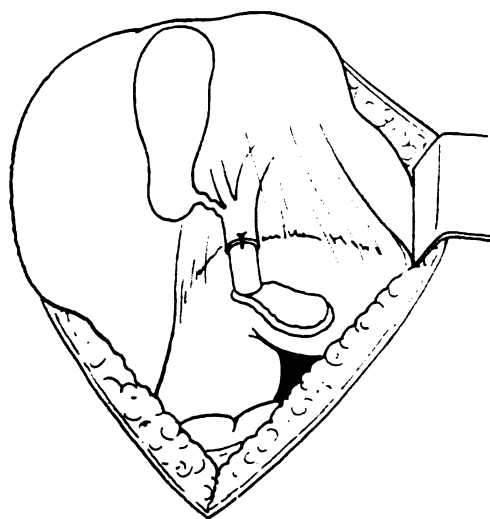


FIG. 9.

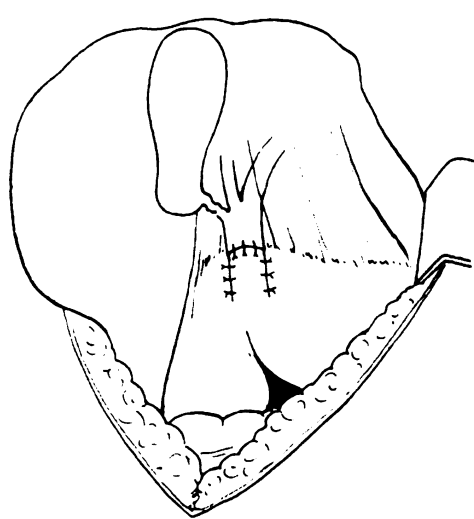


FIG. 10.

FIG. 9.—Tube sutured in divided duct and passed through duodenal opening.

FIG. 10.—Duodenal flap sutured around tube.

through the upper border of the duodenum and the posterior wall of the divided duct and tied, the two structures being thereby drawn into apposition. The union of the posterior wall of the duct and the upper border of the

duodenum is completed with a few small stitches. A flap is then cut from the anterior surface of the duodenum and is turned downwards (fig. 7). The upper part of the resulting opening is sutured until it is approximately the same size as that of the common bile-duct (fig. 8). A small rubber tube is inserted into the stump of the duct and sutured in place with plain catgut, and the lower end of the same tube is inserted into the opening of the duodenum (fig. 9). The flap is brought up over the tube and sutured to the duct and anterior wall of the duodenum which lies behind the tube (fig. 10). A drainage tube is inserted down to the junction in case there should be any leakage. This operation in practice is quite simple to perform and is practically applicable to any case in which a large portion of the common duct is absent. The duodenum can almost always be brought into good apposition with the cut end of the duct. The new portion of duct is lined with duodenal mucous membrane which is accustomed to the presence of bile and therefore will not slough. A long valvular opening is formed so that there is little chance of an infection spreading upwards. The small piece of tube, being sutured in place merely with plain catgut, is usually passed about ten days after the operation and gives rise to no further trouble.

When the duct is not divided but is dilated from an obstruction below, such as a carcinoma of the head of the pancreas or chronic pancreatitis, a slight modification of this operation is easily applicable. In this case a lateral opening is made into the duct and the lower edges of this opening are sutured to the upper border of the duodenum. A flap is made in the anterior wall of the duodenum and the operation carried out on similar lines to those just described, the tube in this case being inserted into the lateral opening of the duct instead of into the end of the divided duct.

My own cases, the majority of which have already been reported elsewhere, are as follows:—

|                                              | Total    | Reconstruction,<br>terminal | Reconstruction,<br>lateral | End-to-end<br>suture | Recovered | Died    |
|----------------------------------------------|----------|-----------------------------|----------------------------|----------------------|-----------|---------|
| Injuries ... ..                              | 3        | —                           | —                          | 2                    | 2         | 1       |
| Stricture ... ..                             | 1        | 1                           | —                          | —                    | 1         | —       |
| Chronic pancreatitis ...                     | 4        | 1                           | 3                          | —                    | 2         | 2       |
| Carcinoma of duct or<br>head of pancreas ... | 5        | 2                           | 3                          | —                    | 2         | 3       |
|                                              | <hr/> 13 | <hr/> 4                     | <hr/> 6                    | <hr/> 2              | <hr/> 7   | <hr/> 6 |

## Section of Surgery.

### SUB-SECTION OF PROCTOLOGY.

President—Mr. ASLETT BALDWIN, F.R.C.S.

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### PRESIDENT'S ADDRESS.

#### The Causes of Intestinal Disease.

By ASLETT BALDWIN, F.R.C.S.

LAST year nearly 50 per cent. of recruits for the army were rejected for medical unfitness, and during the war it was common knowledge that an appalling number of men were placed in class C3, which meant that they were of very little use to the fighting forces of the country.

It has been estimated that 25 per cent. of the patients attending the hospitals out-patients department apply for relief of gastro-intestinal disorders. Both in this country, on the Continent and in America there are special facilities provided for intestinal irrigations, and other methods of treatment for the relief of colitis with very indifferent success, many patients travelling year after year to various spas, and undergoing treatment at home with only temporary relief. One is constantly seeing patients in a state of chronic ill-health owing to chronic intestinal poisoning, due to partial degeneration of the intestine, which leads to delayed and incomplete evacuation. In many cases chronic intestinal stasis is the result; this Sir Arbuthnot Lane believes "is the cause of all the diseases which are acquired as the result of civilization." I believe he is very near the truth, if diseases due to industrial causes are omitted.

Is it not then our duty to try to elucidate the causes of this condition which according to so high an authority leads to such disastrous results?

If we inquire into the health of primitive peoples we find that this trouble is non-existent, their physique in many parts of the world is magnificent, they show no signs of the deterioration found in civilized countries, and intestinal disorders and the diseases following in their train are rare or absent. For example Colonel Robert McCarrison of the Indian Medical Service, informs us that in the State of Hunza, in Northern India, where he resided for seven years, the ordinary diseases of civilization are practically unknown, and the only attention they require from medical men is for the treatment of injuries and senile cataract. What then is the cause of the difference in their health as compared with that of civilized peoples?

Colonel McCarrison says, speaking of that country: "So limited is the land available for cultivation that they can keep little live stock other than goats, which browse on the hills, while the food supply is so restricted that the people, as a rule, do not keep dogs. They have in addition to grains—wheat, barley and maize—an abundant crop of apricots; these they dry in the sun and use very largely in their food. Amongst these people the span of life is extraordinarily long; and such service as I was able to render them during



some seven years spent in their midst was confined chiefly to the treatment of accidental lesions, the removal of senile cataract, plastic operations for granular eyelids, or the treatment of maladies wholly unconnected with food supply. Appendicitis, so common in Europe, was unknown. When the severe nature of the winter in that part of the Himalayas is considered, and the fact that their housing accommodation and conservancy arrangements are of the most primitive, it becomes obvious that the enforced restriction to the unsophisticated foodstuffs of nature is compatible with long life, continued vigour, and perfect physique." Here we have a people who are exposed to great hardships, due to a rigorous climate, poor housing accommodation, primitive or no sanitation, a limited food supply, but which they prepare for their consumption by methods common among primitive peoples, yet they appear to be immune from ordinary infections, have healthy and vigorous bodies, and are extraordinarily long lived.

If we look in the direction of certain parts of the world we find various peoples who were previously healthy, but who subsequently became victims to diseases formerly more or less unknown to them. In Labrador, Dr. Grenfell tells us, numerous villages were found where the inhabitants were suffering severely from scurvy, they were subsisting mainly on potatoes which had been peeled before cooking; when the potatoes were cooked in their jackets and merely skinned, the disease disappeared.

In Newfoundland and Labrador, where in mid-winter and spring many persons are obliged to live largely on bread made of fine wheat flour, beri-beri, the deficiency disease, frequently occurs, though when the bread was made from "brown" flour the disease was unknown. In 1910 a ship laden with whole wheat flour ran ashore and a considerable proportion of her cargo was removed in order to lighten her and, later, this flour was consumed by the adjacent population. There was no case of beri-beri in that region for a year afterwards. Again beri-beri was rare on Norwegian ships before 1894, its frequent occurrence afterwards coincided with an alteration of the diet in the Norwegian mercantile marine, made compulsory in that year in response to a popular demand for an "amelioration" in the conditions of life. Previously the sailors on long voyages used biscuits made from rye flour; subsequently the masters of ships were obliged to supply bread baked from white wheaten flour, or a mixture of wheat and rye. Now in the milling of rye flour there is no separation of the germ. Finally there are the recent experiences of our troops in Mesopotamia. In the early stage of the siege of Kut-el-Amara, Colonel Hehir states: "A recrudescence of beri-beri amongst British troops gave rise to some apprehension but it then disappeared." During the first two months the British troops received a cereal ration of white wheat flour; when they were attacked with beri-beri. In February, 1916, the supply of white flour ran short and they were supplied with the same flour as the Indian troops had, namely, barley flour and atta, the coarsely milled wheat usual in the Indian Sepoy's ration. It is very significant that beri-beri should have broken out among the British troops while upon their normal rations of white wheaten flour, and should have cleared up when they were obliged to share in the coarsely milled and doubtless, germ-containing grain of their Indian fellow soldiers. The latter were throughout the campaign saved from beri-beri, not only by an unspoilt cereal, but also by a generous daily ration of dhal, consisting of several dry pulses which are also valuable sources of anti-beri-beri vitamins.

In regard to the well known epidemic of beri-beri in the Japanese navy, in

his evidence before a committee in the House of Commons in 1919, Colonel Sir Arthur Mayo-Robson said: "The polishing of rice has led to an enormous number of deaths in China, India and Japan. I remember listening several years ago to a Japanese general, who was describing the terrible ravages that beri-beri had created in Japanese ships; it took many years before we found out what caused the disease. It was argued that it was due to some germ or to some infection, whereas all the while it was due to the absence of vitamins from polished rice. The use of polished rice in the Richmond Asylum, Dublin, a few years ago, resulted in an alarming outbreak of beri-beri there, and caused a number of deaths."

The symptoms and pathological changes found in pellagra closely simulate those found in animals fed on diets deficient in the accessory food factors. In the same way it is well known that scurvy is due to similar causes. It is also well established that the health of the people of this country, particularly of the poorer classes, since the introduction of the American methods of milling wheat flour which removes the germ, the aleurone cells, much of the phosphates and the bran, has greatly deteriorated. Many experiments have been made on birds and animals in this country, in America and in India by various investigators to find out the effects of feeding birds and animals on foods deficient in the various vitamins. I shall quote now from those experiments made by Colonel McCarrison and his remarks thereon.

#### PIGEONS FED ON AUTOCLAVED RICE.

Diarrhoea is one of the earliest, as it is one of the most frequent manifestations of deficiency disease in pigeons. Its occurrence is evidence of the distaste of the gastro-intestinal tract for the deficient food presented to it. The excess of starch in the dietary interferes with the assimilation of protein, and the diarrhoea further increases the deficiency owing to the rapid transit of food through the tract; microbic fermentation of the ill-digested starch results in excessive production of mineral and organic acids, which, on absorption, may presumably lead to a condition of acidosis. It has been noted that in cases treated with vitaminic extracts the diarrhoea has ceased with a rapidity equalled only by that with which the acute nervous symptoms disappear.

In applying these results to the human subject, it is to be remembered that the character and severity of the diarrhoea will depend upon the characters of the bacteria which happen to be present. The birds suffer from loss of appetite, the respirations become progressively deeper and slower, there is a fall in body-temperature indicating that oxidative processes are depressed when the organism is not receiving the normal amount of vitamins, there is a progressive loss in body weight, which occurs earlier when butter and onions are added to the diet, and the total loss is greater. It is worthy of notice also that, after the first seven to thirteen days the temperature is lower; the amount of heat given off is smaller in birds fed on polished rice, than those that are starved. This remarkable lowering of the temperature without a corresponding increase of the heat given off, indicates, according to Novaro, that the mechanism of heat production is effected by the deficiency of vitamins. The birds get progressively weaker and show little inclination for active movements. To these evidences of increasing weakness others—referable to changes in the central nervous system—are often suddenly added, and the final or neuritic stage of the disorder is then reached. The nervous symptoms are weakness of the legs, paresis of the legs or wings or both, paralysis, inco-ordination,

cerebellar symptoms, and convulsive seizures. To these may be added varying grades of loss of sensation to touch, pain, and heat, impairment of deglutition, and in some cases of vision. It is to these symptoms that the term "polyneuritis columbia" is applied; it corresponds with beri-beri in man. Monkeys fed on deficient dietaries presented in general the same clinical features as pigeons: loss of appetite, gastro-intestinal disturbances, progressive loss of weight, progressive anæmia and asthenia, and, in those surviving long enough, symptoms due to malnutrition of the nervous system. These are to be regarded as the cardinal effects produced by food deficiencies. The essential differences in the effects produced by various combinations of deficient diets, were the greater or less degree of rapidity with which symptoms manifested themselves, and the greater or lesser delay in the inevitable issue—death. In animals, the basis of whose food was autoclaved rice, symptoms appeared early, and death resulted rapidly; in those receiving autoclaved food, they appeared later, and death was longer delayed.

#### LOSS OF APPETITE.

Distaste for deficient food is the first symptom to appear. In animals fed on autoclaved rice loss of appetite was complete in ten to fourteen days, after which death resulted in a comparatively short time; the fatal issue was hastened by an insufficient intake of food and by the severe diarrhoea. Loss of appetite made its appearance later in animals fed on autoclaved food. The appetite may become depraved (some of the animals have actually been observed to eat their own fæces). Distaste and loathing of food, loss of appetite, and it may be also depraved appetite, are thus cardinal symptoms of deficiency disease, and their significance is great. They are due in part to the monotony of the food, but in the main to insufficient supply of vitamins, and of vitamin B in particular. Thus Osbourne and Mendel have found that if animals, fed on purified dietaries, free from this vitamin are given yeast separately, it increases their appetite for the deficient food, no doubt in consequence of its high content of vitamin B. The well-known effect of yeast in improving the appetite in human beings is probably due to the same cause. Drummond finds the addition of this vitamin to a synthetic diet, causes a greatly increased intake of food and consequently increased rate of growth. It would seem that the improved appetite is due to the improvement in the growth processes and general condition of the animal, consequent on the provision of this vitamin, an improvement which results in the production of gastric and other digestive juices, and in acceleration of the chemical reactions requisite for growth. The animals are impelled to eat more in order to satisfy the cells stimulated to growth by the vitamin. Vitamins are thus indirect stimulants of appetite, they induce the desire for food, and are, therefore, indirect stimulants of digestive juices. It seems to me that "loss of appetite" is one of the most fundamental signs of vitamin deprivation. It is a protective sign, the first danger signal of impending disaster. It should at once excite suspicion as to the quality of the food in any patient who may exhibit it, for food deficiency is to be found in the most unlikely subjects, and in cases of the most diverse symptomatology. Thus the distaste for boiled milk exhibited so often by sufferers from enteric fever, may indicate the necessity for supplementing the milk diet with vitamins. It is certain that invalid dietaries are frequently dangerously deficient in these indispensable substances. Persons receiving too little of these substances are often "finicky" eaters, nibbling at their food.

## DIARRHŒA.

The most important, as well as one of the most constant of the symptoms of food deficiency in monkeys is diarrhœa, either with or without mucus and blood in the stools. It is also one of the earliest, as well as one of the most constant of the symptoms of food deficiency in the human subjects as is evident from the literature of such deficiency diseases as "war œdema" and "pellagra." Its true significance is often not recognized, as many have not fully realized that the same fundamental cause which produces "war œdema" or "pellagra" also causes the diarrhœa or dysentery, namely, food deficiency. Of greater importance still is the fact that a well-balanced food containing sufficiency of vitamins will prevent the diarrhœa or dysentery, as certainly as it will prevent the more obvious symptoms of deficiency diseases. With the knowledge provided by these observations in monkeys, the colitis with mucus and blood, so often noted as the most common and troublesome complication of war œdema, assumes a new significance, as also does the observation that diarrhœa of this type rarely occurred among well-fed prisoners, although they were exposed to infection. Of twenty-five monkeys fed on deficient dietaries, twenty-one suffered either from diarrhœa or dysentery while the controls remained free from both, although confined in the same animal room, cared for by the same attendant, and subjected to the same sources of infection—the only difference made being in the food. Some of the monkeys which suffered from dysentery passed the *Entamœba histolytica* or their cysts, and among the controls several were found to be carriers, but these showed no symptoms of dysentery. It is obvious that a well balanced dietary secured the immunity of the controls. Dr. Drummond says that among rats fed on synthetic food deficient in vitamins, blood-stained diarrhœa is often noticed.

## MORBID ANATOMY.

I do not propose to say anything about the morbid changes found in the various organs, numerous and interesting as they are, except those of the gastro-intestinal tract. The digestive system is the first to suffer in food deficiency.

## INTESTINAL LESIONS.

The lesions in pigeons, guinea-pigs and monkeys were much the same, they consisted of great thinning and often ballooning of the intestines, atrophy and sometimes disappearance of muscular coats and intense congestion. With regard to guinea-pigs fed on crushed oats and autoclaved milk, in some cases there was intense congestion of the upper part of the duodenum, in others it was patchy, in others it extended to the whole of the small intestine. There were frequently hæmorrhagic infiltrations of the duodenum. On opening the bowel, ecchymoses were frequently seen, whilst areas resembling punched-out ulcers were occasionally seen, sometimes with adherent sloughs. In some cases punched out, necrotic ulcers were found in the stomach also.

Microscopical appearances showed degenerative changes, often with rupture and disappearance of the muscular fibres, atrophic and necrotic changes in the cellular elements and glands of the mucous membrane, degenerative changes in the mesenteric plexus of Auerbach: the normal histological structure of the ganglia was much altered, some were infiltrated with blood, in which no single cell contained a normal nucleus and the nucleoli were fragmented. The ganglia were considerably swollen and in many cases whole cells or groups of cells had disappeared; this being so, the nervous control of the bowel will be impaired in proportion to the degree of degeneration.

## ATROPHY OF LYMPHOID STRUCTURES.

The lymphoid cells, which fill in the space between the crypts and form a layer between the bowel lumen and the muscularis mucosæ, were atrophied, in some areas they were completely wanting. The muscularis mucosæ also had frequently disappeared.

## SYSTEMIC INFECTION FROM THE DISEASED BOWEL.

This is rendered more easy in the presence of these pathological processes owing to :—

(1) Continued congestion of the mucous membrane and consequent mal-nutrition of its secretory cells, leading to impaired production of digestive juices.

(2) Loss of the protective layer of lymphoid cells.

(3) Increased leucocytic invasion of the mucous membrane and increased leucocytic traffic in micro-organisms between the bowel, mucous membrane and the blood stream.

(4) The greater ease with which micro-organisms grow in the debilitated mucous membrane.

(5) Actual breaches of continuity in the walls of the bowel.

Occasional specimens have shown a direct continuity of blood corpuscles from an eroded villus to the blood-vessels running internal to the serous coat. In some cases bacteria of various types were found in large numbers throughout the necrotic mucous membrane and also in the sub-mucous, muscular and sub-serous coats. Intussusception was present in ten out of fourteen monkeys, they were usually multiple and invariably descending, they were found in the small intestine from the jejunum downwards. In some of these considerable constriction was present, although no inflammatory changes had occurred, the invaginated bowel being then much engorged and sometimes of a reddish-brown colour. It appears probable that the changes of the neuro-muscular mechanism of the bowel were concerned in their origin.

## LARGE INTESTINE.

There was colitis and great ballooning of the colon, with atrophy of its muscular coats, loss of the characteristic rugæ and a thin paper-like texture of the viscus. The colitis was as a rule both extensive and severe, small ulcers with adherent sloughs were often present. In some cases the colitis involved the whole colon, the serous coat being often ecchymotic. More commonly it was limited to the lower part of the colon and to the rectum, and occasionally a patchy colitis was present affecting limited areas. The colitis was invariably associated with enlargement and discoloration of the mesenteric glands. These changes, of great pathological importance, in the gastro-intestinal tract of monkeys can occur as a result of deficiency of vitamin B, alone, although they are more commonly caused by deficiencies of vitamins in general; the most severe of them is colitis. They are, especially as regards the stomach and small intestines, more frequent in the presence of an excess of starch and want of vitamin C. Similar changes in the sufferers from war œdema have been described by Oberndorfer.

The gastro-intestinal disorders occurring in deficiently fed animals were more marked in those whose diet was not only deficient in vitamins, but poor in protein and excessively rich in carbohydrates or fats or both. This defect is frequently seen in the dietaries of infants and young children and may be

expected to lead to similar disorders of a chronic nature from a long-continued use of deficient food, especially when given from an early age, when the proper development of the digestive apparatus would be permanently impaired. Continued use of deficient foods in adults will also cause more or less permanent changes in the digestive apparatus. The late war produced many instances of this, in addition to acute affections such as dysentery. Guarini reported forty cases of "large abdomen" occurring in prisoners of war; when in prison they had suffered severely from gastro-enteritis due to bad feeding. Months after their release, the abdomen remained enlarged and tympanitic; radiological examination showed gastropotosis and marked meteorism, especially of the colon. They suffered from constipation and were unable to do hard work or walk much and complained of shortness of breath.

Professor Sir Arthur Keith has found similar lesions, in the colons removed from patients suffering from chronic intestinal stasis, to those described above found in animals who had died from taking deficient food.

We have thus seen how deficient feeding produces grave defects in the glands, musculature and nervous mechanism of the intestinal tract; this is also true of the whole of the other organs and tissues of the body, but the mischief is first evident in the digestive tract. May we not then conclude that the fundamental cause which gives rise to intestinal stasis and its disastrous train of diseases, and, in fact, practically all intestinal diseases, is deficient feeding, which is often started at an early age?

There may be other causes in the production of intestinal diseases, such as adulteration of food, bad cooking, diseased teeth and oral sepsis. Investigations show that the two latter are greatly dependent on the use of fine white flour, which is so largely used to-day in civilized communities. Mr. Edgar J. Saxon makes the following statement:

"In the Highlands the deterioration of physique, even during my own recollection, is so marked as to cause concern to all. When I was a child I remember well what splendid looking men were found everywhere, and especially do I remember their beautiful teeth. Now very few have good teeth, and except on the more distant crofts (where they still keep much to their old food and habits); the men are not superior, and in some cases even less fine than in towns and in England, while the women are much more affected. Many have lost all their teeth by the time they are twenty-five or even younger. They drink strong tea and live on white bread and canned meats. Many of them are anæmic and most suffer from violent indigestion, in spite of coming from healthy stock and living in such fine air. Their houses and general sanitation have greatly improved since the time of their parents, and yet their general health and physique have greatly deteriorated, which must therefore be due to change in diet."

This report, with but slight variations, would hold good for every district of this island, for the towns as well as the open country.

Miss May Yates also states in her pamphlet "Science and Daily Bread":—

"The fellaheen in my father's Egyptian factory, who lived principally on brown bread, were very strong, and could easily carry bales of cotton weighing five hundred pounds; whilst I noticed equally good results among Sicilian peasants. Friends report that Hindoos of the North Western Provinces can walk fifty or sixty miles a day with no other food than 'chupatties' made of whole meal with a little 'ghee' or Galam butter."

Patrick Mummery has shown that races living in Southern Asia, who lived principally upon whole cereals, fruit and vegetables, had remarkably good

teeth; soldiers recruited from Sikh, Punjabi and Pathan races who mainly subsist on wheat meal, have very fine physique, stamina and perfect dentition. In the Northern Provinces of India, dentists are unknown and unrequired, but white flour is conspicuous by its absence. In Australia and New Zealand, where people use white bread, they have very bad teeth, 95 per cent. being afflicted with dental caries. Sir George Newman states that in Great Britain there are three million children in the elementary schools requiring dental treatment. The deterioration of teeth generally in this country is well known, this being coincident with the introduction of white flour into this country.

Dr. Samuel Rideal, President of the Society of Public Analysts, in his evidence before a House of Commons Committee, in December, 1919, stated:—

“The importance of whole meal bread as regards the teeth lies in the fact, that a great part of the calcium and phosphates are removed from white flour, and when wholemeal bread is used, the phosphates combine with lime in a mixed diet, particularly in vegetables, and thus increase the quantity of calcium phosphates and so contribute to the formation of bone and teeth. Wholemeal bread requires more chewing than white bread, and the soluble phosphates of the whole meal probably have a protective action upon the teeth. Lactic acid in the mouth readily combines with phosphates to form harmless lactates. Wholemeal bread contains about double the amount of calcium found in fine white flour and three times the amount of phosphoric acid.”

The importance of phosphates is recognized when we remember the large quantity of calcium found in bones and teeth: bone contains 51 per cent., cementum 57 per cent., dentine 64·5 per cent., and enamel 88 per cent.

The great importance of whole meal for the prevention of dental disease is constantly being emphasized by my brother, Sir Harry Baldwin, who was President of the Odontological Section of this Society a few years ago. He is strongly of opinion that the poorer classes of this country are suffering from partial vitamin starvation, and the more practical way of rectifying this is to ensure that the bread and flour which is supplied to them shall not be deprived of its vitamins. This, as it happens, goes hand in hand with the preservation of the valuable proteins and salts, which are also present in the germ.

It is notorious that the poorer classes of the community almost always use white bread; it forms a large part of their diet and its effects on them are much more disastrous than in the case of the well-to-do who can supplement the deficiencies of white bread by other and more expensive foods.

Professor Bunge has shown that rats fed on whole meal weigh four times as much as those fed on white flour, and owing to the whole meal containing four times more iron; the percentage as well as the absolute amount of hæmoglobin is increased. Mr. E. V. McCallum states that pigeons fed on fine flour, lost weight quicker and died sooner than pigeons who had had nothing at all to eat.

It has been stated that wholemeal flour is indigestible, but it has been proved by digestive experiments, at the Universities of Cambridge, Glasgow and London, that wheat meal ground to a uniform fineness can be assimilated by ordinary healthy digestions.

A good deal of modern constipation is caused by food being too refined; this applies to white bread, the aperient action of the bran being entirely eliminated. At the last dinner of this Sub-Section, a doctor who was my neighbour, told me he had had much trouble with constipation in his family, but since he had adopted the practice of giving them a teaspoonful of uncooked ground bran three times daily, the trouble had disappeared.

Professor Leonard Hill, F.R.S., said in an address before the British Association, 1913 :—

"Wholemeal or standard bread should be substituted for white bread. This question is of vital importance to the children of the very poor, brought up on white bread, margarine and tea."

And again in the *British Medical Journal*, he wrote :—

"To grind the wheat and take away, not only the germ—the very embodiment of growth—but to impoverish the flour in most of the essential organic phosphorus compounds contained in the outer layer, seems a most extraordinary manipulation of the staff of life. More extraordinary still is the fact that the State allows this to be done, and does not insist on the miller notifying that the flour has been thus impoverished. If such a thing were done to any other article of food the law surely would step in."

We have seen, since the introduction of fine white flour, a great deterioration in health and physique of peoples who use it in place of the whole meal they formerly used, especially of the poorer classes. The number of diseases connected with food deficiency are so great that there is no time to touch on them further, except those connected with the teeth and digestive tract. Colonel McCarrison's experiments have shown that in extensive vitamin deprivation there are grave lesions of the stomach and intestines, namely, atrophy of the muscular wall, of the plexus of Auerbach, of the glandular structures, and in fact of the whole of their tissues; congestions and also ulcerations of the pyloric end of the stomach, hæmorrhages and ulceration of the intestines being most marked at the upper end of the duodenum and in the colon and rectum, also ballooning of these organs. The symptoms are diarrhoea and dysentery, also degeneration and wasting of the organs and tissues of the body. These results were brought about in adult animals fed on sufficient quantity of food, but deprived of vitamins.

Similar changes, but in not so marked a degree, were produced in prisoners of war by insufficient feeding, both in quantity and quality.

In addition to the acute effects of food deficiency on the digestive organs, others of more chronic course or more remote onset are due to the same cause, especially when improper feeding is begun during early childhood, and continued into adult life. I cannot do better than again quote from Colonel McCarrison . . . "the derangements of bowel function to which food deficiency may ultimately gives rise are :—

- (1) Impairment of the protective resources of the gastro-intestinal tract, against pathogenic organisms.
- (2) Impairment of secretory and digestive functions.
- (3) Impairment of assimilative power.
- (4) Impairment of the neuro-muscular control of the stomach and bowel."

The three last are in a large measure responsible for the first, since the protective resources of the tract include the normal production of the gastric, pancreatic, biliary and intestinal secretions, the ordered processes of digestion and absorption, and the orderly transit of the gastro-intestinal contents along the digestive tube. If then, in the light of these results, we consider the case of children fed, it may be from birth, on deficient and ill-balanced foods, and the frequency with which in later life such faulty foods continue to be used, we are in a position to realize the ultimate consequences of such foods, and to anticipate the sequence of events leading up to grave derangements of the bowel functions, namely :—



## (1) CHRONIC GASTRO-INTESTINAL DYSPEPSIA.

This was constantly present in deficiently fed monkeys. That it may arise from this cause in human beings is illustrated by the effects on the prisoners of war already referred to, and is no doubt largely responsible for the vast amount of digestive disturbance of the present day.

## (2) MUCOUS DISEASE.

One of the earliest results of deficient and ill-balanced foods as observed in animals, is congestion of the gastro-intestinal mucosa; such a state of congestion in children may well give rise to gastro-intestinal catarrh, which characterizes "mucous disease." This disorder is very common among children who are fed largely on sterilized milk, artificial foods, white bread, polished rice, poor butter, overcooked vegetables, and excessive quantities of sugar.

## (3) COLITIS.

One of the most constant results of ill-balanced food and deficiency in vitamins is colitis. Many other features of this malady, as seen in nervous constipated women, are anæmia, unhealthy skin, loss of weight, lassitude, backache, colicky pains in the abdomen, bouts of diarrhoea alternating with constipation; the mucous stools, the neurotic condition, congestion of the uterus and ovaries, were reproduced in monkeys. Many of these cases in the human subject have no doubt resulted from long continued use of deficient food from childhood onwards. Although the condition of many of these unfortunate patients can be ameliorated by judicious treatment, they are never really cured, owing to the irreparable damage suffered by their colons.

## (4) INTUSSUSCEPTION.

Many of the cases of this condition found in animals dying of food deficiency was no doubt associated with the final stage of their existence, but some presented appearances suggestive of an earlier onset. May it not be that we have in vitamin deprivation the cause of the interference with the proper neuro-muscular control of the bowel which causes this catastrophe in young children, especially as the number of cases in Germany was greatly increased during the food shortage period of the Great War?

## (5) GASTRIC AND DUODENAL ULCER.

Both these conditions are met with in animals deprived of vitamins, and fed on ill-balanced foods. This points to the probability that in adults these ulcers may follow the continued irritation over a number of years due to partial vitamin deprivation.

## (6) CHRONIC INTESTINAL STASIS.

Professor Sir Arthur Keith has found pathological conditions in human colons removed for chronic intestinal stasis, similar to those found in the colons of animals dying as a result of being fed on ill-balanced food, deficient in vitamins. This points irresistably to the same cause in human beings, which is especially evident when operating from an early age.

We have seen a great deterioration in the physique and health of civilized communities. The one outstanding feature which one notices since the

degeneration has set in, is the use of fine white bread and flour, and this, I think, we may justly look upon as one of the chief causes of that degeneration, including constipation which is so prevalent and which is the forerunner of intestinal stasis and all its attendant miseries. Modern constipation is due to the removal of the aperient properties of the grain from the flour; these reside in the germ and in the bran, which should be retained in the flour, but it must be finely ground, to avoid indigestion in those people whose digestive powers have been weakened by the use of white bread and flour.

In my own household, no fine white bread or white flour is used. We have wholemeal bread, and household bread and flour. All our pastry is made of household flour. Household bread is readily accepted by all lovers of white bread, including the domestic servants. The household flour is stone ground and contains some of the germ but the bran is removed.

I feel some apology is due to you for giving an address upon what is not a strictly surgical subject, but its object is the avoidance of surgery. My excuse is, its immense importance, not only at the present day, but also to the future welfare of the Empire.

### **Case of Resection of the Transverse Colon and part of the Ileum for Growth.**

By J. P. LOCKHART-MUMMERY, F.R.C.S.

THE patient, a woman aged 50, began to complain of difficulty in getting the bowels to act six months ago. Recently there had been serious loss of weight and occasional vomiting. General dyspeptic symptoms and some abdominal discomfort.

*On examination* there was a large very hard tumour at the umbilical level and slightly to the left of the mid-line felt per abdomen. The tumour was movable.

#### **OPERATION.**

A large growth found in the middle of the transverse colon and adherent to stomach and mesentery of small intestine. The ileum was threatened by obstruction at a very early date, so that there was no means of relieving the condition apart from complete resection. This was a most formidable proceeding, the growth was dissected off the stomach and the lesser sac of the peritoneum opened. The transverse colon was then divided between clamps and the mass stripped off from above downwards and behind forwards from the coeliac axis and superior mesenteric artery with considerable difficulty. When this had been completed the small intestine was examined in order to ascertain how much of the blood supply had been damaged. It was then seen that about 8 in. of small intestine was dusky and the intestine was therefore resected 6 in. clear of this on both sides. The small intestine was then joined end to end, and the large intestine also joined end to end. The large intestine was then stitched to the lower edge of the stomach and the omentum turned over the join. Tubular cæcostomy was then carried out and the abdomen closed.

The patient made an excellent recovery and the abdominal wound healed by first intention. The tube in the cæcum came out on the ninth day.

Three Specimens of "*Atonic Cæcum*," removed by Operation.

By LIONEL E. C. NORBURY, F.R.C.S.

*Case I.*—Female, aged 37. Constipation for years. Attacks of pain in right iliac region, associated with increased constipation and marked swelling in cæcal region. Nausea—loss of weight, "bilious attacks" frequently. Appendicectomy thirteen years ago, with slight improvement in symptoms. Resection of loose atonic cæcum, September, 1923. Lateral anastomosis of ileum, with ascending colon. Mucous membrane of cæcum and termination of ileum deeply pigmented.

*Microscopy*: "Cæcum showed marked pigmentary changes in submucous tissue; muscular tissue entirely absent and replaced by fibrous tissue; similar, but less marked changes in termination of ileum; small hard calcareous gland at ileo-cæcal junction."

Considerable improvement in patient's condition since operation.

*Case II.*—Female, aged 35. Chronic constipation. Frequent attacks of pain in right lower abdomen with backache and vomiting, six months. Resection of mobile atonic cæcum and terminal inch of ileum, May, 1923. Lateral anastomosis between ileum and ascending colon.

*Microscopy*: "Complete muscular atrophy of cæcum with marked fibrosis. No actual ulceration."

Marked improvement in patient's condition.

*Case III.*—Female, aged 57. History of constipation. Attacks of abdominal pain soon after food, four years, worse six months. Loose atonic cæcum and colon. Resection of terminal portion of ileum, cæcum, ascending colon and hepatic flexure, November, 1922. Lateral anastomosis between ileum and transverse colon. *Result*: Satisfactory.

*Microscopy*: "Muscular atrophy and fibrosis involving both cæcum and appendix. Large stony hard gland at ileo-cæcal angle."

A Specimen of "*Hyperplastic Diverticulitis of the Cæcum*" was shown by Mr. W. B. GABRIEL, M.S., a description of which will be published later.

## Section of Surgery.

President—Mr. CYRIL A. R. NITCH, M.S.

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### PRESIDENT'S ADDRESS:

#### Cystic Pneumatosis of the Intestinal Tract.

By CYRIL A. R. NITCH (President).

(ABSTRACT.)<sup>1</sup>

MR. NITCH after referring to two cases of this remarkable condition which had been under his care and which were fully described by Professor Shattock and himself in the *Proceedings* of the Section of Pathology in 1919,<sup>2</sup> stated that out of eighty-five cases he had traced, only five had been recorded in the British Isles.

He then gave the historical details in chronological order from 1737 onwards, together with an account of an analogous condition found in the intestines of animals. After giving a full account of the anatomy and pathology of the lesions, he pointed out that the cysts were generally subserous or sub-mucous, usually in the small intestine, and were associated with an obstructive lesion of the gastro-intestinal tract in over 70 per cent. of recorded cases. In reviewing the various theories which have been advanced to account for the formation of the cysts and their gaseous contents, he suggested that the diversity of the opinions regarding the ætiology of intestinal pneumatosis was due to the existence of two definite varieties, the mechanical and the infective, which were illustrated by his two cases. Treatment consists of appropriate measures for the relief of the primary disease in the mechanical form and resection of the affected bowel in the infective form.

#### Treatment of Recurrent Lesions of the Semilunar Cartilages of the Knee-joint, with special reference to manipulation.

By A. G. TIMBRELL FISHER, M.C., F.R.C.S.(Eng.)

(Late Hunterian Professor of the Royal College of Surgeons of England, 1921 and 1922.)

THE subject of internal derangement of the knee is a very large one, owing to the great number of different conditions that may give rise to symptoms of internal derangement, some intra- and some extra-articular. The differential diagnosis of these conditions is, perhaps, one of the most difficult problems that the practitioner encounters.

<sup>1</sup> The complete paper will appear in a future issue of the *British Journal of Surgery*.

<sup>2</sup> "Emphysema of the Intestinal Wall," *Proceedings*, 1919, xii (Sect. Path.) pp. 46—86.

We have also to face the somewhat unpleasant fact that many of the community regard this subject as lying within the legitimate domain of the unqualified bone-setter, a condition of affairs that reflects discredit upon us principally because there has been a lack of serious and organized research into the underlying pathology of the cases that may be cured by manipulation. Moreover, our knowledge of the physiology of such joints as the knee is almost mediaeval. This attitude of apathy and neglect on the part of our profession towards problems of such considerable importance is a subject of grave concern to many, particularly to the public, who are often not so ignorant about medical problems as some of us imagine. Yet there is another aspect of the problem that must be borne in mind and this we may call the "publicity factor." The successful cases of the bone-setter are broadcasted by the popular press, but the public hear little of the failures, and those of us who have subsequently to treat many of these cases of "cures" know that the doctrine of infallibility that has been sedulously fostered will not bear close investigation. Yet if the bone-setter by mere rule-of-thumb methods, often uncertain and sometimes dangerous, can achieve good results by manipulation in certain cases, may we not believe that when the underlying physiology and pathology of the cases cured by manipulation is fully understood, a therapeutic measure of the highest value will be added to the surgeon's armamentarium? It is absurd to assume that all cases of internal derangement can be cured by manipulation, nevertheless there are certain types of cases in which one frequently employs this method with most gratifying results.

I propose this evening to confine my remarks to the treatment of the chronic or recurrent lesions of the semilunar cartilages, touching briefly upon those physiological or pathological aspects that have a direct bearing upon treatment. In such a short paper it is only possible to give a very condensed and abbreviated outline of my work, which has been dealt with more fully in a recent publication.<sup>1</sup>

#### PHYSIOLOGY.

In arriving at any conclusion concerning the respective merits of non-operative and operative methods of treatment, it is clear that this must be based upon the fundamental question of repair in the menisci, which is largely governed by their nutrition. Repair in cartilage is slow and sluggish, and the semilunar cartilage is no exception to this rule. The convex or peripheral parts of the cartilages are better nourished than their central or concave portions. By delicate injection methods it can be shown that the peripheral portions contain a plentiful supply of blood-vessels, which run transversely across the peripheral part, whereas these cannot be detected in the inner half of the cartilage. There can be little doubt that the latter portion is nourished by the synovia. There is, therefore, an analogy between the nourishment of the semilunars and that of the articular cartilage.<sup>2</sup> It would appear clear that, with appropriate treatment, we may expect repair to take place more readily when a detachment or tear has taken place near the capsular attachment through the vascular zone, but that in those cases in which the tear has occurred in the centre or near the concave edge, repair is extremely slow and in many cases may be quite absent. We find in practice that where the original injury

<sup>1</sup> "Internal Derangements of the Knee-joint. Their Pathology and Treatment by Modern Methods." H. K. Lewis. 1924.

<sup>2</sup> "Researches into the Physiological Principles Underlying the Treatment of Injuries and Diseases of the Articulations," *Lancet*, September 15, 1923.

has been improperly treated, the opposed edges of the tear frequently fail to unite and become smooth and endothelialized. When this change has occurred, the chances of repair are reduced to a minimum. It is probable that the recurrence of symptoms, which sometimes occurs even when the original injury has been treated by a long period of rest and careful treatment, depends upon this physiological factor. Certain specimens of oblique or transverse fracture demonstrate this in a striking manner. Repair by fibrous tissue has taken place peripherally and is entirely absent in the more central parts. Experimental evidence confirms in a striking manner the deficient powers of repair of the central portion. The right knee-joint of a rabbit was opened, under aseptic precautions, and a longitudinal incision made for one-third of an inch in the anterior portion of the internal semilunar near the concave margin. The actual anterior attachment was not divided. The joint was closed and fixed to prevent movement. At the end of six weeks the animal was killed, and it was seen that no repair had taken place in the incision in the semilunar. Indeed, the cartilage on either side of the incision had undergone necrosis. It was found, under similar conditions, that in the case of the transverse incisions the peripheral part healed readily and that the inner part remained ununited.

#### PATHOLOGICAL TYPES.

These physiological facts have a fundamental relation to the pathological types that may occur, and hence upon treatment. There is a surprising discrepancy between the reports of various operators concerning the relative frequency of lesions of the menisci. But most observers are agreed that fractures of the cartilage form the commonest lesion, and that of these, the longitudinal or bucket-handle type predominates. In Martin's series, 95.5 per cent. showed definite splits or tears, and of these, longitudinal tears predominated. In the author's series:—

Longitudinal fractures were 38.1 per cent.

Detachments of anterior horn, without obvious fracture, 33.3 per cent.

Detachments of posterior horn, usually associated with oblique fracture, 19 per cent.

Transverse fractures, 9.6 per cent.

We have seen that in these cases the inner fragment or portion of cartilage is cut off from its principal source of nutrition. If reduction is effected early, then we may expect repair to take place slowly in a certain proportion, but the longer accurate apposition is postponed, the less are the chances of repair. In neglected cases, the fractured surfaces become smooth and shiny, and although it would be rash to say that union is then impossible, the chances of it occurring, even with manipulation followed by a period of immobilization, are very slight.

The bearing of these physiological and pathological facts upon treatment seems to be that the grosser lesions of the menisci, such as the complete longitudinal fracture-dislocation (the bucket-handle type) that have become chronic, do not lend themselves to cure by manipulative replacement, except in rare cases.

It has been held by some that by repeated manipulation the handle of the bucket is gradually divided, and that a cure thus ensues. The argument against this lies in the fact that in those cases in which the division has occurred by natural attrition without manipulation, the symptoms still persist, although in a less degree. For example the diagram exhibited shows three internal semilunar cartilages, which, by a coincidence, were removed in succession. The first shows a typical complete bucket-handle type; the second is clearly of a similar

type, in which the handle of the bucket has undergone division in its centre; and the third is a similar type in which the remains of the handle of the bucket consist of a small flattened disc. Yet in the two latter cases the patients complained of frequent giving-way of the knee, without actual locking. The second stated that a rounded body, the size of a hazel nut, appeared occasionally at the front and inner side of the right knee over the joint interval during certain twisting movements. This was particularly apt to follow a twisting of the leg outwards upon the fixed thigh, or twisting of the thigh inwards upon the fixed leg. Occasionally definite locking occurred. This, however, was of short duration, and he had learned the knack of reducing it himself.

(A) *Some Pathological Types Amenable to Manipulation.*

It has been seen that of the cases of recurrent semilunar derangement that come to operation, the great majority are obvious fractures or gross displacements, yet it would be extremely rash to assume that such obvious lesions constitute the most frequent type of lesion. There is a group of conditions to which I particularly desire to draw attention, that interfere with the normal "screw-home" movement of the inner femoral condyle, and which are particularly and conspicuously amenable to treatment by manipulation. These are:—

(a) Minor displacements of the anterior half of the internal semilunar cartilage due to stretching or tearing of the coronary attachment.

(b) Inflammatory induration of the semilunar process of the infra-patellar pad of fat with adhesions in connexion with the latter process.

Leaving out of account original unreduced displacements of the menisci, functional disabilities of the knee, and cases where stiffness from adhesions following prolonged immobility is present, all of which are a fruitful field for manipulative surgery, I shall proceed to speak of the foregoing two divisions.

(a) *Minor Displacements of the Anterior Horn of the Internal Semilunar.*—To refer briefly to the anatomy of this important region: If a transverse section be made through the knee-joint at the level of the semilunar cartilages, it will be seen that the internal semilunar, in addition to the attachment to the true capsule, is firmly adherent to the deep fibres of the internal lateral ligament, and behind this to the aponeurotic layer. The anterior half of the cartilage, however, is separated from the aponeurotic layer by a well marked interval filled with loose fatty tissue, and the periphery is attached to the margin of the internal tibial condyle by a portion of the true capsule, known as the coronary ligament. A weak mechanical site therefore exists at the spot where the mobile anterior half meets the more fixed posterior half, opposite the level of the internal lateral ligament. A sudden twist inwards of the femur upon the fixed tibia with the knee slightly flexed, may, without causing a definite tear of the cartilage or of its anterior attachment, cause a partial rupture of the coronary attachment and a minor displacement not sufficient to cause obvious locking, but a feeling of discomfort on full extension, which may be limited by a few degrees.

(b) *The Semilunar Extensions of the Infra-patellar Pad of Fat.*—In the investigation of 100 formalin-hardened knee-joints I found that the anterior one-third of the internal semilunar cartilage is usually protected by a process of the infra-patellar pad of fat, upon which the important screw-home movement of the internal femoral condyle occurs at the termination of extension; and this process is frequently injured, either alone, or simultaneously with injury

to the anterior end of the internal semilunar. Although the process is so important clinically, it has not been considered worthy of a name by the anatomists, although it is often wrongly called the inner alar pad. Possibly "semilunar process" or "pad" might be a better name. In a large number of cases in which it is considered that a lesion of the cartilage is present, it is really this process which is at fault. A lesion of the process may be followed by the formation of adhesions between it and the cartilage, or some other adjacent structure.

It seems clear, however, that it is in this group of conditions that manipulation is a valuable therapeutic measure.

#### METHODS OF MANIPULATION.

The knee should first of all be fully flexed and fully extended. It then should be fully flexed and slowly extended, and during the whole of the last act the knee should be rotated inwards and outwards at least ten or fifteen times. A movement upon which I lay great stress is that towards the end of the extension the tibia is to be forcibly externally rotated, an exaggeration of the screw-home movement, which is of great value in forcing the anterior end of the cartilage or semilunar pad into place, or in rupturing adhesions at this spot.

*Illustrative Case.*—A. E., aged 17, complained of frequent "giving-way" of the knee, which was particularly apt to occur at football, and was followed on occasion by swelling. There was an absence of true "locking." He stated that eighteen months previously, while playing football, he twisted the right knee, the foot being fixed upon the ground and the thigh rotated outwards. There was sudden severe pain in the joint and he fell to the ground, but there was an absence of locking. Although he was treated by his doctor by prolonged rest, eight months later the knee suddenly gave way again, and this happened several times subsequently.

On examination, slight swelling of the joint was found, and tenderness was present over the anterior ends of both semilunars, particularly the internal. There was no tenderness over the attachment of the internal lateral ligament. Movements were as follows: Flexion limited by thirty degrees, extension full, but complete external rotation of the tibia at the end of the former movement was painful, and suggested to me that the internal semilunar pad was enlarged and tender, and that probably adhesions were present in its vicinity. There was one inch wasting of the right thigh in the lower third.

Under gas and oxygen anæsthesia, which produced excellent relaxation, manipulation was performed in the manner I have described. During movement, an adhesion was distinctly felt and heard to give way. The knee was firmly bandaged over a thick layer of cotton wool, and the following day the bandage was removed, and massage and exercises commenced. A week later a note states: "The joint is normal in appearance, the swelling having subsided. Movements are full and painless. He is walking normally." Several months later the joint was still normal and the symptoms of "giving-way" had entirely ceased.

#### RE-EDUCATION OF THE QUADRICEPS EXTENSOR.

Manipulation should, as a routine, be followed by a course of exercises for the quadriceps, and in many cases these exercises, if persevered in, are alone sufficient to bring about a cure in many cases of displacement.

The rationale of this mode of treatment consists in the fact that the fibres from the subcrureus can be traced directly into the true capsule, which here forms a suspensory ligament for the anterior portion of the internal semilunar cartilage and the semilunar pad.



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*Illustrative Case.*—During a trial game, an undergraduate severely twisted his knee, displacing his internal semilunar cartilage. This was “replaced” by a bone-setter, but ever since this “replacement” he had been subject to frequent “giving way” of the joint, without, however, definite locking. On examination, there was localized tenderness over the anterior end of the internal semilunar cartilage, a little lateral mobility, and definite wasting of the thigh, principally involving the vastus internus. After a few months’ treatment, directed towards improving the tone and power of the quadriceps, during which time the principle of weight deflection to the outer side of the foot was faithfully followed, the wasting disappeared, and his knee gave rise to no further trouble.

### DIAGNOSIS OF CASES AMENABLE TO MANIPULATION.

Can we diagnose this group of conditions? The following points are helpful: There is tenderness on firm pressure over the position of the process, and a little pain and limitation of full flexion. Full extension may be painful, and if the tibia be firmly rotated outwards at the end of extension the patient complains of pain. There is another complaint of weakness and unreliability of the joint, a tendency for it to swell after exercise, and a sensation of “giving way,” but an absence of true locking or of the more acute symptoms associated with a gross lesion.

It should be remembered that there are other pathological conditions of the infra-patellar pad of fat from which these lesions have to be differentiated. These include generalized hypertrophy, hæmorrhage, nodular tuberculosis, chondromatosis,<sup>1</sup> and I have recently encountered a myeloma in connexion with this structure.

#### (B) *Brief Notes on Treatment of Semilunar Derangements not amenable to Manipulation.*

Having dealt, in a cursory way, with the types of cases amenable to manipulative methods, I will now briefly discuss a few points concerning the treatment of that other large group of cases in which some gross recurrent lesion, such as fracture-dislocation, is present, usually characterized clinically by attacks of true mechanical “locking.”

(a) *Treatment by Mechanical Methods.*—There is no doubt that in certain cases the “locking” or “giving way” of the knee may be prevented by the wearing of a suitable type of knee-cage, which, although permitting flexion and extension, prevents rotation and lateral movements. Compared with operation, it is tedious and unreliable. Fifty years ago an operation upon the knee-joint was a somewhat desperate adventure, but in modern days the occurrence of septic complications in experienced hands is extremely rare, and the results of operation are such that the removal of the damaged semilunar cartilage should be confidently recommended, rather than the condemnation of an otherwise healthy and vigorous person to the wearing of an apparatus, possibly for the rest of his life. In these cases, moreover, although the “locking” may be prevented by a well-fitting support, yet the damaged cartilage remains as a focus of intra-articular irritation, and often brings about the occurrence of chronic arthritis. The occurrence of tuberculous disease as a sequel has been recorded.

The onset of chronic arthritis<sup>2</sup> in neglected cases is so frequent that treatment by apparatus is only justified if the patient’s age or condition does not

<sup>1</sup> “A Hunterian Lecture on Loose Bodies in Joints,” *Lancet*, 1921, i, p. 839.

<sup>2</sup> “The Nature of the So-called Rheumatoid Arthritis and Osteo-Arthritis,” *Brit. Med. Journ.*, July 21, 1923.

warrant an operation; if advanced osteo-arthritic changes, not amenable to operation are already present; or if the patient declines operation, the arguments for and against the different forms of treatment having been put before him.

#### OPERATIVE TREATMENT.

Any method of approach which entails division of the lateral ligaments, particularly the internal, is undesirable; experience teaches that their division may lead to considerable instability. A variety of incisions have been adopted from time to time by surgeons. Vertical, oblique, transverse, or curved incisions over the joint interval all have their advocates.

The ideal incision must provide a good exposure of the semilunar cartilage, must be of such a nature as to allow sufficient retraction to permit removal, aided by vision, of the cartilage, or such portion of it as may be necessary, and in addition should permit of examination of the remainder of the joint. The only exposures which would appear to fulfil the last of these conditions are the patella-splitting exposure, and the author's patella-displacing exposure.<sup>1</sup> Both these methods give a satisfactory exposure of the whole of the anterior compartment of the joint, but are unnecessary in cases with classical symptoms, although they are strongly indicated in complicated or anomalous cases. The transverse incision of Annandale must, for anatomical reasons and if we respect the patella and lateral ligaments, be very limited.

The limited exposure, save in very experienced hands, makes the removal of the whole cartilage, should this be necessary, a difficult procedure. In addition, it does not permit a satisfactory view of the neighbouring parts. The vertical incision allows a better view of the surrounding parts and wider retraction, but is not ideal from the point of view of removal of the cartilage. Probably the best exposure is obtained by a curved incision, which is a combination of the vertical and the transverse.

The mode of access I have designed for cases with classical symptoms is also a curved incision. The more vertical element of the curved incision coincides with the anterior margin of the internal lateral ligament, and the more horizontal element curves forwards towards the patellar ligament. This exposure not only allows removal of the anterior end of the semilunar when alone at fault, but greatly facilitates the removal of the entire cartilage when necessary. It is found that by this method the internal lateral ligament can be retracted, and all the attachments of the cartilage divided with ease and aided by vision. It is, of course, equally applicable for removal of the external semilunar when made upon the outer aspect of the joint. A tourniquet is used, and the leg bent at a right angle to the thigh over the end of the operating table. The aponeurotic layer is first divided, and next the capsule and synovial membrane. The procedure to be adopted depends on the type of lesion found.

In cases in which the anterior portion of the cartilage is the sole portion affected, this only should be removed, a transverse incision being made with a narrow-bladed scalpel as far back as possible, care being taken not to loosen the posterior part by powerful traction, since this may bring about a recurrence of the symptoms. The scalpel should also be used to divide any further attachments present, again care being taken not to leave behind portions which may give rise to trouble. When a pedicled tag is present, the rest of the cartilage being normal, it is sufficient to remove the former.

<sup>1</sup> "Treatment of Internal Derangements of the Knee-Joint: A New Method of Operative Exposure," *Lancet*, May 12, 1923.

Although there is, perhaps, room for a difference of opinion on this point, experienced surgeons are less favourably inclined than hitherto towards partial removal of a damaged semilunar, believing that anything short of complete removal is a cause of recurrence of symptoms. Perhaps the wisest procedure is to treat every case on its own merits and not to attempt any hard and fast rule.

It not infrequently happens that on opening a joint, nothing abnormal can be seen, although the symptoms before the operation were typical. The uncertain surgeon, in such cases, either closes the joint or removes an unoffending portion of cartilage. This, however, is an unwise procedure. The surgeon should, before closing the joint, be as sure as possible of his diagnosis, and, if he finds nothing abnormal at first sight, should proceed to remove the whole cartilage; for when the latter is drawn across the joint, a tear of the posterior part is often disclosed.

In those cases where it is obviously desirable to remove the whole cartilage, a combination of traction and cutting is adopted, the anterior portion being grasped by strong forceps and drawn across the joint. Blind snipping with curved scissors is to be avoided, for damage may be done to the posterior crucial ligament. In removing the posterior end, useful manœuvres consist in the pushing forward of the head of the tibia by pressure in the popliteal space, or by external rotation of the leg. This often brings the posterior attachment of the semilunar cartilage into view, and this can be divided aided by direct vision. A fine, blunt-ended tenotomy knife will often be found useful at this stage. No operation is to be considered complete until the condition of the opposite semilunar cartilage has been ascertained, and the surgeon has satisfied himself concerning the absence of coexistent pathological factors, particularly of hypertrophied fringes in the vicinity of the cartilage. Great care and gentleness must be exercised, to avoid dragging upon the edges of the wound, and the finger should never be introduced into the joint. Accurate and careful suturing of the synovial membrane and of the semilunar pad in the lower part of the incision is an important factor in preventing post-operative hæmarthrosis. The aponeurotic layer should also be sutured very carefully, as hernia of the synovial membrane may follow the giving-way of this suture. A drain is only very rarely needed, firm bandaging over a thick layer of wool being as a rule potent in preventing post-operative distention of the joint. Perhaps the best comment on the practice of drainage is afforded by the following sentence from a book written in the early days of the surgery of the semilunars: "When the case is emphatically a 'dirty' one—that is to say, when much blood has got into the joint or much fingering has been requisite—I use drainage."

#### NOTES ON AFTER-TREATMENT.

There is often considerable pain for the first twenty-four hours after operation. It is a wise practice to order a hypodermic injection of morphia to be given on the first night. A slight rise of temperature for one or two nights is frequently seen, but need give rise to no apprehension in the absence of other signs. Active movements should be instituted at an early moment. Prolonged immobilization is still too frequently practised by many surgeons. The after-treatment is thereby greatly prolonged, and subsequent mobilization often becomes necessary. Skin stitches are removed on the seventh day after operation, and the institution of massage and movements, both active and passive, should never be delayed beyond this date; indeed in most cases massage of the thigh and leg, and movements of the distal joints may be commenced

actually before the removal of the stitches. The patient may be allowed to sit on a couch with his legs up from the fifth day onwards. At the end of a fortnight, active flexion should be possible to certainly not less than  $90^{\circ}$ , often considerably more, and the patient is encouraged to walk about aided by stick or crutches, massage and exercises being continued. After a few days these may be discarded and full weight-bearing encouraged.

#### CAUSES OF RECURRENCE OF SYMPTOMS.

It will be obvious from these remarks that incomplete removal of the cartilage is a potent cause of recurrence, particularly when the portion remaining has been dragged upon by the surgeon, and its attachments loosened. Recurrence was common after the now obsolete operation of stitching the detached portion of the cartilage *in situ*. Another frequent cause of recurrence of symptoms lies in the presence of some co-existent pathological condition, such as loose body, hypertrophied fringe, ligamentous lesion, or arthritic changes.

Faulty after-treatment, particularly prolonged splintage, may be a cause of prolonged disability.

#### PROGNOSIS.

The prognosis, assuming a correct diagnosis and a satisfactory type of operation, depends upon two principal factors: (a) the number and severity of attacks, (b) the presence or absence of arthritic changes.

(a) Repeated attacks of locking or giving way, followed by distension of the joint cavity, lead in time to relaxation of ligaments and joint instability. Operation in these cases may be of great benefit, but in some there is a feeling of weakness, and insecurity of the joint persists, in spite of careful after-treatment.

(b) Chronic arthritis is apt to supervene in neglected cases, particularly in those who have passed the meridian of life. The age factor, however, is by no means constant. I have encountered many cases of marked and generalized arthritic changes in young men in the twenties, due to neglected semilunar derangements. On the other hand, I have met with cases in which derangement has existed for twenty or twenty-five years and in which arthritic changes were absent. It is clear, therefore, that both these factors are due to neglect, and are therefore preventable, and that with more careful selection of cases for operation, this operation should be one of the most successful in the whole realm of surgery.

#### DISCUSSION.

Mr. R. H. ANGLIN WHITELOCKE said that living as he did, in an athletic atmosphere—Oxford—cases of the kind were brought to him almost daily, and he had lived long enough to go upon his own ideas, formulated as a result of his practice. His work was among athletes, as compared with Mr. Martin's work among miners in Newcastle. The injury to the joint was different in those two groups of persons. In one case the more common injury was a splitting longitudinally of the semilunar cartilage: the miner swinging his body from side to side, with both knees flexed fully, got an injury to his semilunar cartilage in the form of a peripheral splitting. The athlete, from rapid turning with the knee but slightly flexed, almost in extension, tore his semilunar cartilage across in its more movable anterior third, the posterior two-thirds being more fixed anatomically. Sometimes the periphery of the cartilage was separated from the capsule, an injury more common in the miner. Those were the few cases which, in his experience, were sometimes cured without operation. An athlete who tore his cartilage would never be a good athlete again unless that torn

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portion of cartilage were removed. He had removed some hundreds during his professional life.

Any incision into the knee-joint which interfered with the internal lateral ligament would prevent the man from being an athlete. A transverse incision, such as was often advocated and practised, as for removal of a cartilage *in toto*, was very apt to damage the internal lateral ligament. Often during and since the war he had had to operate on and treat officers whose internal lateral ligaments had been damaged; and some of those subjects were now neither fit to be soldiers nor athletes; some could not even hunt. His main object in speaking was to emphasize these points. The whole of the internal cartilage should not be removed, if that could be avoided; attempts to do so often damaged the internal ligament.

With regard to the minor cases, he did not think it was possible to diagnose internal derangement in the absence of locking; recurrent locking he regarded as the only symptom on which one should advise the knee-joint to be opened.

The manipulative treatment of these cases, now so much in vogue, would, he thought, soon go out of fashion were it not that members of the profession sent cases to those people, bone-setters and "manipulators," to treat. And, as Mr. Timbrell Fisher said, the public did not learn of the bone-setters' failures, but only the successes in distinguished people. Within one month, last term, five people who were said to have been cured by bone-setters consulted him, yet two of them within three days of their "cure" were again *hors de combat*. Moreover, any case of pain in the knee-joint was in these days considered to be one of internal derangement. Still, internal derangements of the knee-joint were very common among athletes, and perhaps even more common in miners. It was a very important event in any man's life.

He did not think osteo-arthritic changes were likely to be met with before the age of 80; and every man over 40 with a knee condition due to displaced or torn cartilage he advised to be cured by operation. He had known cases in which the joint had gone wrong because that had not been done. The operation was not difficult, and with proper care and a correct surgical technique the risk was very small. Practically speaking, the prognosis should be almost perfect. A man who had had the injured part of a cartilage properly removed should be able to play any game. In one International Rugby match, between Scotland and England, he had operated upon seven of the players, on both knees of one of them. He never used a splint for these cases, and he did not now drain, though at one time he did. By the eighth or ninth day he had his patient walking with the aid of a stick, and he was expected to play for his "Blue" in six weeks.

Mr. GREY TURNER said he thought the work which the author had devoted to the pathological points was most important, as it had previously been much neglected.

There was at present a tendency, not only on the part of the public, but also of the profession, to say that bad results frequently followed operations for torn semilunar cartilages, but this was a wrong statement. If people would cease assuming that every person who pressed to have an incision made about the knee-joint had really suffered from a definitely torn or diseased cartilage, the error would be largely rectified. If the pathological condition was such as to justify operation, and the torn or diseased cartilage was properly removed then there could be no doubt that, in the majority of cases, the results were extremely satisfactory.

The prognosis, in each particular case, depended on the degree of damage which had been done to the knee-joint by the repeated attacks following the locking of the cartilage. There was a class of case in which a progressive arthritis was set up, particularly in older subjects, and in that class arthritis was not always arrested, though it might be improved, by the removal of the affected cartilage. The results were so satisfactory that it was not uncommon for a miner to have a cartilage removed from the opposite knee-joint. Recently he had looked up a small series of cases in which the pathology was absolutely definite, a tear being present in the cartilage removed; no patient in the series had been operated upon less than nine years previously. Of that series, one patient had died, from another cause, one was killed in France and nine could not be found. The remaining thirteen patients came to show themselves. They

were all well, and had continued to carry on their laborious work, and were quite content with the result of the operation. One man subsequently had a cartilage removed from the opposite knee.

He (Mr. Grey Turner) wished to defend the transverse incision for these cases. Anatomy showed that any incision which injured the internal lateral ligament must be wrong, but it was no part of the transverse incision to interfere with this structure. There were times, especially during the war, when men who had had no training in the surgery of joints adopted the transverse incision without regard to its limitations, and with dire consequences. But that was no reason why the operation in competent hands should be condemned. The transverse incision was taught to him by Mr. Martin, of Newcastle-upon-Tyne, years ago, and he (the speaker) did not hesitate to characterize it as a thoroughly good one. When dealing with a lesion of the internal cartilage it gave ample exposure, and there was no occasion to injure the internal lateral ligament.

He was glad to hear Mr. Anglin Whitelocke's objection to attempts to take away the extreme posterior end of the internal cartilage. If the cartilage were divided in front of it, the tension of the ligament pulled the posterior part backwards opposite the intercondyloid notch where it could not be nipped or do any harm. He was much interested in hearing Mr. Fisher say that the joint should not be closed until one was satisfied as to the condition of the external cartilage, but he did not agree that one could put the finger into the joint to feel the external cartilage. If he (Mr. Grey Turner) did not find a tear in the internal cartilage and he felt sure the patient had a torn cartilage, he made an independent incision over the external cartilage, and on several occasions he had been rewarded by finding a tear in that cartilage. By cutting across the patella it was possible to get a perfect exposure of the whole of the interior of the knee-joint, but for ordinary cartilage cases that was never necessary.

Mr. Fisher led the Section to think that the knee-joint should never be drained; but he (Mr. Grey Turner) thought drainage should be employed if there was hæmorrhage into the joint; or in the case of a very sensitive type of joint, in which there was likely to be a severe reaction after operation, with pain and temperature.

Mr. C. MAX PAGE, raised the question of the use of the tourniquet in these cases when operating. At the present time it was the fashion to employ the tourniquet for knee-joint operations, but he thought it was wrong to do so. To deprive any part of the body of its blood supply for any length of time meant devitalizing the tissue to some extent, and by using a tourniquet one was increasing the chances of the establishment of infection in a joint. He thought there was less effusion and less bleeding into the joint if the operation were done in the ordinary surgical way, controlling the hæmorrhage as it arose, without employing a tourniquet. The only unfortunate knee-joint case he had had—due to infection—was in a man on whom he operated and used a tourniquet. This patient had a large hæmarthrosis afterwards, followed by late infection and a stiff knee; he (Mr. Max Page) had felt that infection would have had less chance in that knee had its resistance not been lowered by the use of a tourniquet.

Mr. R. C. ELMSLIE said that Mr. Timbrell Fisher's paper was so complete that little was left for others to say. But Mr. Fisher had asked him a question upon which he (Mr. Elmslie) felt strongly, namely, whether he (the speaker) believed that a cartilage which had been badly torn—for example, the bucket-handle split—was, or could be, cured by manipulation. He felt certain that some of those cases were so cured. For example, there were cases with definite splits of cartilage in which the knee was completely locked, and which had to be unlocked, under an anæsthetic, by manipulation, and the patient made a good recovery and had no recurrence. He could mention a number of such cases. Something had happened which had either reduced that cartilage back to its original site, to which it had adhered, or possibly it had not been reduced to its original position, but to one in which it no longer gave trouble. He believed there were some cases in which the cartilage got into the centre of the joint and lay antero-posteriorly across it, became adherent there and did not interfere with movement. Whether those people could afterwards carry on the life of an athlete, in the meaning given to it by Mr. Anglin Whitelocke, he did not know, but they could afterwards play Rugby football, and jump.

## 56 Fisher: *Lesions of Semilunar Cartilages of the Knee-joint*

And other conditions might follow manipulation. Mr. Martin referred, at a meeting of the Association of Surgeons, to a case in which, after cartilage symptoms were complained of, the knee-joint was opened and no semilunar cartilage was found at all. In a case of his (the speaker's) own there was a slight access of synovial fluid, and it contained a number of flakes of cartilage. The man had had his knee locked some months before. It had not been reduced, but the knee became straight, and it was straight when he (Mr. Elmslie) operated, and it still had some fluid in it. That was another instance in which a semilunar cartilage, having been displaced, might be destroyed to some extent and the remains become fixed in a situation where it caused no trouble. But he felt, with Mr. Timbrell Fisher, that the majority of the cases which were cured by manipulation had never had a real cartilage injury at all; and he thought the profession was responsible for a great deal of error regarding the cure of cartilage conditions by manipulation by diagnosing such conditions on far too slight evidence. It was notoriously difficult to get a precise history from patients who came with an alleged cartilage injury, and in nine cases out of ten there had been no real locking. He agreed that in the absence of locking a diagnosis of injury to semilunar cartilage was not justified.

With regard to drainage, all were familiar with the cases which had a reaction, a slight rise of temperature with an effusion into the joint which often contained blood; but he did not see the need of draining. He preferred aspiration of the joint, after which his experience was that the joint did not refill.

Sir CHARLES GORDON-WATSON said that Mr. Anglin Whitelocke had stated that in a large percentage of his cases tears of both the anterior and the posterior extremities had been met with. He (the speaker) believed that in four out of five cases a circumferential split occurred—the so-called “bucket-handle” type. Sometimes the bucket handle was torn across at the time of the initial accident or during a subsequent displacement. This condition, in which a split occurred, combined with a transverse fracture, might be described as “fracture dislocation,” as opposed to “dislocation.”

Once a split had occurred, recurrent displacements were to be expected, but if the split portion or “bucket-handle” was torn across, the torn ends might shrivel up out of the way and spontaneous cure result. In his (the speaker's) opinion this was an important factor in the bone-setter's success.

In the case of patients seeking advice, with the cartilage displaced, preventing full extension, or the knee locked, forcible extension might convert the dislocation into a fracture-dislocation and as stated above be followed by spontaneous cure in many instances. In other instances extreme violence might fail to produce the desired result and more serious consequences ensue. He knew of one instance in which this had occurred and a stiff joint had resulted. His explanation of the splitting of the cartilage was as follows: During flexion, external rotation and abduction the internal cartilage was normally drawn toward the apex of the femoral condyle. If the abduction was excessive the cartilage might slip in between the condyle and the tibia and get nipped. When this occurred the knee would be locked, pain would be severe and there would be a tendency to fall. The patient, in making an effort to save the fall, would contract his extensors. If the cartilage remained nipped between the femur and the tibia during the act of extension a tear in the circumference would result. He did not know whether Mr. Grey Turner would agree that this was the mechanism in the case of the miner who carried out his work in a stooping position. The miner would no doubt contract his extensors when endeavouring to save himself from a fall.

He (Sir Charles Gordon-Watson) said he felt very strongly that it was not necessary to remove the posterior fourth or fifth where there was a circumferential split of the anterior half of a cartilage. Removal of the posterior extremity was not easy, and it was often difficult to avoid injury to the articular cartilage of the tibia and femur in cutting away the posterior portion. He did not believe that the posterior fifth when left intact gave rise to subsequent derangements.

He agreed with Mr. Grey Turner that the external cartilage could not be properly examined from an internal incision. On the question of the transverse incision through the capsule, he agreed with Mr. Grey Turner.

Mr. R. P. ROWLANDS said he was disappointed to hear Mr. Max Page say he did not believe in the tourniquet for knee cases; he (the speaker) regarded it as the key-stone to the question. It enabled one to do good work in a short time, and with no fear of bleeding. But it was necessary to put on a massive firmly bound dressing before the tourniquet was removed, a course of action omitted by many surgeons. If one secured proper pressure at the right time and in the right place there was no trouble; he had not seen any, and he had operated upon hundreds of such cases.

Occasionally in these cases there was effusion, with temperature; he did not now see these cases as no important ligaments were cut, and he did not now suture the aponeurotic coverings and the synovial membrane all the way. If one left a little gap in the aponeurosis and the synovial membrane, then all the joint effusion was exuded into the cellular tissues of the thigh, and hence these patients had a more uniformly quiet, happy and painless convalescence.

To secure a good result in these cases it was best to avoid sending them to the massage department. He had learned, during the war, entirely to avoid the massage department; he had also learned to avoid using a pillow of any sort under the knee-joint after the first twenty-four to thirty-six hours because that was a potent cause of failure to extend the knee completely during convalescence. If a man could not extend the knee fully he could not walk properly.

It was unwise to mention adhesions in the patient's presence, especially if there was any question of compensation. He should be encouraged to move the limb by himself, being watched to see that he straightened the knee properly, taking a long step with the good leg, a short one with the bad leg. Movements could be started in bed at once, and the patient encouraged to walk about on the third day, and move the knee more freely day by day. At the end of a fortnight the full range of movement should be restored; at the end of a month the limb should be as strong and useful as ever before.

Mr. PHILIP TURNER said he thought that there was great difficulty in the diagnosis of these cases, especially when "locking" was not present. Mr. Elmslie had said something with which probably all would agree, namely, the way in which patients were apt to make the diagnosis of displaced semilunar cartilage themselves. Medical men, especially juniors, often made this diagnosis on insufficient grounds. At hospital he had been impressed at the way in which cases of trivial injury were sent to the out-patient department with the diagnosis on the card of displaced internal semilunar cartilage, without sufficient reason. During the war the same tendency was noticed. For some reason, which it was difficult to follow, even trivial injuries were often diagnosed as displaced cartilages and, as had been pointed out, that did a good deal of harm. If a man with a sprain of his knee were told that that was his condition, he would be much happier, and there would be a quicker recovery.

He had been very pleased to hear Mr. Timbrell Fisher speak of the importance of the infra-patellar pad of fat. He (the speaker) had been for a long time convinced that this was an extremely important structure in injuries of the knee-joint. If one saw a patient who, after an injury, could not fully extend the knee—perhaps there was a shortage of extension of 5° or 10°—and when on examination there was seen to be a bulging on each side of the ligamentum patellæ, with little or no excess of fluid in the joint, he thought it likely there was hæmorrhage into that infra-patellar pad of fat. And if there was bulging on each side, there must be a similar bulging upwards into the joint, and probably the same swelling in the pads which Mr. Timbrell Fisher demonstrated. He considered that swelling at the sides of the ligamentum patellæ was of considerable diagnostic importance, and that it should always be looked for in cases of injury to the knee-joint.

Mr. A. G. TIMBRELL FISHER (in reply) said that Members had dealt too kindly with his paper. He had brought forward a number of pathological points for the express purpose of having them discussed. He had been specially anxious to know whether others had encountered adhesions similar to those he showed in the specimens, and whether there had been experience in the treatment of them by manipulation. He thought that in a certain number of cases the adhesions might be congenital, and that



after twisting of the knee they might become inflamed with or without involvement of the pad of fat, and cause almost as much trouble as if they were originally due to inflammation. If others would keep an eye on these matters it would be helpful to their colleagues generally.

With regard to drainage, personally he never used it, as he considered that firm bandaging after operation was a very important preventive of post-operative hæmarthrosis. He also was particular to do the bandaging himself; he did not think it ought to be left to an assistant. At least  $\frac{3}{4}$  in. of wool should be used and should surround the whole joint, the bandage put on firmly, and a certain amount of wool left above and below the latter, so as to prevent undue constriction of the limb. This obviated the need of drainage. He had seen two cases in which drainage had been carried out and in which hernia of the synovial membrane had occurred beneath the capsule. The danger of introducing infection was also obvious.

Reference had been made by one speaker to putting the finger into the joint. He (Mr. Fisher) was strongly against that practice; he thought it was giving hostages to fortune. One could not always be quite certain of the rubber gloves used; there might be a small hole due to a prick of the needle, and through that a concentrated essence of micro-organisms trickled into the joint.

With regard to the transpatellar incision, he would like Members to look at the pathological specimens in their museums, of cases of fracture of the patella which had united some time previously in good apposition. The bone was as a rule firmly united; so also was the lateral part of the articular cartilage, but the central part of the latter hardly ever showed any sign of union. On either side of the fissure in the cartilage was an area of osteo-arthritis, often marked. His experience of cases which had been operated upon by the split-patellar incision was a somewhat unfortunate one. He had two cases which had been operated upon—elsewhere—by the split-patellar incision, in which there was only five degrees of movement in the knee-joint. He agreed, however, that in the hands of the master who originated it, it was probably an excellent procedure.

He also held that one should remove the posterior part of the cartilage, except in those cases where it was clear that the anterior portion only was involved. In the latter case one should remove the anterior part of the cartilage, and leave the posterior part alone.

It was now generally agreed that the commonest lesions to cartilages were circumferential tears and splittings, extending into the posterior part of the cartilage; and in those cases it was advisable to remove the whole cartilage. The difficulty of removing the posterior portion had, he thought, been exaggerated.

## A Scientific Method for Removing Metallic Foreign Bodies.

By DUNCAN C. L. FITZWILLIAMS, C.M.G., F.R.C.S.

EVER since the introduction of X-rays into the practice of medicine, the uses to which they have been put have been so widely extended that they have now become part of the equipment of even the smaller hospitals. In surgery, however, their uses have been restricted, being confined merely to investigation and diagnosis before surgical procedures have been decided upon; uses after this stage have unfortunately been neglected. That is due very largely to the want of co-operation between the radiologist and the surgeon.

During the recent war there was a great opportunity for the radiologist and surgeon to work more closely together; that they did not do so was probably due to want of imagination on the part of the surgeon. Throughout the war the

[January 2, 1924.]

X-rays were used largely, if not entirely, near the front line, for the localization of fragments of metal and foreign bodies lodged in the tissues. These cases occurred by the thousand. Compound fractures need not here be discussed as their treatment was carried out more at the base.

Various methods were used to assist the surgeon in the removal of these foreign bodies. There were stereoscopic photographs, involving the use of two plates, and a special apparatus for viewing them simultaneously. These were good, but expensive, and the apparatus had to be in the theatre, and to some people such as myself, stereoscopic photographs are useless. Localizers were plentiful and all equally faulty and in many cases useless. We gave a fair trial to almost every pattern of localizer in conjunction with enthusiastic advocates of the various patterns, and were easily able to demonstrate by actual measurements the errors in each. Electric instruments combined with stethoscopes were invented and used for a short while; these were pushed about in the tissues on the chance and hope of meeting with the object. None of them seems to have had a long vogue. The obvious and easy method was almost entirely overlooked. An able abstract of the different methods of the localization of foreign bodies was given in the *Practitioner*, March, 1921, pp. 172-182, by Dr. William Cotton. He mentions no less than nine different methods for finding and localizing the fragment, and he advocates a method to be used in conjunction with an atlas of sectional anatomy. The following passage shows what he thought was the best of these methods.

"In the absence of diagrams a very simple example must suffice. In a patient, the distance between whose acromial tips is 18 in., after careful centring and marking we have localized a m.f.b. (metallic foreign body) 3 in. in perpendicularly from the mark on the right side of his back and 6 in. from the middle line at the level of the eighth dorsal vertebræ. In the atlas of proportionately diminished sections the interacromial distance is 8 in. In the plate showing the sectional anatomy of the chest at the level of the eighth dorsal vertebræ, we can show the surgeon on the right side the exact anatomical position of the m.f.b. or other fragment  $2\frac{3}{4}$  in. from the middle line and  $1\frac{1}{2}$  in. from the posterior surface of the body, upon the anatomical diagram itself, as if upon a chart."

In summing up, Dr. Cotton says of this method that it was used experimentally,

"but it impressed me as an immense step forward, at very little extra trouble, in securing the confidence and appreciation of the surgeon."

Later on the writer states that

"successful finding seemed to depend more on the individual surgeon than on the particular method of localization or on the regional position of the m.f.b.,"

a statement with which we are in entire agreement.

The technique of the operation for the removal of a foreign body as usually carried out, and the method pursued in most hospitals at the present day, is, briefly recorded, the following: The patient's skin is marked with a blue pencil in two planes by the radiographer in the X-ray department under the screen. The patient is then taken to the theatre accompanied in most cases by one or more photographs, stereoscopic and otherwise. An incision is made in what seems a convenient place and the finger of the surgeon is thrust blindly into the depths of the wound to grope for the object. When it cannot be found the surgeon gazes at the photograph for inspiration, continues to plough the patient's tissue with his finger and everyone very wisely keeps silence. How often have we all witnessed this scene? Sometimes if the foreign body is

large and the surgeon is lucky it is found, but sometimes the operation is abandoned, after a varying amount of damage has been inflicted, for a similar attempt at a future date. No one can deny that this method is far from satisfactory; it would be difficult indeed to find anyone who would seriously defend so barbaric and unscientific a procedure.

The reasons for its frequent failure are not far to seek. The method is blind; there is nothing to tell us we are getting "hotter" as we approach the object, and nothing to say we are getting "colder" as we pass away from it. A foreign body is difficult to feel when embedded in muscle, it can be so easily displaced by the finger, and this is one reason why localizers are all so useless in practice. Everyone knows the difficulties sometimes encountered in attempting to find an artery or a nerve in a dead body; even when the exact anatomical situation of the structure is well-known, how much more difficult must it be to find something whose position is uncertain.

Now the removal of a foreign body can in reality be made simple, safe, sure and scientific. The method is one merely of applied common sense. Other surgeons may have used it and may have developed it to better uses than we have, but as far as we are aware the method has never been made public in detail. The method is to remove the foreign body by means of an instrument guided by sight and by feeling under the X-rays, the X-ray machine being regarded merely as a surgical instrument, and in war it really becomes this.

Some years before the war we had occasion to remove a bullet from the wrist of a doctor's son, and as it was embedded in the carpal bones, we operated in the X-ray department, and were able to remove it without damage to the tendons. Other operations performed in the same way followed for the removal of needles, &c. After a time an ease and facility of operating was established which was astonishing. The operation had become a simple procedure carried out under the eye, and did away with the old "needle in the haystack" method. This was the commencement of the method which was developed later when the removal of foreign bodies had become a common operation.

During the war I organized several large hospitals, and the same principle was followed in all, namely, placing the X-ray department next the theatre. If possible a large door was made leading from one to the other, so that the patient could be wheeled from one to the other without difficulty and an operation commenced in the theatre could be continued if necessary in the X-ray room. As practically nothing but wounds were examined in the X-ray room, the room was kept as clean as the theatre, and the same regulations were applied to each. In all these hospitals one could operate as readily in the X-ray room as in the theatre. Examinations of chests, lungs, hearts, stomachs, &c., practically were never carried out in these hospitals, the patients needing these investigations being passed to the base.

The technique of the operation as we performed it was rather peculiar and is described as follows: We will take the removal of a foreign body from the thigh, and suppose that the wound of entry has either closed or is so far distant that we need not consider it. The limb is prepared as usual and laid upon a sterile towel, a second sterile towel is laid over the limb, and all the manipulations of instruments take place under this last towel, which separates the field of operation from the fluorescent screen. The light is extinguished, and a moment is spent in accustoming the eyes to the darkness. The screen is laid upon the upper sterile towel, and the rays turned on. The foreign body is then seen. Rubber gloves should always be worn during the whole of the manipulations; even though the hands do not come in contact with the wound,

the gloves act as a protection to the electricity which constantly passes from the patient or the table unpleasantly to the operator when the current is flowing. With the finger, pressure is then made upon the skin of the thigh, and the foreign body will be seen to move under the pressure. The finger is gradually moved about, making pressure intermittently till the point is found where the foreign body makes the greatest excursion in response to that pressure. That is the spot where the foreign body is nearest the finger and will be reached most easily. The spot is easily found very rapidly. The excursion made by the foreign body is sometimes very free, perhaps  $1\frac{1}{2}$  in. in either direction. It is this ease of movement which makes the foreign body so difficult to locate from a photograph or by means of a localizer. The extreme point of movement being found, the finger is kept upon the spot, the rays turned off, and the light turned on. An anæsthetic is now given, in most simple cases gas was found to be quite sufficient if one was proficient in the method. The upper towel is laid back and an incision made into the muscles only large enough for the removal of the piece of metal. The knife is laid aside and the towel replaced again after the introduction of the point of a pair of forceps into the wound. All the subsequent manipulations are performed underneath this sterile towel, which intervenes between the screen on which the objects are seen and the limb. The light is extinguished and the rays set going. Under the guidance of the eye the closed forceps are thrust down to the piece of metal which can be seen to retreat before the point of the instrument in exactly the same way as it will retreat before the much larger finger. If the fragment of metal is large it can be felt distinctly when touched by the instrument, when of small size the contact is judged by the movements of the fragment. If the instrument passes the piece of metal, the fragment is either in front or behind the instrument; the eye prevents it passing to one side. By appropriate movements the position of the metal in front or behind is quickly ascertained. It is at this point that experience and skill are needed. The point of the instrument must be brought into contact with the piece of metal. The fingers outside the limb are often very helpful at this moment. The instrument is opened, thus making a track along which the fragment can be withdrawn and the forceps made to close upon the piece of metal. If the fragment has been in the tissues some time, rotation is most necessary in order to loosen the fibrous grip which the tissues have formed around it. If an attempt is made to withdraw it at once, the forceps may slip, as considerable force may be necessary, and then the process of gripping the object must again be gone through. If the object slips from the forceps when half withdrawn it is very easily caught, as it lies in the track of the instrument. After withdrawal the light can be turned up, and the rays stopped; a stitch is then put in the wound, which is dressed in the usual way.

Nothing is simpler than this procedure, which takes much longer to describe than it does to carry out: in the majority of cases, as has already been stated, it can be conducted under gas. At the same time there is a great amount of knack in its performance, many people being utterly unable to catch hold of the object at their first few attempts. Once facility has been acquired one would never think of performing the operation in any other way.

The obvious conclusion to be drawn from this is that the X-rays should be available in every modern operating theatre. This can easily be arranged when a new military hospital is being planned, but in a civil hospital it is more difficult. In most hospitals, the X-rays are accommodated, far from the theatre, in some

rather inaccessible place, which was vacant when they were installed years ago. It would be impossible to move the department now. Nor would this be suitable in a civil hospital, where such a large part of the work done by the X-rays is in relation to the heart, chest, or alimentary canal, and the room could not be set aside and kept in a condition for surgery. All that is necessary is to have an operating table with an X-ray apparatus under it in the theatre, and have arrangements whereby the theatre can be darkened. In course of time the need for a darkened room might pass, and the surgeon have a sort of box visor with a fluorescent screen fastened to his head; on pulling this down over his eyes he would be in a dark room and would see his object on bending forward to bring the fluorescent screen over the limb. A dark room would only be necessary for teaching and demonstration purposes.

The advantages of the method are so obvious that they scarcely need enumerating. There is a small incision, the tissues are damaged in the least possible degree, the foreign body is not displaced, nothing but sterile instruments enter the wound, and no matter at what depth the fragment lies, it can be removed with certainty. I quote a case (p. 64) in which the fragment lay in the liver  $8\frac{1}{2}$  in. from the surface. The expense is reduced to a minimum which is an advantage in war-time.

There are practically no disadvantages. A shadow, merely, is seen to guide the surgeon, but once facility and confidence have been gained there is no difficulty.

The following typical cases are quoted, in some of which it would have been practically impossible to have removed the fragment by any other method except by extensive operations, great damage to tissues, and in the first case serious risk to the patient's life.

Throughout the campaign many hundreds of foreign bodies were removed by this method which was taught and explained to all surgeons visiting the various hospitals in Malta, Roumania and Russia, and at Queen Alexandra Hospital, Millbank.

*Case I.*—Lieut. C., of the S.B.A.L., aged 22.

Date of wound, December 18, 1918, when he was struck on the right side of the head with shrapnel. Admitted January 2, 1919, with a scar on the right side of his head. His papers showed that he had been operated upon at once and a part of the right temporal fossa removed, as his pulse was 45; no bullet or fragment was found, the frontal lobe was lacerated and the base of the skull fractured. He remained irritable for some time with a slow pulse, no vomiting, and then recovered completely except for a droop in the right eyelid.

X-ray showed a large fragment of metal in the frontal lobe of the left side just across the middle line about 2 to  $2\frac{1}{2}$  in. from the front, just above the sella turcica.

*Operation:* January 28, 1921.—A flap of soft tissues was thrown down from the left temporal region, the bone trephined, the opening enlarged, and the dura opened. The X-rays were turned on and the fragment seen. A probe was passed down direct to the piece of metal. Forceps were passed in, the metal grasped, it slipped from the forceps when half withdrawn but was easily caught again and removed. There was an escape of brain debris after the fragment was removed. *The time from entering the brain to the time the removal was completed was exactly thirty seconds.* The wound healed, the patient was up and about in a fortnight, and later left for the front, where he was killed.

*Case II.*—Private J. J., 339th Inf., U.S.A. Admitted September 30, 1918. Gunshot wound just below and to the outer side of the right patella. The knee

seemed fairly comfortable on admission to hospital, but in a few days became swollen and painful. The wound was dry and healing; there was no exit wound. Under the X-rays a shrapnel bullet was seen behind the patella and apparently fixed between the condyles. It was a little difficult to make certain of its position; no antero-posterior view could be obtained as the knee could not be straightened.

*Operation*, October 5, 1918.—A small incision was made under the X-rays into the joint, avoiding the old wound. Some difficulty was met with in locating the bullet, which was found to be firmly embedded in the inner surface of the outer condyle far

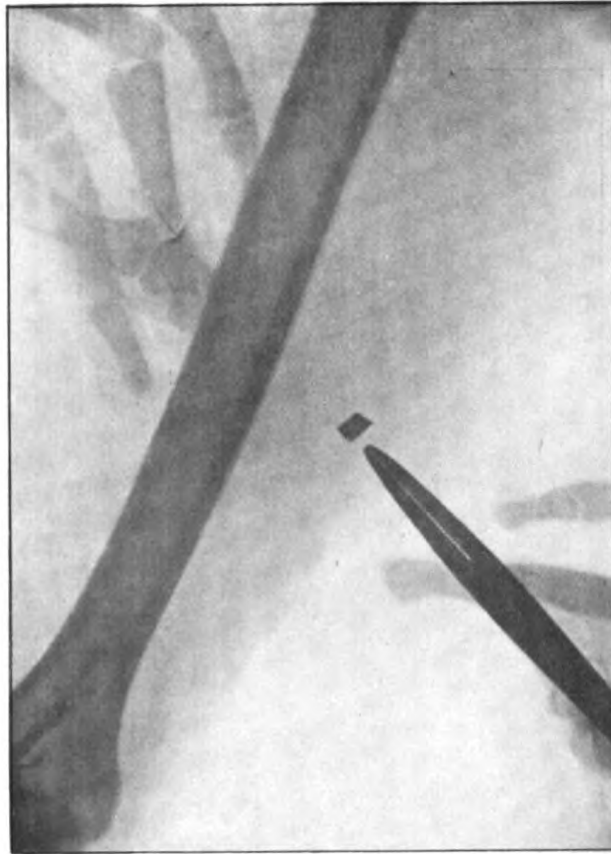


FIG. 1.—Foreign body being removed from arm. Insertion of forceps and location of foreign body.

back. There was difficulty in catching the round shrapnel ball, but eventually this was done, and the bullet removed.

Healing was uninterrupted and recovery perfect.

*Case III.*—Somewhat similar to the last. A rifle bullet had entered the popliteal space and was located embedded deeply in the back of the upper part of the tibia. Under the X-rays a small part of the bullet could be seen protruding from the bone. A pair of forceps was passed in through a small incision, some difficulty was encountered in loosening the bullet, which was firmly embedded for three-quarters of its length in the bone. It was removed with an excellent result.

## 64 Fitzwilliams: *Removal of Metallic Foreign Bodies*

*Case IV.*—Gunner C., R.G.A., admitted to Queen Alexandra Hospital, Millbank, June, 3 1920.

History: A shrapnel wound of the back of the left leg some two years previously. The wound had healed but he complained of the bullet, which he knew was still there.

X-rays showed the round bullet embedded in the back of the tibia some way above the ankle-joint. An incision was made in the usual way and forceps passed in. These failed to hold the bullet as only part of the rounded surface was presented to the grip of the forceps, which always slipped off. A chisel was then passed in, and the bullet divided, and the two halves were then removed under the rays in the usual way.

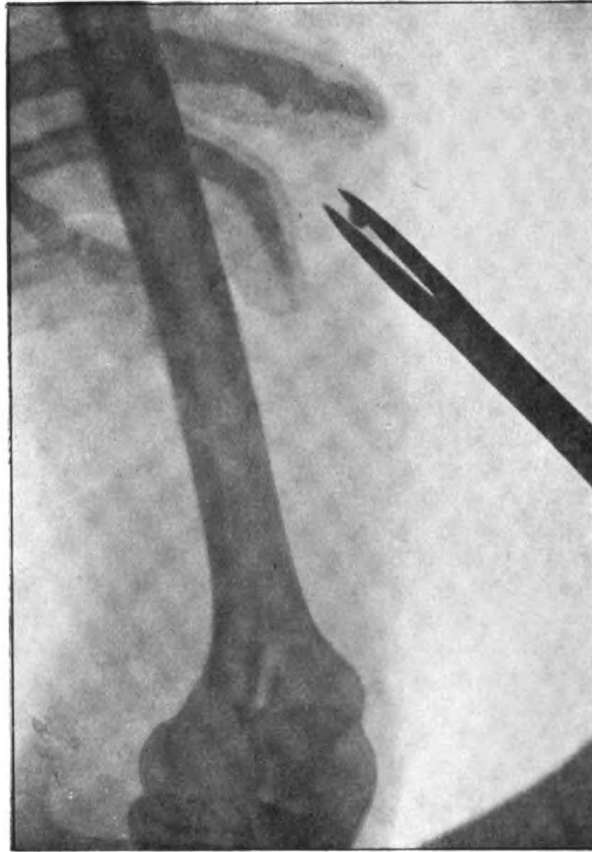


FIG. 2.—Foreign body being grasped by forceps previous to removal. Note the fingers outside the skin.

After removal the two halves showed the markings made by the slipping forceps. The incision needed was almost an inch long.

*Case V.*—L., a pensioner, who had received a shrapnel wound in the left side of the abdomen, was admitted to Queen Alexandra Military Hospital, November 24, 1919, with a sinus which had been discharging ever since the receipt of his wound some two and a half years before.

On examination he was found to have a small sinus on the left flank below the rib margin. A probe could be inserted into this for a distance of about  $8\frac{1}{2}$  in., so that its end was near the upper part of the right lobe of the liver far back, having passed right across the abdomen. As we suspected a foreign body we had him X-rayed but there was no shadow seen. We then screened him, when a small shadow became visible.

It is rare to see a shadow when one is not shown in a photograph. An anæsthetic was given and a pair of crocodile-billed œsophageal forceps were inserted; they were the only instruments of sufficient length to reach the foreign body. The foreign body was grasped and attempts made to withdraw it. It was so firmly embedded in fibrous tissue that the lower jaw was broken from the forceps after the metal had been withdrawn about 3 in. At a subsequent operation with stronger forceps both the foreign body and the jaw of the broken forceps were removed.

It would have been practically impossible to remove this foreign body by any other method. The foreign body was flat, which may have accounted for its not being seen on the photograph, for it completely disappeared on being turned sideways in the grip of the forceps.

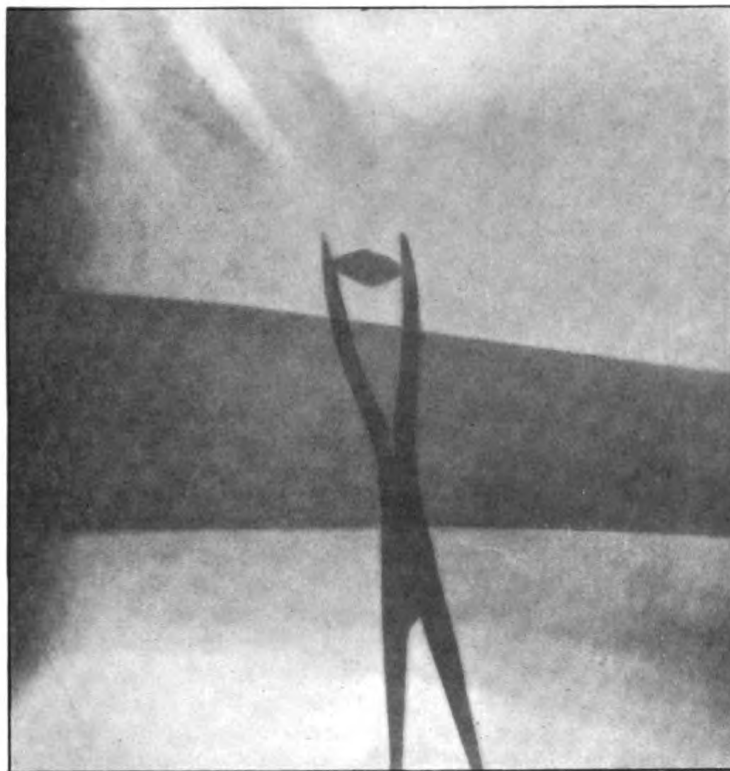


FIG. 3 —Foreign body being removed from the inner aspect of the thigh, through the wound of entrance on the outer side. The fingers outside the limb are almost as important as the forceps inside.

For the excellent photographs used I am indebted to Captain P. Cotter, M.B., B.Ch., R.A.M.C., who was the X-ray officer to the Fifty-third Stationary Hospital at Archangel during the whole time of the North Russian Expedition. He was most helpful in developing the method and bringing it to the high state of efficiency which it reached.





## Section of Surgery.

### SUB-SECTION OF PROCTOLOGY.

President—Mr. ASLETT BALDWIN, F.R.C.S.

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#### **A Case of Carcinoma of the Rectum treated by Local Resection by the Coccygeal Route, after Colostomy, in a Patient aged 72. Subsequent Closure of Colostomy with Complete Restoration of Rectal Function.**

By Sir C. GORDON-WATSON, K.B.E., C.M.G., F.R.C.S.

*History.*—Patient, a female, aged 72, had complained of discharge of mucus per anum for nearly five years, and in 1921 was treated in the Royal Free Hospital for "colitis."

September, 1922: Inoperable carcinoma of the rectum diagnosed. Colostomy performed.

March, 1923: X-ray treatment applied for the rectal growth.

May, 1923: Patient came under my care at St. Bartholomew's Hospital, anxious that something might be done to relieve the copious mucous discharge from the rectum and the severe sacral pain.

On examination of the rectum I found a polypoid growth with an indurated but pedunculated base. The apex of the polypoid mass was ulcerated. The growth projected into the bowel and seemed to fill the lumen. It was attached to the anterior wall, but the attached portion was just beyond the reach of the finger, so that on digital examination the growth felt like an annular growth with some intussusception.

Examination with the sigmoidoscope established the pedunculated nature of the growth. The long history of mucous discharge and the absence of bleeding pointed to the growth having commenced as an innocent adenoma, and the marked induration with fixation at the base, to a malignant change. The diameter of the base was about  $1\frac{1}{2}$  in. and the diameter of the projecting growth about 3 in.

The patient was most anxious to be relieved of her colostomy if possible, and, having regard to her age and her long history, I decided to attempt a local removal with preservation of the pelvic floor and sphincters.

June 4, 1923: An incision was made from just behind the anus to the junction of sacrum and coccyx. The coccyx was removed and the rectum opened from behind in the transverse axis. The growth was pulled out through this opening until the base was fully exposed. The growth was then excised with the whole thickness of the rectal wall, including a clear margin, and the gap left was sutured transversely. The posterior opening in the bowel was sutured round a small tube, which was brought out through the external wound, which was then closed round it. The tube was removed twenty-four hours later. The wound healed rapidly, and stricture was prevented by careful digital dilatation during the healing process.

Section showed columnar-celled carcinoma.

On August 3, 1923, I closed the colostomy and at the same time performed appendicostomy as a safety valve.

At the present time (February, 1924) the patient is in good health and the bowels act regularly with aperients. A fibrous band can be felt in the rectum at the site of resection, but there is no marked narrowing of the lumen. There is a ventral hernia at the site of the old colostomy, but this causes no inconvenience.

*Remarks.*—The following are points of interest in this case: (1) The unusual type of malignant growth in the rectum, a growth which in all probability had commenced as an innocent growth; (2) the umbilicated surface of the polypoid growth, which to the touch at first suggested an annular growth with prolapse and a tight stricture; (3) a method of operative treatment which, though in no sense radical, may well be considered in a patient over 70 years of age, who is strongly adverse to colostomy being done, and who is afflicted with a new growth which, on clinical grounds, may be regarded as one of low-grade malignancy.

### A Case of Streptococcal Ulcerative Colitis Cured by Serum and Vaccine Treatment.

By C. E. DUKES, M.D., M.Sc., D.P.H., and W. B. GABRIEL, M.S., F.R.C.S.

It was evident from the recent Discussion on Ulcerative Colitis at this Sub-Section<sup>1</sup> that considerable uncertainty still exists concerning the bacteriology of ulcerative colitis, many different microbes having been accused of causing this disease at different times. The present unsettled state of this problem makes the following case one of interest, since the patient whose history we are recording was proved by bacteriological investigation to be suffering from a streptococcal infection and he responded with great rapidity to specific treatment.

The patient, a male, aged 23, a "job buyer," living in London. He had never been abroad. On presenting himself at the out-patient department of St. Mark's Hospital, in November, 1923, he gave a history of having passed blood and slime for six months; he had three or four motions a day; he showed no tenderness or pain in the abdomen; he was of a distinctly nervous disposition. He was examined by one of us (W. B. G.) with the sigmoidoscope and a diffuse shallow ulceration over the whole of the rectum and sigmoid was observed. He was admitted to St. Mark's Hospital, on November 3, as an in-patient. He appeared well-nourished; he showed no other signs of disease. His temperature often rose to 100° in the evening.

A series of bacteriological examinations was made by one of us (C. E. D.), both on the fæces and material collected through the sigmoidoscope on several occasions. The fæces were watery in consistence, red in colour, and contained much blood and mucus, together with enormous numbers of pus cells. Repeated examinations showed no pathogenic protozoa. Direct films were made from scrapings from the ulcers and on each occasion these revealed an excess of Gram-positive cocci, many of them phagocyted by polymorphonuclear leucocytes. Following the usual routine adopted at St. Mark's Hospital, the fæces were plated out on a large number of different media in order to obtain

<sup>1</sup> *Proceedings*, 1923, xvi (Sect. Surg., Sub-Sect. Proc.), pp. 91-110.

a comprehensive idea of the bacterial flora as well as to search for specific bacteria known to cause inflammation of this region of the body. No non-lactose fermenting strains were ever isolated and the patient's blood did not agglutinate any of the six dysentery bacilli. Fifty-two colonies of different organisms were selected from the plates at random on different occasions and these were all tested against the patient's serum but without any significant agglutination. Though no evidence was obtained to incriminate any of the bacilli isolated, suspicion fell at once on the streptococcus group, for these were represented in most unusual numbers from the cultures obtained by scraping the ulcers, often more than 50 per cent. of the colonies on the plates being small transparent colonies of streptococci. Blood cultures were negative but on two occasions urine was received from the catheter into trypsinized-ox-heart broth and yielded on each occasion a growth of streptococcus with the same morphology and cultural characters as that of the organisms isolated from the fæces. This was a long-chained streptococcus, non-hæmolytic, growing with difficulty on ordinary laboratory media, but producing characteristic discrete colonies on serum agar. Dr. Mervyn Gordon kindly examined this culture and agreed that it could not be placed in either the *salivarius* or *fæcalis* group: it is apparently one of the unclassified streptococci. Many attempts were made to determine if the patient's serum would agglutinate or cause a precipitin reaction with cultures of this streptococcus, but the difficulties of carrying out such tests with streptococci are considerable and no satisfactory method has yet been found of overcoming them. The majority of the suspensions agglutinated spontaneously: when a stable suspension was at last obtained it was agglutinated by the patient's serum often to as high as 1 in 1,000, but unfortunately some agglutination (though not to the same titre) always occurred with control normal serum. It was felt, however, that sufficient evidence had been obtained to regard this as a case of streptococcal infection, particular weight being given to the evidence supplied by stained films from the ulcers, the large number of colonies of streptococci obtained by culture and the isolation of this organism from two catheter specimens of urine.

So far the patient had received no treatment other than saline irrigations, and the local conditions remained much as before. On November 28 treatment was commenced with antistreptococcal serum obtained from the Lister Institute, and the patient received 40 c.c. in the course of the next ten days. An autogenous streptococcal vaccine was prepared and injected subcutaneously, commencing with 10 million on December 2, the dose being gradually increased to 40 million by December 10. An improvement was noticed immediately specific treatment began. The diarrhœa diminished, the fæces became formed, and the patient seemed better in every way. On December 14 no ulceration was seen by the sigmoidoscope examination, and the contents of the rectum and colon, instead of consisting of blood, pus and mucus, now showed normal fæces. The patient was given an anæsthetic on December 17, and again examined by Mr. Lockhart-Mummery with the sigmoidoscope. No ulceration of the rectum or sigmoid was found, and the mucous membrane was normal in appearance. The patient was discharged from hospital on December 20, twenty-three days after specific treatment was instituted. He was examined again on February 1 of this year, and still remains well, the mucous membrane being normal in appearance.

The result of treatment therefore confirmed the laboratory investigations and led to a satisfactory cure.

**Specimen of Melanotic Sarcoma of the Rectum (with  
Microscopic Slides).**

Shown by H. GRAEME ANDERSON, M.B.E., F.R.C.S.

THE specimen was removed from a male, aged 80, with the following history of nine months' duration: Gradually increasing constipation leading to faecal impaction with loss of sphincteric control and spurious diarrhoea. There was a sensation of a lump in the rectum, and the patient during the later months had felt this with his finger. There had been no hæmorrhage nor prolapse, but there had been a mucoid discharge. For the last three months he had tenesmus, and was losing weight.

Examination at first revealed to the touch a large, firm non-ulcerated tumour, 3 in. by 1½ in., filling the ampulla of the rectum and attached by a fairly broad pedicle, just over the lower part of the prostate. The sphincters were very weak. The bowel above was loaded.

The patient entered a nursing home, and for a week the bowel was slowly emptied by means of enemata.

Under a spinal anæsthetic the tumour and its pedicle were removed by means of a partial Whitehead operation. The cut mucosa above the pedicle was stitched to the anal margin. The sphincters were divided in the mid-line posteriorly to allow delivery of the tumour. The tumour was very dark in colour and it was sent to Dr. Dukes, who prepared the specimen and reported it to be a melanotic sarcoma. Sarcomata of the rectum are rare. For every 200 cases of rectal carcinoma only one case of sarcoma occurs. Those with melanotic change are more common than those not showing this change. They are all very malignant, but in the case recorded by Sir Charles Ball, the patient lived ten years after removal of the rectal sarcoma free from recurrence, and died from acute pneumonia. Not many cases of melanotic sarcoma of the rectum have been recorded in literature. My case is the first of the kind that I have seen in twenty years' experience. It is said that melanotic sarcoma of the rectum is fairly common in horses—especially white horses.

**Specimen of Villous Tumour of Rectum Undergoing  
Carcinomatous Changes.**

By LIONEL E. C. NORBURY, F.R.C.S.

W. C., MALE, aged 60. History of swelling at the anus, fourteen months. Prolapse of tumour, twelve months. Great difficulty in reducing tumour and prolapse recently. No history of constipation nor of rectal discharge. Large velvety tumour of rectum with wide pedicle growing from anterior wall and extending laterally on both sides. Indurated in places. *Sigmoidoscopy*: No other tumours seen in rectum or pelvic colon.

January 21, 1924: Colotomy (pelvic colon).

February 4: Perineal excision of rectum. Growth just below level of peritoneal reflection. Small glands in perirectal tissue. Bowel divided 3 in. above growth.

Microscopical examination of base of tumour: Columnar-celled carcinoma.

**Specimen of Rectal Polyp removed from Boy, aged 11 Years.**

By LIONEL E. C. NORBURY, F.R.C.S.

A. K., MALE, aged 11. History of passing blood with every motion. Prolapse of polyp after each motion. Good control.

*Examination.*—Sphincter active. Anus widely patulous. Polyp seen to be attached by long pedicle to the lowest valve of Houston.

Removal of polyp.

Anus soon regained its normal appearance.



## Sections of Surgery, of Medicine, and of Therapeutics and Pharmacology.

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### DISCUSSION ON "THE TREATMENT OF SEVERE GASTRIC AND DUODENAL HÆMORRHAGE."

AFTERNOON MEETING.

Chairman—Mr. CYRIL A. R. NITCH, M.S. (President of the Section  
of Surgery).

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Mr. HERBERT J. PATERSON, C.B.E.

THE surgical treatment of hæmorrhage from gastric or duodenal ulcers may be discussed in reference to two distinct groups of cases, the sudden acute hæmorrhages, so severe as to threaten life itself, and the chronic or recurrent hæmorrhages, less alarming in extent but by their repetition tending to produce a serious and profound anæmia, which, if unchecked, may ultimately prove fatal.

From a surgical point of view this classification seems preferable to that in which a distinction is drawn between hæmorrhage from a so-called acute ulcer and hæmorrhage from a chronic ulcer, and this for two reasons. First, we do not know that there is such a pathological distinction; the existence of an acute ulcer is purely conjecture, and, secondly, if such distinction there be, we cannot recognize it clinically. The duration of symptoms has no relation to the chronicity of an ulcer as seen in the operation theatre.

#### (I) ACUTE HÆMATEMESIS.

I am well aware of the danger of laying down hard and fast rules. To do so may give those who come after us cause "to reflect on the vanity of received opinion"—nevertheless I have formed very decided views as to the wisdom of surgical intervention in cases of acute hæmatemesis, which I may express in a single sentence. The subject for discussion this evening is the surgical treatment of acute hæmatemesis. There should be no such thing. Operation during the progress of or shortly after a severe gastric or duodenal hæmorrhage is a delusion and a snare, and in my judgment should never be attempted.

Acute hæmatemesis is one of the unappropriated blessings still remaining for the physician. It is not a condition which lends itself to operative measures. The collapse induced by loss of blood is favourable for the arrest of hæmorrhage but unfavourable for surgical intervention. Usually,



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it can be arrested by medical treatment, and if this fail, surgical interference is not likely to help matters. By subjecting such patients to operation, we expose them to added dangers, which may just turn the scale against them, whereas without operation the probability is that they will recover. I will endeavour to give very briefly reasons for the faith which is in me.

In discussing this subject two questions must be answered: (1) What is the mortality rate from hæmorrhage in cases treated medically? (2) What is the mortality rate in the cases treated by immediate operation?

(1) What is the mortality rate from hæmorrhage in cases treated medically?

Most clinicians are agreed that the mortality rate from hæmorrhage in cases of gastric and duodenal ulcer treated medically is under 5 per cent. In the four series of cases given in Table I the mortality rate is 3·8 per cent.

TABLE I.

| Recorder          | Number of cases |       |     |     | Mortality rate |
|-------------------|-----------------|-------|-----|-----|----------------|
| Lenhartz ...      | ...             | 201   | ... | ... | 3·0 per cent.  |
| Ewald... ..       | ...             | 166   | ... | ... | 4·8 "          |
| Wirsberg ...      | ...             | 320   | ... | ... | 5·9 "          |
| H. P. Hawkins ... | ...             | 556   | ... | ... | 0·7 "          |
|                   |                 | 1,243 |     |     | 3·8 "          |

(2) What is the mortality rate of the cases treated by immediate operation?

The last published statistics I can find are those of Lindberg.<sup>1</sup> He tabulates eighty-three cases with thirty deaths, a mortality rate of 36 per cent. If I relied on these statistics doubtless the objection would be raised that they are out of date, and that with more modern technique and especially by the use of blood transfusion, the results of operation are far better at the present time. I am not clear that there is ground for this optimism, but at any rate the results of surgical treatment are still indubitably inferior to the results of medical treatment. Through the courtesy of the surgical registrars of some of the London hospitals I have obtained some statistics as to operation for acute hæmatemesis in recent years and they are shown in Table II. I should like to take the opportunity of thanking most sincerely those who have helped me.

TABLE II.—OPERATION FOR ACUTE HÆMATEMESIS TREATED BY IMMEDIATE OPERATION.

| Number of cases | Deaths |   |     |     | Mortality rate |
|-----------------|--------|---|-----|-----|----------------|
| 19 ... ..       | ...    | 7 | ... | ... | 36·8 per cent. |

Obviously it is not fair to compare these figures with the mortality rate from hæmorrhage in all cases of ulcer whether they bleed or not. They must be compared with the mortality rate in severe cases of hæmatemesis. Hæmorrhage occurs in at least two-thirds of all cases of gastric ulcer, and is severe or recurrent in about 30 per cent. Calculated on this basis the mortality rate of those cases in which there is severe or recurring hæmorrhage treated medically would be about 11 per cent. I think this is probably an exaggeration but we shall be well within the mark in stating that of the patients who bleed severely or bleed repeatedly and are treated medically, not more than one in nine will die, while of those treated by immediate operation, one out of three will probably die. No doubt some surgeons by superior skill, aided by a considerable share of luck, may be able to show much better individual results than these. I say "aided by luck," for we must bear in mind

<sup>1</sup> H. Lindberg, *Nordisk Med. Archiv.*, 1914, xlvii.

that in a given case it is impossible to predict whether we shall be able to find, let alone to deal with, the source of the bleeding. It is inevitable that in a certain proportion of cases the patient will have the added risks of an operation without any effective treatment of the bleeding point. In at least three of the successful cases in Table II, no definite source of bleeding was discovered and nothing further could be done. The operation was purely exploratory and therefore an unnecessary complication to the patient's recovery.

I think this is what we should expect, as I am convinced that bleeding from the ulcer itself is less common than is imagined. In duodenal ulcer especially, the source of the bleeding is a general oozing from the mucous membrane of the stomach—the whole surface appears to be weeping blood—and I have in some cases cut sections of the mucous membrane which showed the blood cells escaping between the epithelial cells of the mucosa. But apart altogether from statistics, I am convinced from my own experience that the treatment of acute hæmatemesis by medical means is the wiser and safer course. Up to the end of the year 1921 I have had under my care or jointly with my colleague, Dr. Soltau Fenwick, forty cases in which hæmatemesis was of such severity that the patients were on the brink of the grave. All of them recovered with medical treatment, indeed I have yet to see a death from hæmatemesis from a gastric or duodenal ulcer. In all of them operation was delayed for two or three months until the patient had recovered from the effects of the hæmorrhage, for I confess I have never yet operated during the course of a hæmatemesis. These cases are tabulated in Table III.

TABLE III.—ACUTE HÆMATEMESIS.

|                      | Number of cases | Hæmatemesis only | Melæna only | Hæmatemesis + melæna | Operation            | Recovered from operation | Recurrence of hæmorrhage |
|----------------------|-----------------|------------------|-------------|----------------------|----------------------|--------------------------|--------------------------|
| Gastric ulcer        | 15              | 9                | 0           | 6                    | 14 G.J. <sup>1</sup> | 14                       | 0                        |
| Duodenal ulcer       | 20              | 6                | 0           | 14                   | 20 G.J. <sup>1</sup> | 20                       | 1 <sup>2</sup>           |
| Gastro-staxis        | 4               | 4                | 0           | 0                    | 4 G.J. <sup>1</sup>  | 4                        | 0                        |
| Gastro-jejunal ulcer | 1               | 0                | 0           | 1                    | Excision             | 1                        | 0                        |
| Totals               | 40              | 19               | 0           | 21                   | 39                   | 39                       | 1                        |

<sup>1</sup> Average interval between hæmorrhage and gastro-jejunostomy = three months.

<sup>2</sup> Had slight hæmatemesis two months after. No further hæmorrhage. Died three years later.

All the patients were treated medically in the first instance and, in all but two, gastro-jejunostomy was performed at a later date. Of the two exceptions, one was a nurse who was anxious to avoid an operation. She was kept in bed for six months, and eventually made a complete recovery and had no further hæmorrhage. The other case was one of gastro-jejunal ulcer, in which the gastro-jejunal ulcer was excised and a new anastomosis performed.

Four of the cases are labelled gastro-staxis, as at the operation I could find no ulcer, and I think that the hæmorrhage was probably secondary to a diseased appendix. These were among my very early cases, and I admit that the performance of gastro-jejunostomy in these cases was a totally unnecessary complication of appendicectomy.

In only one of these patients was there any recurrence of hæmorrhage. This patient had a slight hæmatemesis two months after operation; there was no subsequent recurrence up to the time of her death three years later from causes unconnected with her gastric condition. One of the patients was re-admitted into hospital a year after his discharge in a moribund condition from a perforated jejunal ulcer. Thus the immediate operative mortality in this series was nil, and all but two of the patients were apparently cured.

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It cannot be emphasized too strongly that patients rarely die of a first hæmorrhage; it is the repetition of the hæmorrhage which is of such serious import. Let us beware of the specious but fallacious argument which I have heard put forward in connexion with these cases: "This patient is bleeding. He will probably die. Something must be done." Even admitting that the premises are true, I see no logical or other reason for the conclusion that, because death is impending, something should be done which will probably make the patient's life shorter still. The case is quite different in acute intestinal obstruction, in which without surgical intervention death is inevitable, and so operation is justifiable even in desperate cases. In hæmatemesis, on the other hand, operation may be the last straw for a patient who otherwise might recover.

As regards the medical treatment of acute hæmatemesis, the important points are:—

(1) Rest in bed; and by this I do not mean what often passes as rest. The patient should be kept *absolutely* still in bed, no movement even of the arms or legs being permitted, and should be fed by hand with a minimum of disturbance.

(2) Tripier's hot-water injections by the rectum, saline at a temperature of from 120° F. to 130° F. being used.

(3) An ice pack to the abdomen.

(4) Nothing should be given by the mouth for at least four days and then feeding should be commenced with teaspoonfuls of iced milk and egg (Lenhartz). This amount is gradually increased and the patient is kept on milk diet for two months, when some diluted beef essence may be added to the dietary.

In my experience it is not safe to commence Lenhartz's diet directly after a hæmorrhage; sometimes I have seen recurrence apparently brought on by too early feeding. Saline injections, with  $\frac{1}{2}$  oz. of glucose to the pint, are commenced soon after the hæmorrhage has ceased. So-called rectal feeding is starvation except so far as fluids and salts are concerned, so that injections of beef-tea, milk and the like, are valueless.

(5) Small doses of morphia are valuable, although some clinicians object to the use of morphia on the ground that its use may prevent contraction of a bleeding artery. I imagine this objection to be academic rather than practical.

(6) The subcutaneous injection of a 10 per cent. solution of sterile gelatine in 40 c.c. doses repeated daily.

(7) If vomiting or hæmatemesis continue the stomach should be washed out very gently with warm water, and after the lavage a dose of crystalline bismuth subnitrate should be given through the tube.

It used to be taught that in hæmorrhage from the so-called acute ulcer, subsequent operation is unnecessary unless the bleeding recur. If we could be certain that the ulcer which bleeds is an acute ulcer this teaching would be sound, but as we cannot make such a pathological distinction the only safe rule is that an acute hæmorrhage requires surgical treatment to prevent recurrence. As to the time of the operation, the interval between the hæmorrhage and operation should be at least three months, and if the patient is still anæmic transfusion of blood before operation is a valuable adjuvant.

#### (II) CHRONIC OR RECURRENT HÆMATÉSIS.

As regards the treatment of the chronic or recurrent hæmorrhages operation should, as a rule, be deferred for two or three months after the last hæmor-

rhage according to the condition of the patient, medical treatment being carried out in the meantime—rest in bed, Lenhartz diet, saline and glucose injections by the rectum, and gastric lavage. It must be borne in mind that in large excavating ulcers septic processes in the ulcer may play a considerable part in causing the hæmorrhage, so that it is important not only to rest the stomach but to keep it clean. Usually these measures can be relied on to prevent recurrence, but even if the hæmorrhage be repeated I would persevere with medical treatment. If in spite of absolute rest there is further recurrence, the question of operation may be considered, but—and this is the important point—absolute rest must have been tried and have failed. Although personally I have never had to depart from my usual practice of delaying operation for two or three months, if in spite of a thorough trial of medical treatment there were many small losses of blood or more than one hæmorrhage of such severity as to cause obvious anæmia, I should not hesitate to operate before the onset of another attack. But my experience is that efficient medical treatment does not fail. If, however, operation be decided upon it should be preceded by blood transfusion.

There will probably be considerable difference of opinion as to the nature of the operation which should be performed for recurrent hæmorrhage. Various procedures have been adopted, such as ligature of arteries, gastro-jejunostomy, resection of the ulcer, cauterising the ulcer combined with gastro-jejunostomy, and partial gastrectomy. Personally, I still pin my faith to gastro-jejunostomy; so far it has not played me false.

It would be outside the limits of this discussion to compare the relative merits of gastro-jejunostomy and partial gastrectomy in the treatment of ulcer, but this much may be said, that hæmorrhage is not an indication for resection. If more than gastro-jejunostomy be considered advisable, infolding the ulcer or cauterizing it by the Donald Balfour method is safer and equally as efficacious as resection. Resection has its place in gastric diseases as in disorders in other parts of the body, but among thoughtful surgeons there is, undoubtedly, a feeling that this practice is being carried too far. "If your colon offends you—cut it out." If the appearance of the gall-bladder suggests a strawberry—cut it out; some surgeons advise removal of a considerable portion of the stomach even for a duodenal ulcer. It would seem as if the perfect man of the future will be one who has had all his removable organs cut out and all his irremovable organs stitched in. There is bound to be a reaction against this fashion of "cutting out" when simpler means may suffice.

Some object to gastro-jejunostomy on the ground that it is an indirect method of treating hæmorrhage. Doubtless, to deal with the bleeding point is the ideal, but it is rarely practicable. That we must look our enemy in the face is an aphorism that sounds well, but one which, if applied to the treatment of gastric hæmorrhage, may lead to injudicious surgery. In the great majority of cases, gastro-jejunostomy may be relied upon to prevent further hæmorrhage from a gastric or a duodenal ulcer; it is the simplest surgical procedure to adopt, and it has the great advantage that we start with a definite plan in view, and so are not tempted to do an extensive exploration which may just turn the scale against the recovery of the patient. If it fail it is time enough to adopt more drastic measures. In some, at least, of the cases in which gastro-jejunostomy has been unsuccessful the suggested alternatives would prove equally ineffective. I recall the case of a lady on whom I performed gastro-jejunostomy for duodenal ulcer with recurrent attacks of hæmatemesis. Some years after the operation she had several recurrences. Believing the duodenal

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ulcer was unhealed I reopened the abdomen. The ulcer was completely healed, but as a precautionary measure I infolded it. She continued to have attacks of hæmatemesis at intervals, and some years later I again opened the abdomen and explored the interior of the stomach but could find nothing to account for the bleeding. She still has attacks of hæmatemesis and melæna occasionally, which, I take it, are of intestinal origin, and so would not be cured by gastrectomy.

I have had under my care or jointly with my colleagues, Dr. Fenwick and Dr. Porter Parkinson, eighty-four cases in which there were recurrent attacks of severe hæmatemesis. These are tabulated in Table IV.

TABLE IV.—CHRONIC OR RECURRENT HÆMORRHAGE.

|                                  | Number<br>of<br>cases |     | Hæmat-<br>emesis<br>only |     | Melæna<br>only |     | Hæmat-<br>emesis +<br>melæna |     | Operation<br>done |     | Recovered |     | Died |     | Recur-<br>rence |
|----------------------------------|-----------------------|-----|--------------------------|-----|----------------|-----|------------------------------|-----|-------------------|-----|-----------|-----|------|-----|-----------------|
| Gastric ulcer ..                 | 26                    | ... | 22                       | ... | 4              | ... | 0                            | ... | G.J.              | ... | 25        | ... | 1    | ... | 0               |
| Duodenal ulcer                   | 52                    | ... | 32                       | ... | 4              | ... | 16                           | ... | G.J.              | ... | 50        | ... | 2    | ... | 6               |
| Gastric + Duo-<br>denal ulcer... | 4                     | ... | 4                        | ... | 0              | ... | 0                            | ... | G.J.              | ... | 4         | ... | 0    | ... | 0               |
| Gastro-jejunal<br>ulcer          | 2                     | ... | 2                        | ... | 0              | ... | 0                            | ... | Excision          | ... | 1         | ... | 1    | ... | 0               |
| Totals                           | 84                    |     | 60                       |     | 8              |     | 16                           |     | 84                |     | 80        |     | 4    |     | 6               |

Four deaths—One from exhaustion; one from pulmonary embolus; two from pneumonia.

In eighty-two of these the hæmorrhage was due to a gastric or duodenal ulcer and in all of these gastro-jejunostomy was performed. There were three deaths in this series, so that adding the thirty-eight cases of gastro-jejunostomy given in Table III there were three operative deaths in 120 operations, a mortality rate of 2·5 per cent.,<sup>1</sup> which although rather higher than the death-rate of gastro-jejunostomy in an ordinary series of cases, cannot be considered high in view of the condition of some of the patients. Six of the patients had some recurrence; to one of these I have already alluded. Another patient had phthisis, of which he died five years after operation, and it was not certain whether he had hæmoptysis or hæmatemesis, or both. In only one of the remaining four patients was there more than one slight attack. So that in the whole series of cases, 2·5 per cent. died from operation, in 4·2 per cent. there was a recurrent hæmorrhage of a trivial character, and in 90 per cent. no further hæmorrhage occurred after the operation of gastro-jejunostomy. I submit that these figures justify the treatment adopted.

### CONCLUSIONS.

(1) Hæmorrhage from gastric and duodenal ulcers may be considered in reference to two distinct groups of cases, the acute hæmorrhages and the chronic or recurrent hæmorrhages.

(2) Surgical measures have no place in the immediate treatment of acute hæmorrhage.

(3) In the treatment of gastric or duodenal hæmorrhage *absolute* rest is a most important factor.

(4) When the patient has recovered from the resulting anæmia, operation should be performed to prevent recurrence. The interval between the hæmorrhage and operation should be at least three months.

<sup>1</sup> Up-to-date mortality rate in hæmorrhage cases is 2·9 per cent.

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(5) In the first instance the chronic or recurrent hæmorrhages should be treated medically, operation being performed when the patient's condition is favourable.

(6) Regular gastric lavage pending operation is an important detail of the medical treatment of the chronic or recurrent hæmorrhages.

(7) As regards operative measures, gastro-jejunostomy is the simplest and safest procedure, and in the great majority of cases may be relied on to prevent recurrence of the hæmorrhage.

(8) Careful and prolonged medical treatment *after* operation is equally as important as medical treatment *before* operation.

(9) With this combination of medical treatment followed by gastro-jejunostomy the mortality rate from hæmorrhage in gastric or duodenal ulcers should not exceed 4 per cent., and freedom from recurrence may be expected in 90 per cent. of the cases.

The treatment of hæmatemesis is one of the most absorbing and fascinating problems of medicine—absorbing, from the satisfaction it affords when we see a patient literally snatched from the very jaws of death, and fascinating because it demands supreme courage—courage to perform that most difficult task of waiting and watching. When by the bedside of a patient so blanched by hæmorrhage that death is painted on her face, whose flickering pulse is witness to the slender thread on which life hangs, it is not easy to resist the insistent question of the relatives: "Cannot something more be done?" To answer in the negative requires no little courage. It is easy to do something, but if that something be an operation, we shall probably have an opportunity of wondering at leisure what would have happened if this operation had not been done.

Inaction is often the wisest and only safe course, but it is always the more difficult. Anyone can try to do something; but it is the strong man who refuses to be tempted into taking risks which he believes can serve no useful purpose.

In conclusion, if I may be permitted to offer one word of advice to my younger hearers, it is this: When called upon to treat a patient with hæmatemesis, take your courage in both hands and trust Nature. Be assured that if you trust her and assist her she will not fail you, and your reward will be that which always seems to me the greatest satisfaction that the practice of our art can give, the knowledge that you have steered a frail bark back to safety without encountering the perils incidental to the handicraft of our art.

Sir WILLIAM WILLCOX, K.C.I.E., C.B.

Severe gastric and duodenal hæmorrhage is due to so many different causes that no routine method of treatment can be safely laid down. Though gastric and duodenal ulceration are responsible for many of the severe cases it would be quite unsafe to assume in a severe case of hæmorrhage that the treatment to be adopted should be that for a bleeding septic ulcer. Each case must be very carefully considered as regards its ætiology, and an opinion formed as to the probable cause of the hæmorrhage. The treatment adopted should be based on this provisional diagnosis.

Some of the most severe cases of gastric and duodenal hæmorrhage that I have met with have been due to such causes as cirrhosis of liver, splenic anæmia, gastric erosions, gastrostaxis and chronic toxæmic conditions, and

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in these cases the treatment usually adopted for a bleeding septic ulcer would often be contra-indicated, and might be harmful or even dangerous to life.

Certain general principles of treatment may be laid down which are applicable for all cases:—

### (I) "REST" OF THE STOMACH AND DUODENUM.

In severe cases the stomach and duodenum become filled with blood, which rapidly clots, and the emptying time of the stomach will probably be greatly retarded owing partly to the character of its contents, which may form a large, firm, tenacious clot, and partly to the delayed peristaltic action, the result of shock from the hæmorrhage, and of the subsequent treatment.

It will, I think, be generally agreed that nothing should be given by the mouth in the first two or three days following the hæmorrhage. The stomach is probably full of blood, more or less partially digested, and until this is evacuated from the stomach either by peristaltic action or by vomiting, it is useless to attempt giving food or medicine by the mouth.

After severe gastric hæmorrhage it is, in my opinion, advisable to withhold everything *per os* for four or five days, and give normal saline *per rectum*, about 15 oz. every six hours, to which 4 per cent. of glucose may be added with advantage. A daily enema is given to clear the rectum.

Are local applications to the epigastric region of any value? An ice bag partially suspended to prevent undue pressure is often employed in the epigastric region. Is it of any value in controlling gastric and duodenal peristalsis, and does it have any local hæmostatic effect? I very much doubt it.

### THE USE OF MORPHINE AND ITS DERIVATIVES.

Morphine is undoubtedly a powerful remedy in controlling peristalsis, and it is commonly given hypodermically in the acute stage of bleeding. A word of caution is necessary regarding its use.

In cases in which the hæmorrhage is due to hepatic cirrhosis, it must be remembered that patients suffering from cirrhosis of liver, whether this is due to alcohol, syphilis or other cause, are strongly susceptible to the toxic effect of morphine and its derivatives, and an ordinary dose of  $\frac{1}{4}$  gr. may in such patients be fatal. I have seen this dose of morphine cause death in a patient suffering from hæmatemesis due to hepatic cirrhosis. I have no doubt that cirrhosis of the liver, from whatever cause, is a contra-indication to the use of morphine.

Where chronic nephritis is present the same contra-indication holds good.

Also some patients, apart from liver and kidney disease, are very intolerant of morphine and its derivatives and in such the use of these drugs is contra-indicated. It cannot be too strongly insisted upon that the giving of morphine hypodermically in cases of gastric and duodenal hæmorrhage as a routine practice should be discouraged. Before giving these drugs the question should be asked: Is there any evidence of hepatic or renal insufficiency?—and if the answer is in the negative then it is justifiable to give morphine.

### (II) THE PROMOTION OF THE COAGULATION POWER OF THE BLOOD.

This is of value in the control of the bleeding.

(a) *Calcium salts* given by mouth are contra-indicated for the reasons given. It is doubtful whether they are absorbed rectally. The most rapid

effect is obtained by giving—as advocated by Vines and Grove—1 gr. of calcium chloride dissolved in 100 minims of distilled water intramuscularly into the buttock. This dose may be repeated in three or four days if required. I have found this method of administering calcium most valuable in cases of hæmorrhage and adopt it as a routine practice. Parathyroid gland gr.  $\frac{1}{10}$  twice daily may be given by mouth in addition. Calcium chloride solution may also be given intravenously. I think, however, the intramuscular method of administration is the safest and most efficacious. There is no local pain when the strength of the solution does not exceed 1 per cent. Collosol calcium has been used by intramuscular injection with the same object as calcium chloride. I do not think, however, that the effect in controlling hæmorrhage is so powerful.

(b) *Serum injections* such as 20 c.c. of horse serum, or other serum preparations on the market, given subcutaneously, appear to have the power of increasing the coagulation power of the blood and so controlling gastric and duodenal hæmorrhage. Where a toxic cause is suspected anti-streptococcic serum (which is usually horse serum) should be given in doses of about 20 c.c. The likelihood of the occurrence of serum fever and a serum rash from three to ten days after administration must be borne in mind, also the possibility of an anaphylactic reaction.

(c) *Blood transfusion.* A blood transfusion from a suitably tested donor of amount about 500 c.c. not only has a great value in counteracting collapse from gastric and duodenal hæmorrhage, but appears to be of definite value in arresting hæmorrhage by promoting coagulability of the blood.

(d) *Inhalations* of washed carbon dioxide gas given by means of a glass funnel attached to a Kipp's apparatus for CO<sub>2</sub>, have been recommended for promoting the coagulation power of the blood. I have used this method of treatment in several cases, but have not been impressed by its value.

### (III) "LOCAL HÆMOSTATICS."

In the acute stage of gastric and duodenal hæmorrhage it is doubtful whether these are of any value. When one remembers that the stomach is full of partially altered blood, the administration of local hæmostatics such as tannic acid, perchloride of iron, acetate of lead, alum, turpentine, &c., simply causes a coagulation and hardening of the already effused blood and renders the gastric contents still more irritating to the bleeding surface. Are they of any value at all? I think not.

Adrenalin chloride by mouth has been recommended. To obtain efficient action it would be necessary to wash out the stomach and remove the sanguineous contents before introducing the adrenalin chloride solution. Is not this a procedure fraught with danger when bleeding is continuing in a collapsed patient? I have not yet ventured to adopt this line of treatment. I have given adrenalin chloride by the mouth in gastric hæmorrhage without previous gastric lavage, and have not been convinced of any beneficial effect.

### GASTRIC AND DUODENAL ULCER.

The recent papers of Moynihan and others have shown conclusively that by far the greater number of cases of severe hæmorrhage result from chronic ulcers, and this fact in itself is an *a priori* argument in favour of surgical as opposed to medical treatment.

A diagnosis of the cause of the hæmorrhage is usually arrived at from a careful consideration of the history and the symptoms and physical signs.



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The character of the vomit and stools will usually give an indication of the ætiological condition. An acid, dark-coloured vomit (coffee grounds), with possibly free HCl present, is strongly suggestive of this cause, as also is the tarry nature of the stools.

In the acute stage morphine may be given freely, e.g.,  $\frac{1}{2}$  gr. hypodermically and repeated as required. If the hæmorrhage is the first one and there is only a short history of weeks or a few months of gastric or duodenal symptoms, careful medical treatment may be adopted with a view to recovery from the hæmorrhage and to the healing of the ulcer.

Where there is a history of symptoms extending over many months or years it is, in my opinion, inadvisable to attempt medical treatment as a cure of the condition. The patient should be carefully treated for the hæmorrhage on the lines laid down and then put on to a course of Lénhartz's diet and operative treatment carried out when the general condition permits.

In some cases of severe hæmorrhage where the hæmoglobin is below 40 per cent. there is a tendency for recurrences of bleeding, and it is often an advantage to accelerate the recovery of the patient by operating after the shock of the hæmorrhage has been recovered from; a transfusion of 1 pint of blood from a suitable donor being given at the commencement of the operation. I have adopted this procedure in several cases of gastric and duodenal hæmorrhage recently with excellent results.

### HÆMORRHAGE FROM CIRRHOSIS OF THE LIVER.

In such cases the acute stage should be tided over by appropriate treatment. Investigations should be made to determine the cause of the cirrhosis, whether alcoholic, syphilitic or toxic, and appropriate treatment for the hepatic condition carried out. Surgical treatment is contra-indicated.

### GASTRO-STAXIS AND GASTRIC EROSIONS.

*Causes due to Chronic Toxæmias.*—The acute stage of the hæmorrhage should be tided over and investigations made to determine the ætiological factor responsible. In most cases I believe these cases arise from some toxic cause and careful investigations of the teeth by X-ray examination, &c., of the tonsils, naso-pharynx and intestinal tract should be carried out. The toxic factor should be dealt with by appropriate treatment and recurrences of hæmorrhage will thereby be avoided.

### SPLENIC ANÆMIA.

Severe cases of gastric hæmorrhage are not uncommon from this cause. After the acute stage has been tided over splenectomy is the only treatment likely to give permanent benefit.

### MALIGNANT DISEASE.

In my experience severe hæmorrhage from this cause is very uncommon but when it occurs control of the bleeding is most difficult.

In a recent case under my care, of growth of the ascending colon, the patient, while awaiting operation, developed a severe hæmatæmesis which was entirely uncontrollable by medical measures and the patient was never in a condition to undergo operation. In this case the evidence pointed to a secondary growth involving a large vessel in the cardiac portion of the stomach.

The severe anæmias, such as pernicious anæmia, leukæmia, hæmophilia, &c., sometimes are associated with severe gastric hæmorrhage. In such cases medical treatment on the lines laid down is indicated. Surgery is of no avail. Blood transfusion is of value but in the case of leukæmic conditions it must be remembered that blood transfusion may be followed by a rapidly fatal thrombosis; this recently occurred in a case under my care.

In conclusion, from the medical standpoint of the most important and urgent condition of severe gastric and duodenal hæmorrhage, I would emphasize the necessity of accuracy of diagnosis as to the causal condition, and of utilizing every means of investigation which is available compatible with the condition of the patient.

In many of the cases the appropriate treatment lies both in the domain of medicine and surgery, and the best interests of the patient are attained by consultations in these two departments of medical science.

Mr. A. H. BURGESS

said he would consider separately post-operative hæmorrhage from the stomach or duodenum, and that arising from disease quite independently of any operative procedure. Post-operative hæmorrhage was further divided according to whether it followed operations upon the stomach or duodenum or whether upon some other part of the body. In the former case prevention was more satisfactory than cure and he laid stress on the importance of two points of technique: (1) All large vessels seen to approach the line of incision of the stomach should be under-run with a suture and ligatured; (2) the inner continuous layer of suture, taking up all the coats, should be closely applied, the needle passing ten times to the inch, and drawn tightly. After gastro-enterostomy or partial gastrectomy, if the patient vomit, the vomited matter will always contain blood more or less altered; usually it is already dark-brown in colour by the time of the first vomit. If hæmorrhage is continuing the vomit will be of a brighter red, and should this bright-red colour persist and be associated with a rising pulse some treatment should be adopted. He had seen only three cases that caused him anxiety on the score of immediate post-operative hæmorrhage and in each of these the hæmorrhage had ceased after employment of a method which he thought was first advocated by Rodman—the introduction of very hot water into the stomach. The stomach was first washed out with warm water to clear it of any clots. Eight ounces of water at a temperature of 130° F. (the temperature must be accurately measured) were then introduced through the tube, allowed to remain one minute, then syphoned off: this was repeated thrice, except that the last 8 oz. were left in the stomach. The only fatal case he had had was after gastro-enterostomy for irremovable carcinoma of the pylorus; in this case, on the fifth day the patient suddenly began to vomit blood profusely and died very shortly afterwards. A post-mortem showed the main stem of the pyloric artery had been eroded in the floor of the malignant ulcer.

Post-operative hæmorrhage after operations elsewhere than on the stomach or duodenum was rare, but had been met with after operations on almost all of the abdominal viscera and even on other regions of the body. The mortality was high—about 55 per cent. (Phifer, *Surgery, Gynecology and Obstetrics*, January, 1923). The ætiology was obscure, but Rodman's view of its septic origin was that most generally accepted. The treatment of the hæmorrhage

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was essentially medical, and surgical measures should be confined to eradication of the septic focus, if possible.

Hæmorrhage from the stomach or duodenum other than post-operative might be grouped according to whether or not it is associated with any gross local lesion of either of these viscera. Profuse hæmorrhage might occur in the absence of such gross lesion ("gastro-staxis") His first experience of this occurred twenty years ago when he was summoned with a view to operation upon a man, aged 42, with very profuse hæmorrhage from what was thought to be chronic gastric ulcer. The patient died from the hæmorrhage a few minutes after he arrived at the house. Post-mortem, although the mucous membrane of the whole of the œsophagus, stomach and intestines was carefully searched with a lens no gross breach of surface could be detected to explain the terrific hæmorrhage. The only lesion found was a slight cirrhosis of the liver, but there were none of the dilated veins at the lower end of the œsophagus which were the usual site of hæmorrhage in cirrhosis of the liver. This case induced him to believe in the reality of "gastro-staxis." Since then he had three times operated for severe hæmatemesis without finding any gross lesion in the stomach or duodenum.

*Case I.*—Male, aged 40. Operation during third attack of profuse hæmatemesis. Laparotomy disclosed no palpable lesion of stomach or duodenum. Stomach opened and emptied of blood and clots. No ulcer discovered, but the mucosa oozed blood wherever it was touched. Gastro-enterostomy performed as an "indirect" method of arresting hæmorrhage. Hæmorrhage continued, and patient died two days later. At post-mortem examination no gross lesion was found to account for the hæmorrhage.

*Case II.*—Female, aged 32. No previous history of "indigestion." Two previous attacks of hæmatemesis within four weeks. Was admitted during third and most severe attack. Laparotomy failed to disclose any palpable cause for the hæmorrhage. The stomach was not opened. Recovery. This operation was in 1918, and when last heard of, twelve months ago, she still had occasional attacks of hæmatemesis, though less severe.

*Case III.*—Male, aged 46. Three previous attacks of severe hæmatemesis with enlarged spleen; considered to be a case of splenic anæmia. Operation in fourth and most severe attack. No gross lesion felt in stomach or duodenum; stomach not opened. Enlarged spleen, weighing 24 oz., removed. Recovery. Two slight attacks of hæmatemesis during the succeeding eighteen months, then death from influenzal pneumonia.

G. E. Armstrong, of Montreal (*Surg., Gynecology and Obstetrics*, April, 1922), in recording a case of his own very similar to the first of these three cases, mentioned ten cases operated on by the surgeons of Montreal during the previous twelve years, all ending fatally. Two more recent cases were not submitted to operation, but were treated by transfusion with whole blood, not citrated, and both recovered.

Two interesting end-results of splenectomy for splenic anæmia were recorded by Duncan Wood, Bristol (*Brit. Journ. of Surg.*, Jan., 1924), both patients dying from hæmatemesis five years and eight months respectively after the operation: in the first case there had been no hæmatemesis preceding splenectomy, and not till three and a quarter years afterwards.

The conclusion arrived at was that severe hæmorrhage from the stomach or duodenum should not be treated surgically unless there was some strong presumption, either from one's previous knowledge of the case or from the history of pain having some definite relation to the taking of food, that the cause was some gross local lesion of one or other of these viscera—usually ulcer. Here, again, there was an important difference between hæmorrhage

from gastric and that from duodenal ulcer. Profuse hæmorrhage from chronic gastric ulcer was usually either quickly fatal from erosion of a large artery, or if once arrested did not show that tendency to early and repeated recurrence seen in duodenal ulcer. If not quickly fatal, there was usually time to improve the condition of the patient by medical treatment, so that the operation when performed subsequently was really one for the cure of gastric ulcer rather than of hæmorrhage. He had never had occasion to operate during hæmorrhage from chronic gastric ulcer, for this reason. He was no believer in the so-called "indirect" method of arresting gastric hæmorrhage in these cases. i.e., by simply performing gastro-enterostomy. If operation were to be undertaken at all it would have to be a direct and drastic attack on the bleeding ulcer—its separation from pancreas or liver, and its destruction either by excision or cautery. In hæmorrhage from duodenal ulcer there was a much greater tendency to early and repeated recurrence of the bleeding, and he regarded it as more immediately serious than gastric and more immediately requiring operation. He had operated on four occasions for severe duodenal hæmorrhage.

The first case was that of a male, aged 50, on whom he had performed a posterior gastro-enterostomy for chronic ulcer on the anterior wall of first portion of duodenum three years previously. The patient had been free from symptoms since until the last month, when he had complained of a little "indigestion." He had had two attacks of severe hæmatemesis and melæna in the last three days and was operated upon in a third attack, he (Mr. Burgess) expecting to find a "jejunal" ulcer. The region of the anastomosis was, however, quite healthy, and the bleeding was from an ulcer in the same situation in the duodenum as at the first operation—whether the original ulcer still in existence or a recurrent ulcer he could not say. The first part of the duodenum was freed, crushed with Miles' crushing clamp, ligatured at each end of the crushed portion, divided across the middle of the crushed portion, and each stump invaginated, the gastro-enterostomy being left undisturbed. No recurrence of symptoms had followed this second operation, now seven years ago.

The second case was that of a male, aged 38, who, while in hospital awaiting operation for chronic duodenal ulcer, commenced with profuse hæmatemesis and melæna. Operation disclosed a bleeding duodenal ulcer, which was crushed with Miles' clamp and invaginated as just described, and posterior gastro-enterostomy performed. Two other similar cases had been operated upon and all had recovered.

The only other occasion upon which he had operated during hæmorrhage was in a male, aged 36, on whom posterior gastro-enterostomy had been performed nine years previously; the hæmorrhage was from a jejunal ulcer, which was excised. Patient remained well for ten months, when profuse hæmorrhage recurred twice in quick succession while away from home, and he died, almost certainly from recurrence of jejunal ulceration.

### Dr. ARTHUR F. HURST.

#### THE MORTALITY FROM GASTRIC AND DUODENAL HÆMORRHAGE.

I believe that hæmorrhage from gastric and duodenal ulcers is far less often fatal than is generally supposed. I have never had a case under my care in which death resulted, although I have only once had to call in the aid of a surgeon to save the patient's life.

Dr. J. J. Conybeare has investigated the records of all post-mortem examinations on cases of fatal hæmorrhage from an ulcer of the stomach and duodenum in the absence of cirrhosis of the liver, which occurred at Guy's Hospital between the years 1911 and 1920 inclusive. The total number was only twenty-three, which is equivalent to 2·6 deaths a year, if allowance be made for the fact that no post-mortems were made in 12 per cent. of patients

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dying in the hospital. The deaths from this cause accounted for no more than 0·37 per cent. of the 6,302 autopsies performed during the ten years. Only thirteen out of the twenty-three died from hæmorrhage without having undergone some previous operation. As about 600 cases were admitted during this period for hæmorrhage from an acute or chronic ulcer, the mortality of severe gastric and duodenal hæmorrhage is only about 2·5 per cent.

### MEDICAL TREATMENT OF HÆMATEMESIS.

The most important factor favouring the cessation of hæmorrhage from the stomach or duodenum is complete immobility. Immobility of the patient keeps the blood-pressure down; immobility of the stomach and duodenum prevents a newly formed clot from being dislodged. Distension of the stomach and duodenum removes the support of surrounding tissues from the bleeding vessels and keeps the aperture in its wall widely open. An empty, contracted stomach, like an empty, contracted uterus, has no lumen, and its effect on hæmorrhage is the same. It reduces the flow of blood through its blood-vessels, and it controls the hæmorrhage by the direct pressure of the opposite wall of the viscus on the bleeding surface.

Immobility of the patient is secured by keeping him quiet in bed, reassuring him about his condition—a very important point, too often neglected, especially in hospital—and giving him sufficient morphia by repeated subcutaneous injections to keep him drowsy and prevent the mental and physical restlessness which are otherwise likely to be present. Atropine should be added to the morphia in order to inhibit as much as possible the secretion of gastric juice.

Immobility of the stomach and duodenum is secured by giving the patient neither food nor drink by mouth. Complete starvation should be continued until, so far as can be ascertained, no bleeding has occurred for forty-eight hours. The ordinary strict treatment of an ulcer with diet, alkalies, belladonna and olive oil should then be begun. During the period of starvation the mouth should be kept scrupulously clean; there is then no danger of parotitis. As much dextrose solution as possible should be given by rectum, and if the mouth is parched and the patient shows signs of being dehydrated, saline solution should be injected subcutaneously. There is no object in giving ice to suck.

To the stomach, blood is as much a food as any other protein, so that gastric juice is secreted and peristalsis occurs as actively as with an ordinary meal. When the hæmorrhage is severe, the rapid distension of the stomach results in its more or less complete evacuation by hæmatemesis. This is followed by contraction of the stomach, which may cause the hæmorrhage to cease. Smaller quantities are evacuated in the ordinary way through the intestines and give rise to melæna. But when the hæmorrhage is severe and continues after hæmatemesis has occurred, or when in spite of its severity it is not sufficiently rapid to cause the sudden distension which results in its ejection by mouth, death may result unless steps are taken to empty the stomach. It is common knowledge that uterine hæmorrhage can almost always be arrested by emptying the uterus and causing it to contract; it is less widely known that similar treatment for very severe gastric hæmorrhage is almost equally effective.

A stomach tube is passed just far enough for the end to reach beyond the cardia. This prevents any possibility of directly disturbing the bleeding surface. The stomach is then emptied; this can be most conveniently done by having the tube connected with Senoran's evacuator. The stomach is now washed

out with about four ounces of ice-cold water and this is repeated over and over until the water which comes back is no longer blood-stained. Instead of water 1 in 1,000 ferric chloride solution<sup>1</sup> also iced, can be used, as its styptic action may help to arrest the bleeding. When the last traces of water have been evacuated, a drachm of 1 in 1,000 adrenalin chloride is poured into the stomach before withdrawal of the tube. This may now gain access to the bleeding surface and cause the leaking vessel or vessels to contract. It is particularly likely to be effective in acute ulcers and in the condition called gastro-stasis by Sir William Hale-White, in which there is general oozing, presumably from innumerable minute erosions. When given in the ordinary way by mouth, the adrenalin is so diluted by the contents of the stomach that the trace which reaches the bleeding surface must be quite ineffective. There need be no fear that the general blood-pressure will be raised and that the danger of a recurrence of hæmorrhage will thereby be increased, as adrenalin given by mouth is not absorbed and has no effect at all on the blood-pressure.

The complete evacuation of the stomach and the stimulation of the mucous membrane with ice-cold water result in firm contraction of the whole organ, and the hæmorrhage is almost invariably arrested if it is gastric in origin. Duodenal hæmorrhage also generally stops, because the duodenum has no longer to deal with blood mixed with acid gastric juice coming to it from the stomach, and both the iced ferric chloride solution and the adrenalin may now reach the ulcer.

Hæmorrhage ceases when a firm clot has formed at the bleeding point. The danger of recurrence depends upon the digestion of the clot by gastric juice, the secretion of which is called forth by blood just as it is by any other food. Consequently, it is essential to render the gastric juice inactive by neutralizing as completely as possible all the free hydrochloric acid which is present. For this purpose alkalies must be given which do not, like sodium bicarbonate, stimulate the secretion of more juice after neutralizing an equivalent amount of acid, or give off large quantities of carbon dioxide, which distends the stomach and stimulates peristalsis. Oxide of magnesia is almost ideal from these points of view, as it has four times the neutralizing power of sodium bicarbonate, it is a comparatively feeble stimulant of gastric secretion, no gas is given off when it combines with acid, and it also acts as a mild aperient and so tends to prevent the severe constipation and resulting putrefaction of blood in the colon, which is the cause of the frequent pyrexia after a severe gastric or duodenal hæmorrhage. Calcium carbonate is also useful, as it has two and a half times the neutralizing power of sodium bicarbonate, it does not stimulate the secretion of gastric juice at all, and it neutralizes the acid so slowly that the gas given off dissolves in the water present as rapidly as it forms, so that no distension results. Possibly some of the calcium chloride produced may be absorbed and increase the coagulability of the blood. Bismuth carbonate has only one-third of the neutralizing power of sodium bicarbonate and it causes the stools to be black, so it is best avoided, as their naked-eye appearance affords a rough indication as to whether the hæmorrhage is stopping.

Oxide of magnesia can be most conveniently given as *emulsio magnesiæ* (B.P.C.), each drachm of which contains 5 gr. of oxide. A mixture is made containing 60 gr. of prepared chalk to each ounce of the magnesia emulsion. Half an ounce of the well shaken mixture is given as soon as possible after the

Daily lavage of the stomach with 1 in 1,000 ferric chloride solution was the routine treatment for gastric ulcer recommended by Bourget of Lausanne twenty years ago.

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hæmorrhage and then hourly during the day, and as frequently as the patient is awake during the night. This is continued until food is begun, when it can be replaced by the usual alkaline powder given half-way between the two-hourly feeds.

At the earliest possible moment after a hæmorrhage the patient's blood should be grouped and a donor should be found, who can be summoned at an hour's notice any time during the day or night should transfusion become necessary. If this precaution is taken no patient should die as a result of hæmorrhage from an acute ulcer, or as a result of the general oozing from the mucous membrane in gastro-staxis. Fatal hæmorrhage from a chronic ulcer should also hardly ever occur, as in the most severe cases, where a sclerotic artery incapable of contracting has ruptured, transfusion should tide the patient over sufficiently long for it to be possible to deal directly with the ulcer by operation. The only inevitably fatal cases are the extremely rare ones, in which death occurs actually during the first hæmorrhage, generally owing to erosion of one of the main gastric or duodenal vessels.

The hæmoglobin percentage should be estimated at the same time as the blood is taken for grouping. Daily estimations should be made until the patient is obviously out of danger. When further hæmorrhage is suspected, although no hæmatemesis has occurred, an additional estimation should be made, as a fall in the percentage would confirm this and indicate the wisdom of washing out the stomach. On the other hand, if the hæmoglobin percentage remains unaltered or rises, it is clear that no further hæmorrhage has occurred.

Transfusion should be carried out at once if the hæmoglobin percentage falls to 30.

Every practitioner may at any moment be called upon to treat a case of severe hæmatemesis. There are few conditions in which life and death depend so directly upon the treatment he adopts. I believe that a fatal result will be extremely rare if treatment on the lines I have laid down is carried out. It is perhaps necessary to point out that for its success to be assured the practitioner must be adequately equipped. He should have in his possession a stomach tube with a Senoran's evacuator. He should have an accurate hæmoglobinometer, which he is in the habit of using for the diagnosis and control of his cases of anæmia. It is one of the instruments of precision which are easy to use and require very little time; it ought not to be regarded as a specialist's toy, which nobody but the expert hæmatologist is expected to use.

There should also be at least one man in every district who can group a specimen of blood and carry out a transfusion. Only six months ago I found that there was nobody in a certain city with 50,000 inhabitants who could do this, and I wondered how many lives had been lost in consequence. Lastly, I hope the time is not far distant when London and every large town will have a list of blood donors, properly grouped, who can be called upon in emergencies, when no suitable relation or friend is available.

When the bleeding has stopped, the percentage of hæmoglobin quickly rises in most cases. Thus, a woman under my care immediately after a profuse hæmorrhage from a gastric ulcer had only 20 per cent. hæmoglobin when admitted on February 14; on the 16th it had risen to 38 per cent., on the 18th to 50 per cent., on the 25th to 60 per cent. Iron given by the mouth or subcutaneously does not seem to have much effect in hastening the blood regeneration, and arsenic is of still less use. When improvement is slow the patient

should be transfused. This not only produces an immediate rise of between 5 and 10 per cent. of hæmoglobin, but it almost always stimulates the delayed blood formation. The general improvement in the patient's condition seems also to hasten the healing of the ulcer, which sometimes tends to be very slow in cases of this kind.

#### INDICATIONS FOR IMMEDIATE SURGICAL TREATMENT.

In view of the very small danger of death from hæmatemesis or melæna it is clear that operation can very rarely be called for with the immediate object of arresting the hæmorrhage. Dr. Conybeare found that in seven of the thirteen cases of hæmorrhage from a gastric or duodenal ulcer, in which death occurred without previous operation at Guy's Hospital between 1911 and 1920, the ulcer producing the hæmorrhage was definitely of the acute type—five times in the stomach alone, once in the duodenum alone, and once in both stomach and duodenum. In none of the seven cases was there any history which would have justified surgical intervention at an earlier date. One patient had delirium tremens three months before death, and in two other cases the acute ulceration was associated with a lung abscess and gall-stones respectively. It may be safely concluded that in none of these seven cases would any operation on the stomach or duodenum have been successful in preventing death, even if undertaken at the time of the hæmatemesis. In only three of the six cases of chronic ulcer, in which the patients died from hæmorrhage apart from operation during the same period—or 0·5 per cent. of admissions from hæmatemesis and melæna—was there a history which could have distinguished them from the acute cases, and in which an operation might have been reasonably undertaken with some hope of saving life.

Quite apart from the very small danger of death from hæmorrhage—2·5 per cent. at Guy's Hospital—the immediate results of operation performed for its direct treatment are so unfavourable that I expect most British surgeons will be inclined to agree with American surgeons, who, according to Balfour, are now unanimous that “the danger of succumbing to hæmorrhage is less than the danger of operation during hæmorrhage.”

I believe that the only indication for operation in the acute stage is the occurrence of severe and persistent hæmorrhage in an elderly individual with a long history, pointing to the presence of a chronic ulcer, whose arteries are so degenerated that they are unlikely to contract sufficiently for satisfactory plugging by thrombosis.

I have only once found it necessary to have an operation performed on account of hæmorrhage from a gastric or duodenal ulcer. I had advised the patient, a man of 60, with severe arterio-sclerosis, to undergo a gastro-enterostomy the first time I saw him some months before his hæmorrhage, as he had a very long history of duodenal ulcer, which had led to obvious pyloric obstruction. He refused to submit to surgery, and continued to wash out his stomach two or three times a day as he had done for years. He then had a severe hæmorrhage, which was repeated at the end of a week's starvation, when I saw him for the second time. I advised immediate operation. The next morning Sir Berkeley Moynihan found an enormous duodenal ulcer producing pyloric obstruction; he performed a gastro-enterostomy and cut off the blood supply to the ulcer by overlapping sutures round it. The patient did very well, and died four years later from pneumonia, without having had any recurrence from gastric symptoms.

I have also once had to recommend operation on a man who bled repeatedly from a jejunal ulcer, which had developed as a result of a gastro-enterostomy performed the same time as a perforation of a duodenal ulcer was sutured. A jejunostomy was made



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and the hæmorrhage ceased, but death resulted from peritonitis owing to the jejunum tearing away from the abdominal wall. All his subsequent misfortunes would probably have been avoided if the surgeon had been content with suturing the perforation without performing gastro-jejunostomy at the time of the first operation.

### SHOULD THE OCCURRENCE OF HÆMORRHAGE BE REGARDED AS AN INDICATION FOR SUBSEQUENT OPERATION?

Before deciding whether the danger of a recurrence of hæmorrhage should be regarded as an indication for operation when the patient has recovered from his acute symptoms, it is necessary to see what the results of surgery are in cases of ulcer which have previously bled, and how frequently hæmorrhage occurs after operations on patients who have never before had hæmatemesis or melæna. According to Balfour, hæmorrhage occurred after operation at the Mayo Clinic in 13 per cent. of cases of ulcer (eighty-three duodenal and seventeen gastric) with a history of previous hæmorrhage, but only in 0·9 per cent. of cases with no previous hæmorrhage. This may be regarded as the minimal risk, which requires correction by such statistics as those of Hohlbaum, from Payr's University Clinic in Leipzig, in which the after-history is taken into account, in order to get an accurate idea of the average risk. Hohlbaum reported in 1922 that no less than 20 per cent. of cases in which a gastro-enterostomy was performed for duodenal ulcer were followed by severe and occasionally fatal hæmorrhage, either immediately or months or even years later. As most of the cases in which hæmorrhage occurred were in patients who had previously bled, the operation is now no longer performed in Leipzig for hæmorrhagic cases.

Of the nine Guy's cases between 1911 and 1920, in which death followed hæmorrhage from an ulcer and in which an operation had been previously performed, only two had suffered from hæmorrhage before the operation. In one of these death occurred from hæmorrhage the day after a gastro-enterostomy for a duodenal ulcer, and the other from hæmorrhage from a gastric ulcer three years after the excision of an earlier gastric ulcer. In all the other cases the first hæmorrhage occurred after the operation. The hæmorrhage took place respectively seven days, eight days and five years after a gastro-enterostomy for gastric ulcer, six days after a gastro-gastrostomy for gastric ulcer, and six weeks after an ileo-sigmoidostomy for gastric ulcer. One of those who died after a gastro-enterostomy had had an ileo-sigmoidostomy performed two years earlier. Lastly, one patient died of hæmorrhage fourteen days after the suture of a perforated chronic pyloric ulcer, and another nine months after suture of a perforated chronic gastric ulcer. As about 900 cases had undergone operation for gastric or duodenal ulcer during the period under review, and nine such cases died of hæmorrhage, the mortality from hæmorrhage after operation is approximately 1 per cent.

I am able to supply further information on this subject from the statistics of New Lodge Clinic. Out of sixty consecutive patients with gastric or duodenal ulcer admitted to the clinic, who had not previously undergone any operation, five out of the sixteen gastric cases and eight out of forty-four duodenal cases had had hæmatemesis or melæna or both, together making 21·6 per cent. Nine of these patients had an operation subsequently performed for their ulcers on account of pyloric or hour-glass obstruction, and one because of failure of medical treatment; the only one who had previously had a hæmorrhage had another severe hæmorrhage the day after the operation.

During the same period twenty-five patients were admitted for symptoms which had followed an operation performed for duodenal ulcer. Among the sixteen of these who had not previously bled, one had a hæmorrhage three days and another four days after the operation, a third had hæmatemesis three years later, and a fourth had repeated severe hæmorrhages from a jejunal ulcer, which had finally to be excised. The remaining nine had had hæmatemesis or melæna before the operation, and in no less than seven it recurred with equal or greater severity afterwards; in two cases a bleeding jejunal ulcer was still present after two additional operations had been performed.

Thus, out of twenty-six cases of hæmatemesis or melæna no less than eleven, or 42 per cent., occurred after operation, and of these, four had not previously bled. These figures show that the liability to hæmorrhage is just as great after an operation has been performed for gastric or duodenal ulcer as after medical treatment, whether the patient has already had a hæmorrhage or not.

SHOULD THE DANGER OF SUBSEQUENT HÆMORRHAGE IN A PATIENT WHO HAS NOT ALREADY BLED BE REGARDED AS AN INDICATION FOR SURGERY ?

The supposed liability to hæmorrhage and the supposed danger of the latter to life are often used as arguments in favour of the operative treatment of gastric and duodenal hæmorrhage. Nielson has investigated the after-history in a series of 182 patients from Professor Faber's clinic in Copenhagen, who were diagnosed with a fair degree of certainty as suffering from ulcer. An operation was performed on forty-two of them between one and twenty-one years later, but never for hæmorrhage. The remainder were traced for between two and a half and nineteen years; of these, sixteen had died, but only two, or less than 1 per cent. of the whole series, from hæmorrhage.

Dr. Conybeare's investigations on the history of the twenty-three fatal cases of gastric and duodenal hæmorrhage which occurred at Guy's Hospital between 1911 and 1920 throw further light on this question from a different point of view. Of the six deaths from hæmorrhage from a chronic ulcer one gastric and one duodenal case had a history of only six and eight weeks respectively, and one patient, an old man of 74, who died from hæmorrhage from a chronic duodenal ulcer whilst in hospital for a fractured femur, had never suffered from indigestion. Only two patients with a long history of indigestion died as a result of hæmorrhage from a chronic gastric ulcer and one from a chronic duodenal ulcer without having previously undergone any operation; of these the duodenal case and one gastric case had had previous attacks of hæmatemesis and the duodenal case had had melæna.

We may conclude that only two patients with chronic gastric ulcer and one with chronic duodenal ulcer dying of hæmorrhage in Guy's Hospital in the ten years between 1911 and 1920, had had symptoms which should have made a correct diagnosis possible before the occurrence of fatal hæmorrhage, and that their lives might therefore have been saved if an operation had been performed sufficiently early. But when it is remembered that during the same period two patients with a history of hæmatemesis died from hæmorrhage after operation, and no less than seven whose ulcers had never bled before died from hæmorrhage after operation, it is clear that the number who actually recovered under medical treatment, but who might have died after operation

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if surgery had been the routine treatment for all cases diagnosed as ulcer, would more than have counterbalanced the death of these three patients who might possibly have been saved by an earlier operation.

The statistics collected by Nielson from Faber's clinic are the only previously published statistics which supply the same kind of information as those of Dr. Conybeare from Guy's Hospital, and the conclusions which can be drawn from them are exactly the same. Between 1897 and 1915, 528 patients were diagnosed as having a gastric or duodenal ulcer. During the same period ten patients died as a result of hæmorrhage from an ulcer. But four of them had had no digestive symptoms prior to the fatal hæmorrhage; four others had had slight pain, nausea or vomiting within the previous six months, but not before; the ninth had had occasional epigastric pains, to which he "had not paid attention" for a couple of years, and the last had had occasional attacks of "gastric catarrh" for six years. In none had there been any previous hæmorrhage, and in none had the symptoms been of such a nature that a definite diagnosis of ulcer could have been made. It is extremely doubtful whether the most enthusiastic surgeon would have operated on more than one or two of them had he seen them at any time before their admission for hæmorrhage.

In view of these results it seems to me that neither the occurrence of hæmorrhage in the past nor the danger of hæmorrhage in the future in a patient with an ulcer, which has not previously bled, can be regarded as in any way pointing to the desirability of surgical rather than medical treatment. In my opinion the only definite indications for surgery, apart from perforation, are hour-glass contraction and pyloric obstruction, neither of which should ever occur if the diagnosis is made as early as it should be, the treatment is sufficiently strenuous and prolonged, and proper after-treatment is instituted.

The following are summaries of the statistics referred to :—

### (A) GUY'S HOSPITAL STATISTICS FROM 1911 TO 1920 INCLUSIVE, COLLECTED BY DR. J. J. CONYBEARE, MEDICAL REGISTRAR.

- (1) Cases admitted for hæmatemesis or melæna ... 600<sup>1</sup>
  - (2) Post mortems excluding two with advanced cirrhosis ... 22 = 0.37 per cent. of post-mortems  
= 2.6<sup>2</sup> a year.
  - (3) Of these (i) no previous operation : acute ... 7 } { 18 = 0.24 per cent. of post-mortems  
chronic ... 6 } { = 1.5 per cent. a year = 2.5 per cent.<sup>2</sup> of cases admitted for hæmorrhage
- Of these a previous history pointing to ulcer in only three.
- (ii) Previous operation : no previous hæmorrhage 7 } { 9 = 0.13 per cent. of post-mortems  
previous hæmorrhage 2 } { = one<sup>2</sup> a year = 1 per cent. of operations on ulcers

### (B) NEW LODGE CLINIC, 1921-1923

- |                                                                                                   |                                                                                                       | Gastric | Duodenal |
|---------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------|---------|----------|
| (1) Sixty patients with proved gastric or duodenal ulcer ...                                      | Previous hæmorrhage ...                                                                               | 5       | 8        |
|                                                                                                   | No previous hæmorrhage ...                                                                            | 11      | 36       |
| Of these a subsequent operation was performed...                                                  | Previous hæmorrhage, 1; subsequent hæmorrhage, 1. No previous hæmorrhage, 9; subsequent hæmorrhage, 0 |         |          |
| (2) Twenty-five patients admitted with symptoms following operation for duodenal ulcer.           |                                                                                                       |         |          |
| No hæmorrhage before operation, 16; of these 4 had hæmorrhage from jejunal ulcer after operation. |                                                                                                       |         |          |
| Hæmorrhage before operation, 9; of these 7 had hæmorrhage from jejunal ulcer after operation.     |                                                                                                       |         |          |

Thus out of twenty-five consecutive cases of ulcers with a history of severe hæmorrhage, in eleven the hæmorrhage occurred from jejunal ulcers after operation for duodenal ulcer, and of these four had not previously bled.

<sup>1</sup> Approximate.

<sup>2</sup> Allowing for 12 per cent. of deaths, in which no post-mortems were performed.

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Mr. R. P. ROWLANDS.

Most physicians and surgeons agree that operation is necessary for the treatment of chronic ulcers that repeatedly and seriously bleed. Fortunately this grave complication is becoming less common owing to earlier and more radical operations. About 25 per cent. of these ulcers cause obvious and severe hæmorrhage, but this does not directly cause death in more than about 2 per cent. Indirectly it contributes towards a fatal result in many more cases. From the surgical point of view the important questions for discussion are *when* and *how* to operate. It is rarely wise to operate during a severe attack of bleeding, especially during the first attack, because:—

(1) The danger of death from the bleeding is not great, and is, in fact, less than when the risk of a major operation is added to that of the severe loss of blood.

(2) If the hæmorrhage is from an acute ulcer (especially in a young patient) the hope of a complete recovery under thorough medical treatment, without any operation, is very good.

(3) The exact cause and source of the hæmorrhage is often uncertain at this stage owing to the impossibility of making a complete investigation by radiography and other methods. The bleeding may be due to other causes, such as splenic anæmia, cirrhosis of the liver, or it may be of the nature of gastro-staxis or general oozing of blood from the gastric mucosa without evident ulceration.

Although a few patients die from the first hæmorrhage, it appears to be safer to try to tide over the emergency by medical treatment and then to make an accurate diagnosis and to operate if necessary as soon as the condition of the patient is favourable. An expert radiographer now rarely fails to demonstrate the crater or deformity of a chronic ulcer, if any exist.

*Recurrence of bleeding*, even when a patient is under careful medical treatment, is a very important indication for early operation, for the bleeding is often of the secondary or septic type and will almost certainly recur or continue to a fatal end. It is generally wise to seize the first favourable opportunity during the reaction. In such a case in Guy's Hospital some twelve years ago, after the second severe hæmorrhage within three days, I successfully removed an ulcer from the lesser curvature of the stomach, with the gastric artery open in its base. Six months earlier a gastro-jejunostomy had been performed and had relieved all symptoms, but, in spite of this, the ulcer had not healed and had gradually extended and finally opened the large gastric artery.

*When the bleeding continues* in spite of medical treatment an emergency operation is sometimes the only hope of saving life. In such a case I recently resected with success the first part of the duodenum and pylorus and made an end-to-end union. The gastro-duodenal artery had been eroded in the base of the ulcer. Transfusion just before or during the operation makes the latter much safer if the hæmoglobin is below about 50 per cent.

*Operation.*—The exact nature of the operation to be performed depends on the condition found and the state of the patient. Ulcers which cause obvious hæmorrhage before operation are very apt to bleed afterwards unless they are

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removed or directly and thoroughly treated. A haphazard gastro-jejunostomy is not enough. Balfour found 13 per cent. of recurrences in these cases unless the ulcer was excised or destroyed.

Therefore, whenever possible, the ulcer is removed by the knife or cautery. Failing this it is invaginated by sutures and the arteries feeding it are tied; for the usual gastric ulcer on the lesser curvature the gastric and pyloric arteries are secured, and the gastro-duodenal and right gastro-epiploic arteries for the posterior duodenal ulcer, which commonly bleeds so seriously. However, a direct attack upon the ulcer is more certain to arrest the bleeding, for it is by no means always possible to be sure of the exact vessel that is leaking. As an instance, the splenic artery may be eroded by a gastric ulcer.

Bleeding from an anterior duodenal ulcer may be dealt with by performing a Finney's operation, the ulcer being excised at the same time; a small ulcer may be cauterized and sutured. A small accessible gastric ulcer is excised and gastro-duodenostomy performed. Sometimes a partial gastrectomy or sleeve resection has to be performed for large gastric ulcers which cause severe bleeding.

It is important to use only absorbable ligatures and sutures in order to avoid the risk of secondary hæmorrhage. A ligature of linen thread, used to tie the gastric artery while excising a gastric ulcer, after some months caused a small ulcer with repeated attacks of severe hæmorrhage. After transfusion of a pint of blood given by my house surgeon, Mr. Whitlock, I operated and found the ligature in the base of the minute ulcer on the lesser curvature. The patient made a good recovery.

## Sections of Surgery, of Medicine, and of Therapeutics and Pharmacology.

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### ADJOURNED DISCUSSION ON "THE TREATMENT OF SEVERE GASTRIC AND DUODENAL HÆMOR- RHAGE."

EVENING MEETING.

Chairman—Dr. ROBERT HUTCHISON (President of the Section of  
Medicine).

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Dr. IZOD BENNETT.

ONE point which has emerged in this discussion, and which speakers on both the medical and the surgical sides have emphasized, is the fact that the immediate treatment of cases of hæmorrhage from the stomach and duodenum should be left, practically exclusively, in the hands of the physicians. And though that has been stated by almost every speaker, including Mr. Paterson, I think it is very important once more to emphasize it, because at the present day we do see these cases, when they come into hospital, sent often directly to the surgeons' wards, a practice which clearly shows that there is a growing idea among students and recently-qualified practitioners that there is a surgical treatment for this condition. There are four reasons which I believe lead to the conclusion that the immediate treatment of these cases should be left entirely in the hands of physicians.

(1) The diagnosis is so often obscure. Cirrhosis of the liver, blood diseases, and other conditions are as frequently the cause of hæmatemesis and melæna as are simple ulcerations of the stomach and duodenum. Erosions of the mucosa, often so minute as to be indistinguishable except with the help of a pocket lens, have frequently caused serious, and sometimes fatal, hæmorrhage.

Does any surgeon seriously advocate the removal of stomachs in which there is no visible lesion?

(2) The object of urgency as regards treatment in those patients is the prevention of immediate death. Granted that a patient has a gastric or duodenal ulcer to what risks is his life exposed? The question can be answered by examining post-mortem records—a form of research which is unhappily no longer in fashion. I have recently made such an inquiry, not selecting the cases of one physician or one surgeon, but taking the total records of a great hospital with a large and distinguished staff. I found records of

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sixty-one autopsies performed during a period of nine years, upon patients who had died as the result of gastric or duodenal ulcer; the immediate cause was perforation in twenty-five cases, hæmorrhage in thirteen cases, and surgical intervention in twenty-three cases. The patients in the cases dying of perforation had nearly all been treated surgically after the perforation, they had died in spite of surgery. The patients in the twenty-three cases which I have classified as dying as a result of surgical intervention had perished from shock, peritonitis following leakage at a junction, post-operative broncho-pneumonia or some other cause enabling one to say that had there been no operation the patient would have lived longer. There were only thirteen deaths from hæmorrhage, so that clearly operation in this series of sixty-one cases caused more deaths than bleeding did.

(3) A patient who has had a severe hæmorrhage is in poor condition to withstand the shock of operation; why then expose him to a form of treatment which, as we have just seen, is responsible for more deaths than is the condition from which he is already suffering?

(4) In the series quoted, hæmorrhage was not responsible for more than 21 per cent. of all deaths due to gastric or duodenal ulceration. The percentage of deaths in all patients with gastric or duodenal hæmorrhage who are treated medically is certainly much lower, amounting to not more than 5 per cent. of all those to whose records I have had access. The common form of surgical treatment for gastric and duodenal hæmorrhage to-day is partial gastrectomy. What is the mortality of this operation? Nobody knows. It is useless to quote the statistics of one or two specially dexterous and experienced surgeons; we require the figures from a really wide circle of operators. What is the mortality in a wide series of that sort? 5 per cent.? 10 per cent.? or higher? Unless it is below 5 per cent. gastrectomy for hæmorrhage would be unjustified even if such patients were as resistant to shock as are those who have lost no blood.

These four reasons seem to me to demonstrate the great disadvantages of surgical treatment for these cases. Modern medical treatment, on the other hand, is very satisfactory in view of the gravity of the condition, though I think that certain principles in the treatment require emphasis.

(a) Absolute gastric and duodenal rest must be attained, and this implies strict starvation by mouth, fluid being given entirely by rectum in the form of saline administered very slowly over long periods.

(b) Intravenous administration of either blood or saline must be avoided, their dramatic restorative effect may be followed by an equally dramatic collapse due to recurrent hæmorrhage following the rise of blood-pressure.

(c) Morphia, hyoscine or omnopon must be given in sufficient quantity to secure complete and continuous quietude on the part of the patient; these drugs not only enhance the rest to the affected parts, they check the digestive secretions which play so serious a part in the ulcerative processes.

(d) Mouth-feeding must only be resumed when a fall in pulse-rate and an absence of symptoms for three or more days point to the hæmorrhage having ceased. When it is resumed mouth-feeding must consist of minute doses of a solution of glucose flavoured with lemon-juice, which should be given half-hourly or hourly, and increased only with extreme caution.

A certain mortality will occur even with such treatment, just as there remains a certain mortality with surgical treatment of perforation, or with gastrectomy performed in the most favourable circumstances, but I would emphasize that this is low, and that remarkable recoveries are often observed

in desperate cases. There is indeed no criterion by which the physician can to-day say that the prognosis in any given case is hopeless.

When all danger from hæmorrhage is past, and the case has become one of straightforward chronic gastric or duodenal ulcer, the problem of treatment is quite different, the advisability of surgical treatment becomes the first question to decide, and the fact that hæmorrhage due to ulceration has once threatened the patient's life may be an additional factor in favour of surgery.

MR. GORDON-TAYLOR

said that the remarks which he had to make were uttered in no didactic sense, but merely expressed the experience of one who had had ordinary luck in the surgical treatment of these cases, and who possessed no superior skill.

The surgical treatment of cases of acute ulceration of the stomach need not detain the meeting, those cases never required surgery at all. The mortality of the medical treatment of them was very low, probably less than 2 per cent.

In the matter of chronic ulcers of stomach and duodenum, the ground was different. He thought it was true that in some cases of hæmorrhage from chronic ulcers of the duodenum and stomach the condition was an absolutely fatal one. Two such cases at least had come under his notice in the last three or four years, where on each occasion an enormous ulcer of the duodenum eroded the hepatic artery, with fatal consequences. He had also seen fatality from an erosion of the splenic or other big artery in the posterior wall of the abdomen in more instances than he could count on the fingers of one hand.

The treatment of these cases of hæmorrhage in connexion with chronic gastric and duodenal ulcer should be prophylactic or preventive; they should be dealt with surgically before the ulcer attained such a size, or reached such a depth, as to cause an erosion likely to have fatal consequences. Once hæmorrhage had started in connexion with these ulcers, the question of surgical treatment arose. Possibly it was wise to wait until the particular hæmorrhage had ceased, and interfere as soon thereafter as possible; but it was only fair to remember that in a certain number of cases erosion of a big artery took place fairly quickly and the patient's life might rapidly be in jeopardy, and surgical interference might not be possible. He had been led to operate on some of these cases when hæmorrhage was taking place, through the cases having been transferred to him by one or other colleague because of recurrent hæmorrhages which seemed almost certain to finish life. Having succeeded in several of these cases dealt with by a direct attack on the bleeding point, associated with transfusion, &c., he had been led to operate on some similar cases earlier, i.e., as soon as the hæmorrhage started, always provided that he could be sure that a chronic ulcer of the stomach or duodenum was the source of the hæmorrhage. He had been encouraged by the remarkable figures of Finsterer, of Vienna, according to whose report mortality from direct operation on bleeding ulcers of stomach or duodenum at an early stage had been very low.

He (the speaker) had had experience of twenty-four cases of chronic ulcer of the stomach and duodenum operated upon when the hæmorrhage was taking place. Sixteen were males, eight females, sixteen of the twenty-four were gastric ulcers, six duodenal, two gastro-jejunal.

The method of operation he adopted had varied. In three cases, gastro-enterostomy was combined either with cauterization of the ulcer, or ligature of the vessel going to the ulcer. In sixteen he did gastro-duodenal resection,



two were sleeve resections to include the ulcer, and two were cases of enormous giant ulcer, treated by jejunostomy and tamponnading that portion of stomach or duodenum where the ulcer was present from which the very large hæmorrhage was taking place.

Of two fatal cases in the series, one was that of a priest who had been operated upon for a perforated ulcer a fortnight before, and developed a large hæmorrhage from the splenic artery. So severe was the hæmorrhage that he was rapidly exsanguinated. The speaker opened the abdomen and dealt with the bleeding point, but the blood-donor arrived too late, and death took place an hour after the operation was completed.

The other fatality took place from septic blood transfusion : he died thirty-six hours afterwards with a high temperature, rapid pulse, &c. Therefore he (Mr. Gordon-Taylor) had the feeling that if one did operate on these cases, and if one was provided with all the methods of resuscitation, particularly blood transfusion if required, these cases, if dealt with surgically, would not show a very high mortality—his own was 8 per cent.

He had been greatly impressed with the enormous value of para-vertebral and splanchnic anæsthesia, thus reducing the amount of anæsthetic required to an absolute minimum. The hæmorrhage from these chronic ulcers was necessarily secondary, and he considered they called for treatment at a very early stage. But when a surgeon did intend to interfere in a case of active gastric ulcer or duodenal hæmorrhage it was useless to think of the possibility of saving the patient unless one was amply provided with all the necessary means of resuscitation. Two of these cases were particularly interesting. There was perforation as well as hæmorrhage. His experience was that perforation and hæmorrhage rarely occurred at the same time; two of his cases were operated upon for this combination. In one case the man had been bleeding all day, and when the abdomen was opened, the whole right side, hypochondrium and lumbar region, were full of blood, and blood was pouring out of an opening in the first part of the duodenum. He was treated by gastro-duodenal resection, and although the man was aged 67, he made a good recovery.

Another interesting case was that of a naval officer, who had a long history of chronic indigestion; he was bringing up pints of blood, and he was extremely ill, almost dying. Partial gastrectomy was done, and now, three years after, he had doubled his weight.

Therefore, in his experience, acute hæmorrhage from chronic ulcer of duodenum or stomach could be treated satisfactorily by operations dealing directly with the bleeding point, rather than by operations which dealt indirectly with the cause of bleeding. He believed that would be increasingly the case in the future.

#### Dr. PHILIP HAMILL

said that arrest of the hæmorrhage was the fundamental therapeutic principle. Gastric or duodenal hæmorrhage differed from hæmorrhage from the surface of the body in that one only knew that the patient had bled, and that the bleeding-point was not susceptible to pressure. He had never seen an immediately fatal case. Treatment, assuming a diagnosis of hæmorrhage from an ulcer, was of a patient who had bled, might be bleeding, or might bleed again. Complete tranquillity of the patient was necessary, and contraction of the musculature of the stomach could be aimed at. He believed that the applica-

tion of an ice-bag did help to arrest hæmorrhage; it undoubtedly eased the patient's mind by relieving soreness. The administration of chalk combined with magnesia was useful for the promotion of coagulation at the bleeding-point. As regards the washing out of the stomach with cold water, he had had no occasion to use so drastic a method. The use of the stomach-tube did not promote tranquillity. Treatment was essentially medical; but cases occurred where surgical measures might be required; many of these were duodenal, and some were in elderly persons with sclerosed vessels—the ulcer perforating through into a vessel.

Mr. C. H. FAGGE

remarked that it would appear to be unnecessary to add to what had already been said against operation in acute gastric hæmorrhage; but, at the time when Sir William Hale-White was writing on gastro-stasis and he (the speaker) was only recently on the surgical staff, he was called to see a girl who was suffering from acute hæmatemesis, with a view to an immediate operation. He thought surgical treatment inadvisable. Hæmorrhage recurred in the next twenty-four hours and the girl was admitted to the Bolingbroke Hospital. Her abdomen was opened, no cause was found for the hæmorrhage, and she died. None was found afterwards. At that time the experience was interesting to him, as he had not seen anything like it.

He regretted he could not see eye to eye with some of the previous speakers as to the danger of hæmorrhage from ulcer of the stomach or duodenum. When some twelve years ago, in the Mayo Clinic, he saw Dr. William Mayo excising duodenal ulcers, it appeared to him (the speaker) to have distinct advantages, and since that date it had been his practice to excise—he was going to say whenever possible, and he thought it was always possible—every gastric or duodenal ulcer in which there was a history of hæmorrhage. He thought his views and practice were in close accord with those of Mr. Gordon-Taylor, in that the way to treat such hæmorrhages was to prevent them. Since he had been treating bleeding ulcers radically, he had seen a certain number of cases of duodenal ulcer on which he had in previous years only done gastro-jejunostomy, and the patients had since bled. And this seemed exactly to answer, in another light, Dr. Hurst's remarks about Dr. Balfour's paper, in which the latter showed that hæmorrhage had recurred after operations on gastric and duodenal ulcers. Surely Dr. Hurst was wrong in urging that operation was contra-indicated. Operation was indicated, but it was not the operation which had been done. In his (Mr. Fagge's) opinion some other operation, one dealing directly with the ulcer, was the right course.

In regard to the treatment of recurrent or continued hæmorrhages in cases in which there was no clinical doubt that an ulcer of the duodenum or stomach existed, here the surgeon and physician were in the greatest difficulty. During the last year or two he had had the care of, jointly or solely, two people who died from hæmorrhage, and death took place at the time when surgical treatment was being considered. They had now under care another patient suffering from continued hæmorrhage, and they were much exercised as to what was the right thing to do and when to do it. Recently he had operated upon a case for a medical colleague who had no doubt that surgery was the only chance of saving the patient's life; he was supposed to be suffering from ulcer of the duodenum. After transfusion, he (Mr. Fagge) opened the abdomen and found the man had a thickened pylorus, due apparently to an ulcer in the duodenum; but he had

also a large ulcer on the lesser curvature of the stomach. Not knowing from which spot the hæmorrhage was proceeding, he excised the ulcer on the lesser curvature, and found in its base a large vessel. And here, as in the case of the two patients who died, he was unable to believe that hæmorrhage from such vessels could have been stopped by any medical means. He also excised the ulcer in the duodenum in the case to which he was referring and the patient lived ten days. That man's hæmoglobin was 14 per cent. It might be urged that that was an unjustifiable surgical risk. It was his practice when patients suffering from hæmorrhage were admitted, at once to estimate the hæmoglobin, to inject a grain of calcium chloride into the buttock, and possibly also 10 c.c. of horse serum, and group the blood. Whether one should make a small infusion and repeat it every two or three days, or whether transfusion should be reserved until the ulcer had been dealt with surgically, was a matter upon which at present his views were fluid; but he felt that a transfusion not exceeding 500 c.c. did not so raise the blood as to predispose to a recurrence of the hæmorrhage. He had hoped to learn that day something about hæmoglobin estimation, i.e., as to what it meant and what guidance it afforded; also as to what one should do in the cases of hæmorrhage in which rest, medical measures, and possibly transfusion, did not raise the hæmoglobin above 30 per cent., and whether at that stage it was wise to interfere. He regarded it as a very difficult problem on which ideas might be completely changed. He could not believe that big ulcers with large vessels at their base, some six to ten of which, in addition to the above, he had successfully dealt with, could be expected to be dealt with adequately without surgery; and any operation should be based on a direct attack upon the ulcer—by excision, sleeve resection, or partial gastrectomy.

#### Professor GEORGE GASK, C.M.G., D.S.O.,

said he would confine his remarks to the treatment which should be carried out for the patient who was bleeding from gastric ulcer, for he assumed that was the most important part of this discussion, and it was one upon which opinions widely varied.

Until about three years ago, he would have endorsed every word which Mr. Paterson uttered in his opening address; he had held and expressed that view as emphatically as he could. But in the last three years his opinions had changed, and the reasons for such change—which to him seemed sufficient—he would like to state.

The first reason was that he had seen a number of cases in the post-mortem room in which patients had died directly from hæmorrhage, and in which a large open vessel was seen in the gastric ulcer, a vessel of the magnitude of the splenic artery, or, as Mr. Gordon-Taylor said, of the hepatic artery. He could not conceive that any medical treatment would be likely to stop hæmorrhage from such cases.

The second reason for his change of view was the value of transfusion. He thought the question of blood transfusion had been the most powerful factor in his change of view. If a patient had lost blood, the one thing he needed was more blood. By transfusion a patient could be restored nearly to the condition before the bleeding had occurred, and the operative risk was then not nearly so great as in the time when transfusion was not practised. If, however, a patient was allowed to bleed twice, three times, even to six times, then the risk was a very bad one.

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The third reason which had influenced him was the undoubted great improvement in the technique of gastric surgery, which had taken place during the last few years. Though up to three years ago it was his opinion that a patient who had bled from a gastric ulcer was more likely to recover if treated by medical means than if treated surgically, he now thought there were a certain number of cases in which the patients could be saved, and should be saved.

He agreed with Dr. Hurst's remark that the ideal was that all gastric ulcers should be prevented; he agreed it should be regarded as a medical complaint, and that the cases should be cured medically. But the fact was that at the present time it was necessary to deal with a certain number of cases which came in a moribund condition, and a certain proportion, though admittedly a small percentage, died.

A case he had in mind, which occurred a few months ago, was that of a young woman, who bled profusely into her stomach, and the blood clotted there. He felt that if that stomach could be emptied and washed out, so that it could be allowed to contract, all the hæmorrhage would be stopped, in the same way as hæmorrhage was stopped from a bleeding point in the lung as soon as the lung was made to collapse. But in this case the stomach was distended and the blood formed a clot. The stomach was opened, and the clot had to be cleared out by hand. The bleeding point was then tied, and the patient made a satisfactory recovery.

The difference of opinion which had been revealed in this discussion, not only between physicians and surgeons, but between different physicians and between different surgeons, made him feel that the truth probably lay between the two: that not all ulcers should be operated upon, but that some should. That was his present feeling. What should be sought was some means of estimating which cases were likely to be fatal. If it could be ascertained which patient was going to die if left alone, an operation should certainly be done, and then a certain number would be saved—he had no idea how many. At St. Bartholomew's Hospital, Dr. Fraser and he had agreed upon a line of action which was now in operation. All urgent cases of hæmatemesis believed to be due to gastric ulcer which came into the wards were seen by Dr. Fraser and himself together, and had an immediate transfusion of blood up to 500 c.c. He did not agree with Dr. Izod Bennett that a transfusion of blood was liable to cause the patient to bleed more; his war experience taught him it rendered the patient less liable to bleed as it increased the coagulability of the blood. However, he did not press that point, as it was not yet on certain ground. The next stage was, that if the patient bled again, they met and discussed the case, trying to make up their minds whether the case was likely to be a bad or a hopeless one, or, on the other hand, whether it was likely to get better.

### **Mr. GARNETT WRIGHT (Manchester)**

said he attended this discussion in order to find out what were the views in London on this very difficult problem, one on which he had not yet made up his own mind.

He thought the first need was to sweep away all records including cases of hæmatemesis due to other conditions than ulcer, because they should be treated by medical means. The figures in Mr. Paterson's Table I and Table II were not of cases which were comparable, as they were different conditions.

It was first necessary to decide whether a patient had gastric or duodenal ulcer. He did not feel so despondent about the possibility of making up his

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mind, on clinical grounds, as to the diagnosis of these cases, because many of the patients gave a clear-cut history, and when they did so one could be sure they had an organic lesion. All accessory methods of diagnosis, such as the radiographic, were out of the question ; he regarded the clinical history as a very safe guide.

Having decided that the patient had gastric or duodenal ulcer, he agreed with most of the speakers that the majority of the patients would get better on medical treatment, that was the best treatment in the first instance. But, in spite of what had been said, one did see patients in whom hæmorrhage had recurred in spite of medical measures ; sometimes the hæmorrhage proved fatal. He agreed with Professor Gask, it was the duty of the physician and surgeon to try to form a view which of the cases were likely to prove fatal, and in those cases to give the chance of operation. Of seven such that he had operated upon, three died, four recovered. He had never operated after the first hæmorrhage. In the case of some of the patients who recovered he believed the operation saved their lives. In one of the patients he found an ulcer adherent to the head of the pancreas, and at the time of the operation there was a spouting artery at the bottom of the ulcer. He was treated by pylorotomy, and made a good recovery.

The difficulty was in deciding which cases to operate upon. If a patient had had two, or three, hæmorrhages, and it was the view that he would not be able to stand another, then, with all the assistance obtainable from transfusion, operation was carried out. He did not think it was right to operate while a patient was bleeding if that could be avoided, as there was considerable shock from the hæmorrhage, and one could wait twelve hours feeling that a recurrence of bleeding was not likely to take place in that time, and that the patient would be in a better condition for operation.

#### Dr. ROBERT HUTCHISON (Chairman)

said that everyone would agree that the Society had had a very interesting and vital discussion on this difficult subject. Listening to the discussion as a whole, as he had done, there were certain things which emerged, about some of which there was general agreement whilst on one or two points there was a considerable divergence of view.

First with regard to the danger of hæmatemesis : was it, or was it not, a condition dangerous to life ? He thought the discussion had shown that it was not a very dangerous symptom ; that patients did not often die from the vomiting of blood. None the less, some of the speakers seemed to have rather under-estimated the danger. He was surprised to hear Mr. Paterson say he had yet to see a fatal case of hæmatemesis. In that respect Mr. Paterson had been much more fortunate than himself (the speaker), as he had seen many fatal cases of hæmatemesis, some from cirrhosis of the liver, some from splenic anæmia, several from gastric and duodenal ulcers, some even from gastric carcinoma. It was, therefore, far from a trivial matter.

There was a general agreement in the discussion about the importance of accurate diagnosis of the cause of the hæmatemesis before commencing treatment. There was a whole class of cases which offered no field for surgery : for the hæmatemesis of cirrhosis of the liver, for that of splenic enlargement, of gastrostaxis, &c., surgery could do nothing. Practically, it narrowed itself down to the possibilities of surgical treatment in hæmorrhage from chronic ulcer of the stomach or duodenum ; but all the others were only to be treated medically. On that point, the meeting had had from Sir William

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Willcox and other medical speakers a description of the different methods available for medical treatment. He (Dr. Hutchison) did not wish to be misunderstood if he said there was no medical treatment for hæmatemesis; that if the patient were put to bed and left alone, the results would be very much the same. He doubted whether the injection of calcium made much difference, or whether any of the hæmostatic sera did either. It was agreed that patients did not often die of hæmatemesis, and it was almost impossible to be sure whether the things one did made any real difference. It was a difficulty one was always up against in therapeutics, but especially here; and he often felt that if the patient had been left quietly in bed and nothing given except morphia, and only saline by the rectum, all that was likely to make any difference was being done. He doubted whether an ice-bag on the stomach did anything but impress the patient's friends, and in such a case he had never yet had the courage to wash out the stomach. He agreed with Dr. Hurst that, theoretically, there was much to be said for washing out the stomach, and perhaps giving adrenalin, but he had not done it, and would particularly hesitate to do this in private practice, for if the patient died he was sure that it was not the hæmorrhage which could be blamed.

Therefore the only point on which there was a great difference of opinion was as to the possibility of surgical treatment in bleeding from chronic gastric and duodenal ulcer. The opening surgical speakers were against immediate operation in such cases, and there was some disagreement as to how long after the hæmorrhage operation should be performed. That evening, some of the surgical speakers had urged the view that, given modern surgical technique and the possibilities of moderate transfusion, there was no reason why many of these hæmorrhages from chronic ulcers should not be dealt with by attacking the bleeding point, without materially adding to the risk run by the patient. There was much to be said for that, he thought, speaking as a physician. The fact that the bleeding point was inside the abdomen should make no difference, and he was pleased to hear Mr. Fagge and Mr. Gask contend in the same way; not to hold one's hand in cases which one had reason to fear would go on, i.e., in which the patient's life was seriously threatened. His own practice had been, following the teaching of Mr. Sherren, not to advise operation while bleeding was persisting, but advise it not later than forty-eight hours after it had stopped, and he had seen no reason to regret this course. He had several times bitterly repented having waited longer than forty-eight hours; he could recall several cases which had slipped through his hands in that way through delay in operating. If he were a surgeon his inclination would be to make a direct attack while bleeding was going on, if necessary, after transfusion. Some surgical speakers referred to the cases seen post mortem—and Fellows had probably all seen them—with a large hole in an artery in the floor of an ulcer, a condition which no medical treatment could be expected to touch; the only chance for such a patient was for the surgeon to cut down and deal directly with the bleeding point, in the same way as a bleeding point was dealt with anywhere else. If there were another discussion on this subject ten years hence, he believed it would be found that practice had swung more and more in the direction indicated by the surgical speakers in the later part of the discussion.

Mr. PATERSON (in reply).

It will be impossible at this late hour to reply to all the speakers. I think that the presence of the physicians who have taken part in the dis-

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cussion is most valuable, and it has been a great help to hear their views and to see how far those views coincide with the views of the surgeons. Such an interchange of thought can only be to the advantage of both.

I agree with Dr. Hutchison that no medical treatment in the limited sense in which that term has been used will stop bleeding from a large vessel, but when I speak of medical treatment, I include Nature, the greatest of physicians. I believe that even the most alarming hæmorrhages, to which such harrowing allusions have been made, will cease if left alone and the patient kept absolutely at rest. In extra-uterine gestation a big artery will bleed profusely and the bleeding will continue until the patient is in such a state of collapse that the bleeding will cease spontaneously, and I have seen this occur in the case of gastric hæmorrhages. Then, if medical treatment be efficiently carried out, there is no reason why the bleeding should recur. Surgery can be undertaken later. Gastric lavage is of great importance, and is often a life-saving measure. Whether hot or cold water is used is immaterial, it is the mechanical relief of the distension of the stomach that matters. Personally, I use warm water. I agree that it requires a good deal of courage to wash out the stomach of a patient who is collapsed by hæmorrhage, but the immediate benefits are so great that I have no hesitation in practising it.

I am particularly interested in what Sir William Willcox has said about morphia, for in previous discussions I have heard physicians object to its use on the ground that it might prevent contraction of a bleeding artery. I regard morphia as of great value, and in acute hæmorrhage it has to be given irrespective of the condition in other parts of the body.

Mr. Burgess said he liked to underpin the arteries going to the anastomosis, and a few minutes later he said if one put in sufficient sutures there would be no bleeding from the anastomosis. With that I cordially agree, and therefore the underpinning, on his own showing, is unnecessary.

Dr. Hurst has said hardly anything with which I can agree, but one thing said by Dr. Hurst is of such extreme importance that I can almost forgive his other lapses from grace. I refer to Dr. Hurst's remarks about washing out the stomach. In the discussion enough emphasis has not been laid on the importance of gastric lavage, both in chronic and acute hæmorrhages. I did not touch on the medical treatment in my opening remarks, as I thought that was best left to the physicians, but if I had done so I would have laid stress on the importance of rest and trust in Nature.

I was surprised to hear Mr. Rowlands' remarks with reference to vicious circle after gastro-jejunostomy; I suggest that Mr. Rowlands should try the effect of anterior instead of posterior gastro-jejunostomy, then he will probably find that bogey will disappear.

Dr. Izod Bennett's remarks about partial gastrectomy were much to the point; I think it is not yet known what is the mortality of partial gastrectomy; possibly it is even higher than the 10 per cent. mentioned. The mortality of gastro-jejunostomy on the other hand is low, therefore it would be a pity if it went out from the Society that partial gastrectomy or resection is the proper treatment for hæmorrhage from duodenal or gastric ulcer.

I congratulate Mr. Gordon-Taylor most heartily on the results which he has obtained in the treatment of hæmorrhage; they are evidence of superior skill on his part. Still, even in that brilliant series the mortality was 8 per cent., and it is not probable it could be brought much lower, for, in addition to risks due to the serious condition of the patient, there are the risks incidental to the nature of the operation performed. The risk of death from

hæmorrhage from gastric ulcer is under 5 per cent., perhaps only 3 per cent., and few surgeons will claim that the risk of partial gastrectomy is lower than 5 per cent.

It has been said that gastro-jejunostomy is an indirect method of treating hæmorrhage; that is true, but although to deal with the bleeding point is always the ideal, it is an ideal which is practicable only in a small proportion of the cases. That we must look our enemy in the face, is an aphorism that sounds well, but if applied to the treatment of gastric hæmorrhage will lead to much injudicious surgery.

I am surprised to hear the experience of some of those who have spoken that recurrences of hæmorrhage are so frequent after gastro-jejunostomy. It has not been my own experience. I believe that in the great majority of cases gastro-jejunostomy is a reliable and efficient treatment for hæmorrhage from a gastric or duodenal ulcer, and I submit that I have proved my case by the statistics I have brought forward.

Finally, I would like to read a quotation on this subject of surgical intervention :—

“Surgical intervention is rarely needed in cases of hæmorrhage from acute gastric or duodenal ulcer. When it is called for, gastro-enterostomy, speedily performed, is the surest means of arresting hæmorrhage. Search for the bleeding point is futile, harmful, and unnecessary; search for, and local treatment of, the ulcer is not necessary. Gastro-jejunostomy will without doubt prevent recurrence of the hæmorrhage and lead to rapid healing of the ulcer from which the blood has come.”

These words were written by Sir Berkeley Moynihan in 1903, and I believe that they are as true to-day as they were when spoken twenty years ago.





## Section of Surgery.

### SUB-SECTION OF PROCTOLOGY.

President—Mr. ASLETT BALDWIN, F.R.C.S.

*Clinical Meeting held July 9, 10, 11, 1924, in conjunction with the American Proctologic Society.*

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Mr. ASLETT BALDWIN (President of the Sub-section of Proctology),

after welcoming the American visitors, outlined the arrangements which had been made for the next few days.

Dr. RALPH W. JACKSON (President of the American Proctologic Society).

[IN acknowledging the welcome from the President of the Sub-section (Mr. Aslett Baldwin) to the American visitors, Dr. Jackson referred to the increase of friendly and professional intercourse between England and America during the past ten years. He then continued:—]

Fourteen years ago when I spent the winter in London watching and profiting from the work of the members of the staffs of St. Mark's and the Gordon Hospitals, and when I was not yet a Fellow of the Proctologic Society, I little thought that I would ever have the opportunity of delivering any such address as this, which I regard as an unusual honour.

No doubt you, Members of the Sub-Section of Proctology of the Royal Society of Medicine, will be interested to know how and why proctology in America has developed from unethical practice to its present status of legitimate specialism. This you will learn later from the paper by Dr. Beach who was a Charter Member of our Society and its first Secretary, and who has an intimate knowledge of its history from the beginning.

I assume this interest on your part, because I know that few, if any, of you are proctologists in the American sense, but are general surgeons with special leanings toward proctologic work. Nowhere has proctology become quite so distinct an entity as it has in America, but in England, probably largely due to your special hospitals, it has made a nearer approach to the American idea than in any other country I know of. I well remember how, when I visited Quénu in Paris fourteen years ago, and told him that I hoped to see some of his proctologic work, he turned on me and said almost resentfully, "But I am not a proctologist, I am a general surgeon." I knew something of what he had written and published in our specialty and was of course disappointed. You in England, though not excluding general surgery from your practice, have sufficient interest and pride in your authoritative work in proctology not to

resent the insinuation which the Frenchman thought I had made. This has ever been evidenced by the courtesies which you have extended to those of us who have visited you individually in the past.

I do not think I am wrong in stating that the fact that the American proctologist is a more distinct specialist is a part of our tendency to individualization in every field of endeavour, whether industrial, professional, or otherwise. Professionally this tendency to individualization has reached its consummation in the Mayo Clinic, one of whose staff is a Fellow of our Proctologic Society, but was unfortunately unable to accompany us on our visit here.

There are advantages and disadvantages in such distinct specialization, but this is neither the time nor the place to bring up the arguments for and against. Personally I have a positive leaning towards this specialization and I think most of the American Fellows feel much the same way. It is enough to say, however, that such a tendency is distinctly strong in America, as is shown by the much discussed but ever-growing practice there on the part of members of the medical and surgical professions to enter into group practice and divide their labours. One thing is certain, that no such group is complete without a member who is skilled in proctology, and in proof of this I need only to cite the arguments advanced by Dr. Zobel in his presidential address before our Society.

No doubt too you, Members of the Sub-section of Proctology of the Royal Society of Medicine, will be interested in the present problems, and the future of the specialty in America. At present we have to contend with the utterly inadequate teaching of the elements of proctology in our medical schools, the poor proctologic work of the general surgeons in our hospitals, the intolerance of those surgeons toward a comparatively new specialty, the activities of advertising proctologists and firms of proctologists, the scarcity of legitimate proctologists, and the ignorance of the laity that there is such a specialty. Individual improvement is of course a part of the reason for Fellowship in the American Proctologic Society, but the organization has a higher and an altruistic mission for the future, and that is to solve the many problems and conditions which I mentioned. An unselfish and intensive campaign will do this; this must be carried on also in the Section of Gastro-Enterology and Proctology of the American Medical Association, which corresponds to your Sub-section of Proctology of the Royal Society of Medicine, though covering a broader field.

Incidentally, as I happen to hold also the Chairmanship of this Section of the American Medical Association, I may bring you, also semi-officially, the greetings of that great organization as well as those of the American Proctologic Society.

### **The Evolution of Proctology in America.**

By WILLIAM M. BEACH, M.D. (Pittsburg).

(ABSTRACT.)

AFTER a survey of the evolution of specialism in America, the author described the origin of the American Proctologic Society. He emphasized the peculiar difficulties that were encountered in the early days owing to rectal practice being then almost entirely in the hands of non-qualified charlatans; and he showed how the Society was at last successfully launched in 1898, with Dr. Joseph H. Matthews as its first President and himself (Dr. Beach) as the first Secretary. Since then the Society had steadily progressed and gone ahead. The present occasion marked their twenty-fifth anniversary.

## DISCUSSION ON THE TREATMENT OF HÆMORRHOIDS BY INJECTION.

Mr. H. GRÆME ANDERSON, M.B.E.,

traced the history of this method. First employed by quacks, it was later taken up by American proctologists, and first introduced to this country by Mr. Swinford Edwards about twenty-five years ago. Many different substances had been employed for injection; these included solutions of carbolic acid, quinine and urea, perchloride of iron, &c. Mr. Græme Anderson then described his own technique; he said he employed a special needle and syringe and injected a 10 per cent. solution of carbolic acid containing glycerine. This method of treatment was applicable only in the case of uncomplicated internal hæmorrhoids. About 15 per cent. of cases recurred, but could be relieved by a repetition of the operation.

Dr. CUTHBERT DUKES, O.B.E.

### **PATHOLOGICAL CHANGES PRODUCED BY CARBOLIC ACID INJECTIONS AND MODE OF ACTION OF CARBOLIC ACID.**

THE pathological changes provoked by carbolic acid injections have been studied microscopically in ten patients in each of whom one hæmorrhoid was injected in the usual way, and this injected hæmorrhoid, together with a control not injected, was subsequently removed by operation, in one patient in one day, in the remainder after two, three, four, five, six, seven, twelve, fourteen and twenty-one days. The pieces of tissue were fixed, embedded in paraffin and serial sections cut from the injected hæmorrhoid and control hæmorrhoid of each patient.

The hæmorrhoid removed one day after injection showed the vessels dilated and engorged with blood and the surrounding tissues œdematous, containing much extravasated blood and infiltrated with polymorphonuclear leucocytes. There was clear evidence of active emigration of these through the vessel walls into surrounding tissues. No signs of thrombosis were seen. The chief feature at this stage was the close packing of the œdematous tissues with innumerable polymorphs.

The hæmorrhoid removed two days after injection showed no pathological changes to distinguish it from its uninjected control. It is probable that in this case the piece of tissue removed was not reached by the carbolic acid.

The hæmorrhoid removed three days after injection did not show any noteworthy change in the vessels themselves, but the surrounding tissues were still œdematous, contained extravasated blood, masses of polymorphonuclear leucocytes, some lymphocytes and very many large mononuclear cells. The abundance of these large mononuclear macrophages was the characteristic feature at this period. Many of them were actively phagocytic and had ingested red blood cells or leucocytes. They appeared to be playing the part of scavengers. Proliferation of the fixed connective tissue cells was noticed at the margin. There was no evidence of thrombosis in any of the vessels.

The hæmorrhoid removed four days after injection presented much the same appearance as its predecessor, but the leucocyte infiltration was less conspicuous and the macrophages were relatively more numerous. These large

mononuclear cells were arranged in clusters round the blood-vessels. There was clear evidence of proliferation of fibroblasts. No signs of thrombosis was detected.

The hæmorrhoid removed five days after injection showed commencing thrombosis in many vessels. No intravascular clotting was seen in the control injected hæmorrhoid from the same patient. The polymorph infiltration of the surrounding tissue had almost disappeared but large mononuclears and young connective tissue cells were plentiful.

The hæmorrhoids removed six and twelve days after injection showed very little change as compared with the control.

In the hæmorrhoids removed seven, fourteen and twenty-one days after injection thrombosis was found in each case, but apart from some increase in fibrous tissue and the budding of new capillaries no other significant change was noted.

#### MODE OF ACTION OF CARBOLIC ACID.

Carbolic acid being a powerful irritant to the tissues, initiates an aseptic inflammation characterized by dilatation of the vessels, emigration of leucocytes and transudation of lymph. By these means the alien liquid is diluted and removed; afterwards the inflammation quickly subsides. All the changes observed microscopically represent the effort of the tissues to repair an injury.

The curative effects of injections of carbolic acid do not depend upon any specific action of this chemical substance. The mere introduction of a needle into the hæmorrhoid results in damage to many blood-vessels and in these thrombosis is likely to follow. Moreover, the cellular injury which this poison inflicts on the delicate lining cells of the capillaries is also likely to contribute to intravascular clotting. It is possible, also, that the contraction of newly-formed fibrous tissue may constrict blood-vessels or even occlude them, but little sign of this was noticed in this histological study. Perhaps after longer periods this would have been more evident.

The early inflammatory changes which occur in the first three days do not play any important part in the cure of hæmorrhoids; it is to the secondary changes, in particular to the intravascular clotting, and subsequent fibrosis, that any beneficial effect must be ascribed.

Mr. SWINFORD EDWARDS and Mr. J. P. LOCKHART-MUMMERY continued the discussion.

#### Dr. ALFRED J. ZOBEL (San Francisco)

said that he used the injection method in the case of patients who could not spare the time necessary for operation, since by the former method the patient was able to carry on his business while undergoing the treatment. He believed that a few were cured, but nearly all were relieved, and if recurrence took place patients were usually willing to submit themselves to further treatment.

#### Dr. JACK HALTON (Sarasota, Florida).

With regard to the quinine-urea injection of hæmorrhoids in selected cases, I inject 2 to 5 minims of 5 per cent. quinine-urea solution into the body of the pile in its median line about 3 or 4 mm. from its upper border. Each pile is injected from two to four times, ten days intervening between each treatment,

one pile being treated at each sitting. The entire time required for the cure of a case is from thirty to forty days. A fresh solution is prepared for each case or treatment. I have been using this method for over a period of twelve years with very satisfactory results. There is no sloughing, there is absence of pain and no loss of time is incurred by the patient so far as his or her business is concerned. This work is done in my office, four to five minutes only being required for each treatment. I have had the experience of very few patients returning with recurrence except in the case of those who have discontinued treatment after the first or second injection, the reason for this failure to return being that the piles gave them no trouble after one or two treatments and that they thought further treatment unnecessary. I use this method only in carefully selected cases of uncomplicated internal piles, never in cases of old indurated or muco-cutaneous pile or in those of external thrombotic pile. My records show over 500 cases successfully treated during the past twelve years. Selected cases of uncomplicated hæmorrhoids, when treated with quinine-urea combined with careful technique, can be thoroughly cured, and my experience convinces me that they remain cured.

Mr. W. S. PERRIN

gave the results of treatment in 380 cases of uncomplicated piles by injection with 10 per cent. carbolic acid solution. Of eighty cases two years after treatment about 50 per cent. were absolutely cured; and a little over 30 per cent. were substantially cured, but complained of trivial symptoms. Four cases of recurrence had needed re-injection and five had eventually to be operated upon. He had come to the conclusion that the method was applicable in most cases of uncomplicated piles provided they were not of very large size.

Mr. CECIL ROWNTREE

said he considered that this method gave admirable results in those cases of uncomplicated hæmorrhoids in which hæmorrhage was the chief or only symptom.

Dr. F. L. JELKS (Memphis) and Dr. SAPHIR (New York)

opposed the injection treatment very strongly and described some disastrous cases which had come to them after being treated by injection.

Mr. LIONEL E. C. NORBURY

said he had seen complicated fistulæ result from the injection of hæmorrhoids which were partly internal and partly external, and thought that this variety of hæmorrhoid should not be so treated.

Mr. GREME ANDERSON (in reply)

pointed out that bad results usually occurred from using too strong a solution. He said that, after trial of various solutions, he had adopted a 10 per cent. solution of carbolic acid as the best.

## DISCUSSION ON PROLAPSE OF THE RECTUM.

Dr. LOUIS J. HIRSCHMAN (Detroit)

described his operation for prolapse of the rectum, which depended upon shortening the mesentery of the sigmoid through an abdominal incision. This was illustrated on the epidiascope. He claimed that the results of this operation were very satisfactory.

Mr. LOCKHART-MUMMERY

admitted the difficulty of treating this complaint and described his own operation.

Dr. W. M. BEACH (Pittsburg).

The physics of prolapse is most interesting. The theory that prolapse is hernial seems to me tenable. The pelvic vault is an inverted cone through the apex of which the rectum passes, and this is its weakest point. Again we must take into account the intra-abdominal pressure which keeps up a warfare with atmospheric pressure, especially when reinforced by the abdominal muscles in action. A fascia relaxed from overstraining becomes more overstrained; and finally it gives way to the *vis a tergo* resulting in prolapsus recti or prolapsus ani as well as invagination. There are many potential cases or cases of partial intussusception which may be overlooked.

In addition, we must consider the recto-sigmoid strait as a vulnerable point in the production of prolapse. This is a structure analogous to the pylorus in that it has a function of contractility and expansion according to the faecal urge. It is the point at which invagination usually occurs; it is an important factor in constipation and a likely location of cancer, being the junction between the hind gut and the rectum proper by coalescence—similar to the ano-rectal line—being about 2 in. in extent.

## TREATMENT.

The treatment of course should be directed towards correction of the ptosis. In extreme cases surgical measures should be invoked. The method proposed by Dr. Hirschman seems rational, but I think it should be complementary to the Moschcowitz technique, as the latter appears more nearly to restore the pelvic fascia, in other words, the combined procedure will more certainly prove effective.

Another type of case which I will mention is the potential or partial prolapse, which does not require the adoption of surgical measures. This may be overcome by the use of topical applications through the proctoscope; to combat the condition the patient should be inverted and the proctoscope introduced, through which 50 per cent. solution of magnesium sulphate should be injected. This agent will readjust the gut wall. If this procedure be repeated for three to five days and a binder be adjusted, most of these cases will be relieved. A good adjuvant to use is the extract of belladonna, given in applications of  $\frac{1}{4}$  gr. three times daily. The danger of stricture following excision of anal prolapse, either mucosal or of the layers, can be obviated by using fine silk sutures placed  $\frac{1}{8}$  in. apart, the long ends being left so that their removal is simplified. I would not hesitate to treat all adult cases by this method.

Mr. W. E. MILES

gave an outline of the pathology of prolapse of the rectum and described a first, second, and third degree. He said that the first and second degrees were easily treated by a variety of well-known operations, but that the third degree (in which the peritoneum was included in the prolapse) was exceedingly difficult to treat, and he doubted whether Dr. Hirschman's operation would be successful in this stage.

Sir CHARLES GORDON-WATSON

said that prolapse of the rectum in children was curable by dieting and careful regulation of habits without any operation.

Dr. RALPH W. JACKSON (President of the American Proctologic Society).

Dr. Hirschman's method of supporting a prolapsed rectum seems to be valuable, but it would be improved if supplemented by the Moschcowitz operation. Some years ago I read a paper before the American Proctologic Society entitled "Adult Rectal Prolapse: Two Cases and a Contrast." In the first and more favourable of the cases I did several operations without success and finally amputated; this cured the prolapse, but the *cul-de-sac* was not obliterated, and the contained coils of intestine promptly turned forward and produced an enormous hernia of the posterior vaginal wall through the vulva, which I cured by opening the abdomen and doing a Moschcowitz operation. In the second and much less favourable case and without any preliminary operations, I promptly cured the whole condition by doing a Moschcowitz operation. The contrast between these two cases was so great that I became convinced of the validity of the hernial theory as to the causation of such prolapses; and since then I have had no reason for doubting that it is not the main cause. I concluded my paper as follows:

"The operation of first choice for major types of rectal prolapse in adults is *cul-de-sac* closure, to be followed and supplemented, if need be, later, by amputation and perhaps some plastic work on the elements of the pelvic floor."

There is another form of rectal prolapse in infants and small children which has been overlooked in to-day's discussion. I have been fortunate enough to have many cases of this to deal with, and have tried all types of operations. The chief cause of this form of prolapse appears to be muscular relaxation and atrophy, especially of the sphincters. It is usually much aggravated, if not caused by, want of sufficient care in the child's home, and by forcing the child to defæcate in the sitting position. If this can be prevented, the child taught to defæcate in the reclining position, and the repeated divulsion of the sphincters prevented for a sufficiently long time, the case can be cured usually without operation. Where it has been necessary to interfere surgically, I have had the best result by introducing a fine gauge bronze wire around the anus and burying it there. In other words, the patient is given a wire sphincter. If aseptically introduced, it is surprising how many weeks or months it may be left in. Eventually of course, it has to be removed, but the annular cicatrix left remaining is a decided reinforcement of the sphincters. With good hospital or good home care there will



be recovery from nearly all such prolapses without surgery, but this will not always be so in poor homes; indeed, it may be asserted that the necessity for operation in infantile prolapse is in direct proportion to the ignorance and poverty of the parents.

## DISCUSSION ON PRURITUS ANI.

Dr. J. G. MONTAGUE (New York)

gave a cinematograph demonstration on pruritus ani; he showed in this film the various histological changes which he had noticed in the peri-anal skin, leading finally to a sclerosis of the connective tissues of the corium and to a cellular infiltration in the papillæ round the end-bulbs of Meissner. In considering the treatment, he described the under-cutting operation which he had devised, based on a modification of Sir Charles Ball, Lynch and others: he said he performed this operation under local anæsthesia, and undermined the peri-anal skin and mucous membrane of the peri-anal canal through eight separate incisions. He demonstrated the special thin-bladed scissors which he had devised for this operation. The operation was combined with treatment by vaccines of *Bacillus coli* and *Staphylococcus albus* which were the usual infecting organisms.

Dr. Montague also gave a diagrammatic representation of his conception that anal pruritus might be evidence of visceral disease due to misreference of sensation; and he emphasized the necessity for thorough general local examination, including sigmoidoscopic examination, in every case of pruritus ani.

Dr. RALPH W. JACKSON (President of the American Proctologic Society).

I have always felt that there was nothing in the whole field of proctology which was so unsatisfactory from the therapeutic standpoint as pruritus ani. The very multiplicity of theories as to its causation, and of methods of treatment is the best proof of this statement. All of the ætiological explanations have contained elements of truth, and all of the methods of treatment have given some relief and occasional cures. Dr. Montague does not claim, either in his recently published book or in the surprising cinematographic demonstration he has given us to-day, to have done anything more than to have brought harmony out of discord. He has furnished a framework on which can be hung, almost without exception, all the known facts about pruritus ani and has resolved the treatment into a trilogy, viz.: (1) correction of distant pathology, which by misreference of sensation causes the symptom of itching about the anus; (2) lessening or blocking the sensibility of the nerves and nerve centres which are responsible for that misreference; and (3) treatment of the secondary infections and pathology in the affected area.

Sir CHARLES GORDON-WATSON

called attention to the enlarged anal papillæ often present in pruritus ani. A certain proportion of cases were cured by cauterization of these papillæ.

Dr. CUTHBERT DUKES, O.B.E.

**The Histology of Anal Papillæ, with Special Reference to Nerve Endings.**

THE anal papillæ project into the lumen of the anal canal a short distance below the muco-cutaneous junction: above this point the rectum is lined with columnar epithelium; below, the anal canal is lined with stratified epithelium continuous with that of the skin round the anal orifice. The anal papillæ are always covered with stratified epithelium which in hypertrophied papillæ is represented by a thinner layer than usual. Beneath the epidermis the cutis vera is composed of dense connective tissue, more open and reticular in its deeper part where it merges with the central core of subcutaneous tissue. Looped capillary vessels pass from the central core to the superficial layer of cutis vera; lymphatics originate near the surface and join larger vessels in the reticular part of the corium. Hairs, sebaceous glands and sweat glands have not been observed.

This histological study was undertaken to determine whether or not these papillæ are supplied with nerve fibres and nerve endings. Three methods have been used. (1) Frozen sections treated with osmic acid and tannic acid (Azoulay's method). (2) Gold chloride impregnation of the fresh tissue subsequently cut with the freezing microtome (Löwit's method). (3) Methylene blue staining of fresh tissue subsequently treated with molybdate of ammonia, the sections being cut with the freezing microtome (Dogiel's method). With the osmic acid technique numerous nerve fibres were found scattered through the core of the papillæ and arranged as a plexus round the blood-vessels. With the gold chloride and also with the methylene blue technique these fibres were traced to end bulbs and tactile corpuscles beneath the epidermis. The terminal ramifications of these nerve fibres varied somewhat in appearance but the commonest form is that represented in the illustrations shown: this resembles closely the tactile corpuscles found in the tongue and skin of the hand. The lamellated connective tissue capsule, an expansion of the perineurium of the nerve, encloses a core of nucleated cells continuous with the endoneurium of the nerve, within which the axis cylinders terminate in complex arborescence. A comparison of the frequency of these nerve endings in anal papillæ and in the remainder of the anal canal showed them to be equally plentiful in each. On the other hand, no such structures have been observed in the lining of the rectum.

This investigation has definitely proved the existence of sensory nerve endings in hypertrophied anal papillæ.

**DISCUSSION ON THE TREATMENT OF CARCINOMA OF THE RECTUM.**

Mr. W. ERNEST MILES

said that of the three methods of spread of cancer of the rectum, by direct extension, by the blood-stream, and by the lymphatic system, the last was of the greatest importance. He then discussed in detail the lymphatic system of the rectum in its various divisions, intramural, intermediate, and external, and illustrated his remarks with diagrams and

drawings of microscopic preparations. He pointed out that the intramural plexuses of fine lymphatics did not have a very long course along the length of the bowel, but were partially separated into systems each occupying a short zone of bowel, and draining by larger trunks to glands lying outside the bowel. The most important of these was the ano-rectal group of ten to fifteen glands lying just above the levator ani. In all the specimens of cancer of the rectum he had examined these glands were infected by cancer cells, even when no other deposits were to be found. From the ano-rectal glands cancer might spread in three directions—upwards along the retro-rectal glands, laterally to the glands in the pelvis as far out as the obturator vessels, and downwards to the fat and glands in the ischio-rectal fossæ. From the retro-rectal glands the cancer cells might find their way to the paracolic glands higher up, so that the latter were not necessarily infected in series; it followed that one could not argue from the fact that the paracolic glands attached to a short section of excised bowel were free from cancerous deposit, that other glands higher in the paracolic chain were also free. Photographs of two specimens excised at operation were then shown. The first exhibited a secondary spread of the cancer upwards, laterally and downwards—widespread dissemination in spite of the fact that it was an early case. In the second the primary growth had extended around three-quarters of the circumference of the bowel. Microscopic examination showed that the cancer cells had already passed through the muscular coat of the bowel and lymphatics. Spread was taking place, though judged by clinical standards the growth was an early one, since it was freely movable. Fixation of a growth in the rectum indicated the passage of the cancer process through the fascia propria of the rectum and also indicated a widespread dissemination and a grave prognostic outlook, even after a very radical operation. Mr. Miles maintained that the mesentery of the rectum and sigmoid in cancer of the rectum were comparable to the axillary contents in the case of cancer of the breast, and that it (the mesentery) should be removed as widely as possible in all cases, early or late, whether the contained glands were obviously infected or not. Nothing less was performed in breast cancer and nothing less should be performed in cancer of the rectum. The perineal operation failed in this important respect; he therefore practised and advocated the abdomino-perineal operation.

#### MR. J. P. LOCKHART-MUMMERY

said that he performed the operation advocated by Mr. Miles twenty years ago, but had abandoned it since 1913, except for the high cancers at the recto-sigmoidal junction. For cancer in the latter locality he recognized that it was the logical treatment and still performed the operation, but for other cancers of the rectum he had abandoned the abdomino-perineal operation in favour of the perineal, which was attended with a lower mortality and was applicable to a larger proportion of cases. There were many advantages in having a standardized operation in which the various technical details did not vary. He performed the perineal operation in two stages. The first was a colostomy, and the second, fourteen days later, was the excision, in one piece, of the rectum and as much of the sigmoid and its mesentery as could be brought down together with the anus and surrounding skin, the contents of the ischio-rectal fossæ, and parts of the levatores ani. This operation removed all the glands primarily infected and some of those in the second group. He used spinal anæsthesia in conjunction with gas and oxygen for men, with "twilight sleep" for women.

The patient was able to get up fourteen days later and to go home within a month. The mortality of this operation, taking all cases, private and hospital together, was 9·3 per cent. The recurrence figures were surprisingly low. During the years 1915-19 sixty-five cases were submitted to this operation, of whom thirty were alive and well five years later. He thought that these figures were as least as good as those obtained with the abdomino-perineal operation.

**Dr. W. H. KIGER (Los Angeles)**

said he considered that Mr. Miles's operation was the best and most thorough that had yet been devised. He had been working on different lines and had had a considerable degree of success from the slow "cooking" of rectal cancers by low temperature heat. This method was based on the lower degree of resistance to heat of the cancer cell compared with the normal, and was advocated only for inoperable or for very early cases, when the patient refused an operation involving colostomy. He used electric cauteries introduced through a water-jacketed speculum. (These instruments were exhibited.) Although many of his cases appeared to be quite hopeless, he had six or eight patients alive and well five years after the destruction of a rectal cancer by slow cooking.

**Mr. SWINFORD EDWARDS**

said that while recognizing the merits of the abdomino-perineal operation from a theoretical or pathological point of view, he held that very good results could be obtained from a much less drastic procedure. He could recall three cases he had dealt with many years ago by local excision through a linear proctotomy with complete restoration of the bowel and a normal anus. He was uncertain of the fate of the third patient, but he knew that two were alive and well to-day. The perineal operation which he had largely practised (a modification of Kraske's) aimed at retaining the normal anus whenever possible—a very important matter from the patient's point of view. This operation, whilst obviously very incomplete from a pathological aspect, had given a cure-rate at three years of 45 per cent. in proven cancers, with a mortality of only 5 per cent. After seeing Mr. Miles's diagrams, he was convinced that he must almost invariably have left many secondary cancerous deposits in lymphatics. From this he could only conclude that these secondaries must in many cases wither up and become inert. He felt very strongly that when so great a degree of success attended these very limited operations, it was unjustifiable to submit patients to a procedure with a mortality of 20-30 per cent., even in expert hands, however perfect that operation might be from a pathological standpoint.

**Mr. G. GREY TURNER (Newcastle-upon-Tyne).**

As a contribution to the subject under discussion I propose to show you lantern slides of a few specimens removed by the lower route, with the after-history of the patients concerned.

The first slide is a very typical growth of the lowest part of the rectum and the anal canal. It was removed from a patient, aged 54, in October, 1915. The man was suffering from a very bad chronic bronchitis and was generally in poor health. A rather limited sacral excision was performed and the operation completed by making a sacral anus. The man made an uninterrupted recovery, and at the present time, eight years and

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eight months since the operation, he is without recurrence, though his general health is not good. This is a type of case with which we have all been for long familiar, and it exemplifies the result of the old-fashioned perineal operation, which as probably all agree may best be used in this type of case.

The next slide shows the rectum from a patient who was 61 years of age at the time of the operation. A higher sacral excision was performed and he made a quite satisfactory recovery. Death occurred sixteen years afterwards without a suggestion of recurrence of the disease. He was always a feeble man and was troubled by some prolapse at the sacral anus, but his general health remained good until the time of his death at the age of 77.

I ought to say in connexion with these specimens that the diagnosis has been confirmed by repeated microscopic examination; but in the two cases which have died no post-mortem examination was obtained, although there was nothing in the history of the fatal illness to suggest any recurrence, either local or distant.

The next slide shows a similar growth limited to a small section of the bowel just at the level of the peritoneal reflexion. This also was removed by the sacral route from a patient aged 60. He made an easy recovery and remained in good health until thirteen years afterwards, when he died quite suddenly from a cerebral attack associated with enlarged prostate and retention of urine.

I now show the specimen from a case in which the prospect appeared particularly unfavourable, for the growth was situated high in the rectum, and the patient suffered from aortic disease associated with attacks of angina. It was originally intended to perform colotomy only but the patient took the anæsthetic so well that an extension of the operation was considered reasonable, and he was turned over and the bowel excised by the sacral route there and then. He not only made a straightforward recovery, but has remained well up to the present time now thirteen years and ten months since the date of the operation. He still attends business and has enjoyed better health since the operation than he did before.

The next slide represents a growth removed with a portion of the vagina from a woman, aged 45. It was a sacral excision and the patient has managed her incontinent anus so well that she has been able to attend to the whole of her household duties ever since and has not hesitated to leave home for a day's outing or an annual holiday. I have seen her from time to time, and now eleven years and eight months after the operation she is in excellent health in every way.

The next patient was a labouring man, operated upon at the age of 59 years. In this case I explored the abdomen in the first instance and performed colotomy. Three weeks later the rectum was excised by the sacral route. The man made a perfect recovery, and has been able to follow his occupation as a blacksmith's striker ever since. As you will see, the growth is situated just about the level of the peritoneal reflexion and completely surrounded the bowel.

The picture on the screen shows a large growth situated in the lower part of the rectum, just above the anal canal. It was excised from a patient, aged 47, who complained very much of urinary symptoms. In the first instance the abdomen was explored and a left inguinal colotomy performed. A month later he was so much better that the growth was excised by the sacral route. The prostate was infiltrated, and it was necessary to remove a slice of the gland together with the seminal vesicles and part of the vasa. Unfortunately, the urethra was opened, and though it was repaired a urinary fistula soon formed and persisted for five weeks, when it closed spontaneously. Ultimately, the patient completely recovered, and now nine years after the operation he remains in good health and without sign of recurrence.

From the summary of results which I now show on the screen you will see that out of forty-three cases who survived the operation performed more than

five years ago, twelve patients are known to be alive and well without recurrence, and two patients of the series have died without recurrence sixteen years and thirteen years after operation. This small series of after results gives a percentage of 32.5 who have passed the five-year period without recurrence. Even among cases that recurred there are many patients who secured a very large measure of relief, and I want to draw your attention to the case which is illustrated on the slide. It was one of an advanced growth which I removed by the sacral route. The patient was perfectly well for four valuable years, during which she was able to look after her family when they most needed her help and guidance. At the end of this time a recurrence became obvious, and she rapidly went downhill and died six months afterwards. The slide shows the large amount of perirectal tissue which has been removed, and on the left the outline shows the way in which the growth has directly invaded the mesentery.

Personally, I shall never be content with an operation which so mutilates the patient as to destroy continence, but I am satisfied that conservative operations will seldom be possible unless the first symptom happens to be hæmorrhage, and unless the case is seen just after this has declared itself. On the screen I show you a specimen from such a case. The growth is quite small, and is limited to the mucous membrane, but it has been carefully examined by two independent competent pathologists, and there can be no doubt as to its malignant nature. The involved section of bowel was removed and the continuity of the bowel restored; and the patient is now alive and well and with perfect control four and a half years since the date of the operation.

Dr. FRANK C. YEOMANS (New York)

referred to the cancer campaign held in America with the object of getting earlier diagnosis of rectal and other cancers. A few ulcers of the rectum were of doubtful malignancy; in such cases he said he preferred the small risk of cutting out a portion for microscopy to the needless risk of submitting a patient with a benign ulcer to a cancer operation. A great improvement in the results of the more drastic cancer operations had recently set in owing to improvements in anæsthesia and operative technique. In New York the abdomino-perineal operation was standardized for cancers at the recto-sigmoidal junction, Balfour's operation for higher growths, and the perineal operation for lower growths. His own rule was to perform a perineal operation when the growth could be reached by the finger, and he had several cases in which the patients were alive and well seven or eight years after this operation.

Sir C. GORDON-WATSON

said he agreed with Mr. Lockhart-Mummery. The abdomino-perineal operation, while undoubtedly superior on theoretical grounds, should be reserved for high growths, while for lower growths the perineal operation was the only one justifiable on clinical and practical grounds.

## Dr. D. C. MCKENNEY (Buffalo)

said he held a brief for the "glorified" Whitehead operation for those patients who refused any operation involving colostomy.

## Dr. CURTICE ROSSER (Dallas)

discussed the question of radiation of rectal cancers either by X-rays or radium. In America, Howard Kelly, of Baltimore, and Quirke, of New York, both claimed cures by radiation alone, and a higher cure-rate from the use of radiation combined with surgery than with surgery alone. Whilst going through the records of his own cases he had come, he said, to the conclusion that growths originally benign frequently underwent malignant change.

## Mr. W. B. GABRIEL

gave some statistical records from 130 cases operated on at St. Mark's Hospital by various surgeons. The operative mortality for all cases was 16 per cent. Sepsis accounted for the majority of deaths; four deaths occurred from urinary fistulæ, two from pneumonia, and one each from a variety of other causes, including post-operative obstruction due to a loop of small intestine being strangled in an opening in the peritoneum. One hundred and nine cases had left the hospital, of whom 44 had since died: 80 per cent. of these deaths occurred within three years. Of the known sites of recurrence the pelvis and liver were the commonest. The oldest patient who still survived the perineal operation was now 82, and had been operated upon thirteen years ago. The operability-rate had risen during the last five years from 44 per cent. to 54 per cent., due, perhaps, to more careful selection of the cases admitted to the wards. The percentage of cases alive and well, and showing no sign of recurrence, was 18 at three years after operation and 11.5 at five years.

## Mr. MILES (in reply)

congratulated Mr. Lockhart-Mummery on the results he had obtained from the perineal operation. His own experience had been less fortunate. He had performed fifty-nine perineal operations with one death and fifty-six recurrences. It was this experience which had led him to perform the abdomino-perineal operation and to treat cancer in this situation as cancer elsewhere was treated, by the removal of all the removable tissue which was likely to be infected. His mortality in 116 operations was 25 per cent., but of fifty-three cases operated upon since the war with improved anæsthesia (spinal anæsthesia combined with gas and oxygen) the mortality was 9.8 per cent. It was noticeable that ether had been administered in two cases since the war, and in both of these cases the patients died. The operation he advocated demanded careful preparation, suitable anæsthesia, a well thought-out technique, and good after-treatment. In his hands it had yielded 50 per cent. of cures.

Mr. LOCKHART-MUMMERY (in reply)

said he thought that much depended upon the type of growth, and that what was chiefly lacking was some means of determining beforehand the degree of malignancy. He believed that some cases could not be cured by the most radical procedure, while others of apparently the same type might do admirably after a local excision.

Mr. ASLETT BALDWIN (President of the Sub-Section)

remarked that he had performed the abdomino-perineal operation successfully upon a lady of over 76 years of age. She came back to see him when she was 85 years, accompanied by her husband, whom she had married since the operation.



## Section of Surgery.

President—Mr. CYRIL A. R. NITCH, F.R.C.S.

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### Hypernephroma of Spleen from a Female, Unmarried, aged 45.

Shown by ARTHUR EVANS, M.S.

*Report on Examination of the Spleen, by Dr. Braxton Hicks.*

Weight, 8 lb. 2 oz.

Externally, numerous small pinhead thickenings resembling tuberculosis. Elsewhere yellow-white caseous looking masses. On section a large yellow-green degenerating area 2 in. in diameter is present in the middle of the spleen and other white glistening vascular deposits of varying size microscopically resembling "*Hypernephroma*" ("renal carcinoma"); and one assumes these are secondary deposits.

### Lymphoma in Vermiform Appendix.

Shown by ARTHUR EVANS, M.S.

*Report on Examination of the Appendix by Dr. Theodore Joeke.*

THE appendix was considerably enlarged and a firm elastic lump was protruding from the appendix into the cæcum. It was impossible to pass even the finest probe from the cæcum into the appendix.

*Section.*—Shows a very well-marked hyperplasia of lymph tissue, especially at the root of the appendix, by which the lumen of the appendix has been entirely shut off from the cæcum. There is a mild degree of dilatation of the lumen and atrophy of the mucous membrane in the lower half of the appendix.

*Diagnosis.*—Simple localized lymphoma. No evidence of malignancy.

### An Appendix Epiploica.

Shown by ARTHUR EVANS, M.S.

THE tip of this was inflamed and adherent to the bladder, causing pain on defæcation. At the site of adhesion was a small thin-walled cyst containing clear fluid.

[December 5, 1923.]

**Melanotic Sarcoma.**

Shown by W. SAMPSON HANDLEY, M.S.

SECTION of primary recurrent growth, stained by hæmatoxylin and eosin, for comparison with naked-eye specimen, also shown.

**Excision of Body of Pancreas ; Specimen Shown.**

By W. SAMPSON HANDLEY, M.S.

THE body of the pancreas, showing chronic pancreatitis, cyst formation and calcareous deposit, removed by operation from the patient, who is exhibited.

**Teratoma Testis : Two Specimens.**

Shown by P. H. MITCHINER, M.S.

THE first was removed in January, 1917, from a Serbian soldier, aged 24. The patient was alive and well in 1922.

The second was removed in June, 1919, from a man alive and well in 1923. The pigmentation of the tumour should be noted. The cord constituents are also obscured by œdema.

**Specimen of Carcinoma Testis.**

Shown by P. H. MITCHINER, M.S.

THIS was removed ten days ago from a patient, aged 19. It was thought to be a teratoma, but histological examination showed that it was a spheroidal-celled carcinoma.

**Aneurysm of Popliteal Artery.**

By Sir HUGH RIGBY, K.C.V.O., M.S.

*History.*—G. C., male, aged 59.

October 9, 1923 : Swelling, noticed by patient for three years, has recently increased in size. Two months ago, pain and numbness in the leg, followed by appearance of discoloured areas in the lower part of the calf and on the foot.

History of alcoholism, but no syphilitic history.

*Signs.*—Pulsating swelling in the upper part of the right popliteal space, extending upwards beneath the hamstrings. Pressure on the common femoral artery obliterates pulsation in the tumour. No pulsation felt in the arteries at the ankle. Vessels distinctly atheromatous. Trophic skin patches present on the foot, and ecchymoses present about the ankle and toes.

X-ray examination shows calcareous deposit in the superficial femoral and aneurysmal sac. (Prints exhibited.)

*Operation I.*—October 26, 1923 : Ligature of superficial femoral artery in Hunter's Canal.

October 29, 1923 : Dry gangrene, commencing in the toes. This gradually spread up to the ankle and up the anterior aspect of the leg.

November 16, 1923 : Line of demarcation well marked. Dry gangrene of toes and foot with an extension upwards to the upper third of the anterior surface of the leg. Brawny swelling of the calf. Aneurysm consolidated.

*Operation II.*—November 20, 1923 : Amputation of the leg in the lower third of the thigh. Flap method. Healing by first intention.

### **Stomach and Intestines from a Case of Congenital Obstruction of the Third Part of the Duodenum.**

Shown by C. MAX PAGE, D.S.O.

BALLOONING of the first and second parts of the duodenum is present. There is a narrowing in the middle of the third part of the duodenum, which seems to be determined by the fold of peritoneum covering the superior mesenteric vessels.

On January 22, when the infant was six days old, an operation was performed and adhesions were divided, which relieved the obstruction. A month later there was a recurrence of symptoms, and death took place four weeks afterwards.

### **Stomach and First Part of Duodenum from Case of Hypertrophic Pyloric Stenosis removed Seventeen Days after Rammstedt's Operation.**

Shown by C. MAX PAGE, D.S.O., M.S.

OPERATION undertaken for relief of persistent vomiting when child was five weeks old. Symptoms relieved. Death from gastro-enteritis seventeen days later. The specimen has been opened through the line of surgical incision into the pyloric sphincter.

### **Specimen of Scirrhus Carcinoma of Stomach (Localized Leather-bottle type).**

Shown by H. W. CARSON, F.R.C.S.

PATIENT, a female, aged 59.

*Clinical History.*—Nausea and vomiting immediately after meals, about once a week for the last four years. Pain after taking food, lasting several hours, has been present during the past year. Loss of weight recently. Tumour in left hypochondrium.

Very scanty spheroidal-celled growth is to be seen in the scirrhus mass towards the serous surface.

**Specimen of Leio-Myo-Sarcoma of Fallopian Tube.**

Shown by H. W. CARSON, F.R.C.S.

PATIENT, a female, aged 60.

*Clinical History.*—Abdominal pain to the left and below umbilicus for nine years. Menses ceased twenty years ago: no loss since. Swelling felt per rectum.

This type of sarcoma is usually found in the uterus. In this instance it is a question whether the growth has not been extended by continuity from the body of the uterus (which was buried in adhesions and not removed). In support of this contention the dilatation and inflammatory thickening of the isthmus of the tube should be noted.

**Specimen from Case of Adenoma of Thyroid.**

Shown by H. W. CARSON, F.R.C.S.

PATIENT, a female, aged 18.

*Clinical History.*—Lump in thyroid region for four and a half years; no symptoms.

*Description of Specimen.*—Acinar and solid cubical-celled non-colloidal adenoma of thyroid, with central hæmorrhage and necrosis, the so-called foetal adenoma.



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PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

EDITED BY  
SIR JOHN Y. W. MACALISTER  
UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

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**VOLUME THE SEVENTEENTH**

SESSION 1923-24

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ACTION OF THERAPEUTICS AND PHARMACOLOGY



LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1924

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## Section of Therapeutics and Pharmacology.

President—Professor A. J. CLARK, M.D.

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### DISCUSSION ON "THE USE OF INSULIN IN GENERAL PRACTICE."

Chairman—Sir WILLIAM HALE-WHITE, K.B.E., M.D. (President of the Society).

Professor HUGH MACLEAN

(in opening the Discussion) said that the value of insulin in the treatment of diabetes was now definitely established. Cases in which it was said not to be a success were heard of, but probably such cases were unsuitable for the remedy. A few days ago he saw a patient who had had glycosuria for some time, was wasting badly, and obviously going downhill. The medical attendant having examined his urine, concluded that the man had diabetes, and gave insulin, but it did not seem to have any great effect. Later it was found to be a case of carcinoma of the stomach associated with some glycosuria. Such cases tended to vitiate the statistics in regard to insulin. The question before the meeting was, could insulin be used successfully by the doctor in general practice? It was not an easy question to answer. He had now seen about 100 cases, and had watched them carefully, and he was inclined to say, without hesitation, that the general practitioner was competent to use this remedy in his practice. True, it might be a dangerous remedy: like fire, it was a good servant but a bad master. Still, if the doctor realized the possible danger, and followed the directions carefully, he could use it safely. In cases in which control was good, the most important point seemed to be to go slowly.

His (Professor MacLean's) own first twenty cases went on very well, nearly all the patients having certain slight symptoms because the insulin was deliberately pushed to provoke them. The twenty-first patient had attended a hospital in the Midlands, and was very ill and extremely thin. He had been treated by his doctor for diabetes for at least three years. The sugar in his urine was 7 to 8 per cent.; blood-sugar was 0.4 per cent. The absence of acetone aroused suspicion, but he was put on to 10 units of insulin twice a day and was also dieted. He seemed all right the next day, and when he had taken 40 units the blood-sugar was 0.2 per cent. On the following morning, though he had his usual dose, the blood-sugar was not estimated, and at 1.30 p.m. he suddenly became unconscious in the ward, the corneal reflexes had gone and his breathing was bad. Yet ten minutes before that he was smoking a cigarette. He was brought round, and lived for some time afterwards. There

## 2 MacLean: *The Use of Insulin in General Practice*

was evidence that he had had pus in the ears, and when he died he was found to have a subdural abscess. This showed the necessity of very great care being taken in some of these cases. If that man had not been treated drastically when he showed insulin symptoms the 50 units he had had would have killed him.

If insulin were given in too large doses, certain symptoms would result, but they were usually of mild type; they varied not only in different patients, but at different times in the same patient. The most frequent symptom was flushing of the face, followed by some pallor. There might be sweating, either local or over the whole body. Many complained of a sense of constriction or of tightness about the chest. Sometimes, however, there were no objective symptoms at all. At other times there was a feeling of apprehension of impending disaster and of nervousness without special cause. In some there was a certain tremor, and an attempt to write resulted in only a scrawl. If the dose had been large and the symptoms severe, there might even be unconsciousness, or motor and sensory aphasia. He had not seen convulsions in man though they were often produced in experiments on rabbits.

The symptoms referred to might come on at almost any time after the insulin had been given; frequently they appeared from four to six hours after the use of the remedy. Some time ago the idea was held that these symptoms were dependent on the state of the blood-sugar, and that when the blood-sugar percentage reached a certain minimum, the symptoms came on. He did not think that idea could be maintained now, as some patients had symptoms when the blood-sugar was but slightly below normal, and others when it was much more reduced had no symptoms at all. Still, in a rough-and-ready way, one could say that symptoms came on when the blood-sugar was low, but that they did not appear when the blood-sugar remained at the normal level or higher.

There was no doubt that in nearly every case the diabetic was restored more or less to a normal condition by the administration of insulin, and certainly after its use the patient felt much better and more energetic.

Fortunately, the treatment of the symptoms of hypoglycæmia was generally a simple problem—namely to give sugar by mouth, if the patient was conscious. If unconscious, the difficulties of giving sugar by the stomach-pump were well known, and it was desirable, then, to inject glucose into a vein. The sugar should be prepared beforehand, otherwise the patient would probably be dead before the solution could be got ready. It was most helpful to give an unconscious patient a subcutaneous injection of adrenalin, i.e., 1 c.c. of 1 in 1,000 solution; or 1 c.c. of pituitrin. Sometimes sugar might be injected into the subcutaneous abdominal tissues. As a rule, the patient came round quickly.

Very important was the particular type of case chosen for the treatment; here lay the chief danger, and it was a weighty point in the present discussion. If a case was unsuitable the danger in giving insulin might be great. For the general practitioner the most important thing was to choose a suitable case. If a patient who was much wasted, with glycosuria and ketosis, had not received much benefit from dietetic treatment, insulin should be given.

A type of case sometimes seen was that in which there was very marked glycosuria. There might be no symptoms, the patient might be stout, middle-aged or elderly; he did not feel up to the mark, but did not know what was the matter with him, except that he felt weak about the legs. Sometimes these patients had a large amount of sugar in the urine. It might be said

by some that insulin was not needed in such a case, as there were no definite symptoms, and the marked glycosuria was apparently doing no harm. That he (the speaker) regarded as a short-sighted view; though there was little tendency to coma, it was difficult to control this condition, for subsequently it was often associated with certain eye changes, sometimes leading to blindness, also great nerve irritation and neuritis, muscular cramp, &c. It was probable that this blindness was an even greater tragedy than coma. In those cases it was desirable to adopt the treatment. A patient might be passing 3 per cent. to 4 per cent. of sugar or more without symptoms but one should weigh the probability of the development of such symptoms as had been mentioned. He did not think it was possible, in any given case, to prognosticate what might happen in four or five years. But it was certain that when eye changes had developed, little good could be done. In such, one should try to reduce the blood-sugar by giving insulin if this could not be done by dietetic means.

The general impression among the profession was that insulin did not cure diabetes, but the view of the public was that it did. He had not yet seen evidence of cure in a bad case of the disease. In some in which there had been much sugar, the condition had cleared up under insulin, but similar results had ensued in the past from dietetic treatment. On the other hand, so long as the patient was taking insulin he kept in good form.

It might be thought that if a patient were kept well nourished and the urine free from sugar, it would allow the pancreatic cells to regenerate, or at least to get into better functional condition, so that gradually less insulin might be needed, and a more generous diet allowed. There might be something in that, but in the last six months he had seen little evidence of it; and experience of Allen's dietetic treatment did not suggest much hope in this direction, for in spite of initial success, sooner or later a little sugar again began to appear in the urine, and the blood-sugar went up. A modification in diet generally checked the downward trend for a time, but then there was another relapse. That was, he believed, the experience of all who had treated large numbers of cases of diabetes by dietetic means. One man with a large experience said he thought that the difference due to careful dieting was that instead of patients living about four years on an average, they now lived about six years.

A type of case against which one had to be on guard was that in which sugar appeared in the urine because of a leak in the kidney—the so-called *renal glycosuria*. The administration of insulin to such a case might end in disaster. Renal glycosuria simply meant that the kidney exuded sugar at a lower level than normal. It could be determined whether a particular patient was suffering from renal glycosuria by estimating the blood-sugar after the administration of glucose, though it was not always possible for the man in general practice to do it. But there was a means of ascertaining it in some cases without doing a blood-sugar test. In this condition the glycosuria frequently came on an hour or two after taking starchy food. After food, the blood-sugar normally rose to some extent, and it was because of this rise that sugar passed into the urine. The test was to give 2 oz. of glucose, and to get the patient to empty his bladder two and a half to three hours later. If the specimen passed after that contained no sugar, the patient had probably glycosuria of the renal variety. As a rule there was no great difficulty, because, whatever the nature of the case, it was well to "go slow," and if that precaution

were observed it was almost impossible for the general practitioner to go far wrong.

Having decided that a case was suitable for insulin, it was necessary to decide how the insulin should be given. There were different methods, and he thought it right there should be. In hospitals and institutions, where the patients were largely under control, various methods might yield equally good results; but it was not easy to apply those methods in general practice; some safe method should be adopted which could be followed up. He was convinced that the best method was to fix a definite diet and adhere to it; then begin with insulin in a small dose, and gradually work up. The diet should not be too large. It was useless to rely on an unsuitable menu continuously for a private patient; he would tire of it. There must be protein, fat and carbohydrate. There should be about  $\frac{1}{2}$  grm. of protein per lb. of body weight, but something must be allowed for the kind of work in which the patient was engaged. There must be a large proportion of fat, so as to afford the required calories. Ten to 15 calories per lb. of body weight were required; the ordinary patient, not doing very much work, required 1,700 to 2,000 calories; he should have 70 grm. or so of protein, and the remaining calories must be made up of carbohydrate, and a large amount of fat. Frequently it might be necessary to cut down the carbohydrate at a later stage. He himself often began with as much as 40 grm. of carbohydrate in twenty-four hours; 140 grm. of fat was a good quantity. It was desirable to use ordinary food stuffs. He recommended bread, cheese, milk, cream, &c., as well as eggs. It was advisable to give the greater part of the carbohydrate immediately after the insulin. The diet was spread over four meals: breakfast, lunch, tea and dinner; insulin should be given before breakfast, and before dinner at night, preferably half an hour before those meals. What he frequently ordered was as follows: for breakfast at 9.30, 2 oz. bacon, 1 oz. white bread,  $\frac{1}{2}$  oz. butter,  $\frac{1}{2}$  oz. cream, 4 oz. vegetables; equalling 20 gr. carbohydrate, 63 of fat, 15 of protein, of a total calorie value of 730. At lunch practically no carbohydrate should be taken if possible; this gave a better chance of the blood-sugar being kept down and the urine kept free of sugar. For lunch, beef tea, 4 oz. white fish,  $\frac{1}{2}$  oz. of starch-free bread,  $\frac{1}{2}$  oz. butter, 1 oz. cheese, 1 oz. cream, 2 oz. vegetables. That gave 3 grm. of carbohydrate, 22 grm. of fat, 35 grm. of protein, the carbohydrate being restricted to the vegetables. For tea, a little tea,  $\frac{1}{2}$  oz. of butter and a "kalari" biscuit (Callard). Before the last meal, dinner, the second injection of insulin was given. The meal should consist of clear soup, 4 oz. meat, 1 oz. white bread,  $\frac{1}{2}$  oz. cheese,  $\frac{1}{2}$  oz. butter, 4 oz. of vegetables, and coffee. This sort of dietary gave satisfaction, and could be endured comfortably for a long time. The dinner yielded 25 gr. carbohydrate, 40 of fat, 30 of protein, an approximate total calorie value for the day of, roughly, 1,830 calories.

Not more than 5 units of insulin should be taken at first, once in the morning, once at night. On the third day, 10 and 5 units could be given, and on the fifth day 10 and 10, and on the eighth day 15 morning, 10 at night. After the ninth or tenth day, if the patient was still passing sugar, one could give 15 and 15 units. The administration should be as gradual as this or more so, because it took some time for matters to settle down. Sugar might continue to be passed after a fortnight's treatment with insulin, but after that it might diminish, and in some cases disappear altogether. It was practically impossible for the patient to have serious symptoms if one proceeded slowly with the treatment. All the urine for the day must be collected and examined qualitatively for sugar; if

possible, it should be also examined quantitatively. When the amount of sugar passed in twenty-four hours began to get very small, then before each injection of insulin the patient should be asked to pass urine, and that specimen should be tested for sugar. If the patient had been passing sugar in the twenty-four hours, but did not pass sugar immediately before the attempt to inject, it was advisable not to give such a large injection as previously. If these precautions were observed he did not think there would be danger from the use of insulin in general practice.

Was it necessary that everyone using insulin should be able to estimate the blood-sugar? He believed a patient could be successfully treated with insulin without this estimation of the blood-sugar; in some cases it was necessary to do it, but not in all. One sometimes saw patients who had even as much as 0.3 per cent. of sugar in the blood becoming free from glycosuria. Blood-sugar estimation was then essential; and in every case when it was done it gave a better indication of the condition present. If one found the patient's urine became sugar-free when he was receiving a certain dose of insulin—say 15 units—should one continue with the 15 units and risk symptoms, or reduce the dose? Experience seemed to show that one could safely go on with the 15 units. The process of increasing the insulin had been gradual, and symptoms, even if they came on, would not be at all severe. Slight symptoms were an advantage, for they showed that the correct dose had been reached. He did not think it advisable to go on to very large doses in order to render a patient quite sugar-free, and he never cared to give more than  $1\frac{1}{2}$  c.c. twice a day; if sugar was still passed, it might be necessary to modify the diet, by cutting down the carbohydrate.

It might be impossible to prevent some slight symptoms coming on; he had seen some peculiar cases in that respect. One patient was in coma, but did very well with insulin. After a period she began to have curious attacks in the night time. She was having 15 units insulin before breakfast and 15 before dinner, and the specimen of the twenty-four hours' urine had 1 to 2 per cent. of sugar; therefore he did not think the night symptoms were due to the insulin. However, it was found that urine passed immediately after the symptoms showed no sugar, and the symptoms were relieved by giving glucose. They were obviously insulin symptoms.

In treating a patient, what ought to be our ideal? Was one to aim at getting the urine sugar-free and the blood-sugar normal in every case? Yes, if one could. As in Allen's treatment, one went on until the urine became sugar-free, and until the blood-sugar got as near normal as possible. But cases would be met with in which this could not be done with reasonable doses, and these patients, though still passing sugar, felt much better and did quite well.

If, in general practice, insulin were used as he had indicated, he thought there was no reason to fear bad results. It was most important however, to emphasize the necessity for care, and a knowledge of insulin treatment was absolutely essential for every medical man who wished to use this remedy. If the practitioner was not prepared to take the trouble to acquire this knowledge, then he should leave insulin alone. On the other hand, insulin might be safely used if he carefully followed the above directions.

#### Dr. GEORGE GRAHAM

said that there was no doubt now that insulin was of very great value in the treatment of diabetes, but it must be recognized that it was only an addition to the treatment and could never replace the treatment by

dietetic restrictions. He had heard of patients who had been passing quite a lot of sugar and eating a good deal of starchy food who had been given 10 or 20 units of insulin a day. The doctor had complained that the insulin was useless as it did not abolish the glycosuria. Such a line of treatment was bound to fail and would only bring discredit on insulin. Joslin's remark was most apt: "Insulin is a treatment for the wise and not for the foolish patient." Although those who had been working in this country with insulin for the last eight months all agreed about its value they were not agreed as to the best method of using it. It was a question about which opinion was divided in Canada and the United States. Banting and the Toronto school, Allen and his co-workers believed as did he (Dr. Graham) that the aim of the physician should be to bring down the fasting value of the blood-sugar to within normal limits. Wilder and Woodyat on the other hand thought that the immediate clinical condition of the patient was the most important point and did not mind if the patient passed sugar and had a high blood-sugar. Joslin occupied an intermediate position and aimed at keeping the urine sugar-free but not at bringing the blood-sugar down to normal.

The aim of C. F. Harris and himself, working in the medical professorial clinic at St. Bartholomew's Hospital, had been to see whether insulin could be so used as to allow the patient's islands of Langerhans to recover as much as possible and also to see to what extent they could recover.

They took a man who had had diabetes for three years. It was true that the initial attack was a mild one but during the last three years he had steadily become worse and worse in spite of much careful treatment. They failed to prevent him from passing sugar in spite of two hunger days, two egg and vegetable days, and two more hunger days. His blood-sugar under this treatment never fell below 0·20 per cent. The diet was then increased up to 1,260 calories: Protein 53·2 grm., fat 109·5 grm., and 16 grm. of sugar in the form of vegetables. On this diet he excreted between 25 and 33 grm. of sugar a day, and the blood-sugar was between 0·23 and 0·24 per cent. The dose of insulin was small at first, and was increased up to 9 units, with the result that he ceased to excrete sugar. The blood-sugar was estimated at two-hourly intervals throughout the thirteenth day and the highest point was 0·20 and the lowest point 0·085 per cent. The important observation was made that the blood-sugar was lower at the end of the twenty-four hours than at the beginning. They therefore decided to wait and see whether the morning blood-sugar would continue to decrease. After fifty-one days the fasting value of the blood-sugar was 0·12 per cent., and never rose above that point during twelve hours of the day, although only 3 units of insulin were given. This man continued to improve and was eventually able to eat 25 grm. of sugar without passing sugar, although the form of the blood-sugar curve was abnormal. Now, eight months after the beginning of treatment, he was keeping very well and he had gained 2½ stone in weight. He was able to assimilate a diet of about 1,800 calories and including 36 grm. of sugar, and only required 10 units a day. Except for about two weeks in July the blood-sugar was always below 0·14 per cent. They had tried to carry out this procedure on all their other patients. Altogether forty patients had been treated at St. Bartholomew's Hospital and elsewhere.

BLOOD-SUGAR ON LEAVING HOSPITAL.

| Type              |     |     |    | Normal between<br>0·10 and 0·13<br>per cent. |    | Raised<br>above 0·13<br>per cent. | Died |
|-------------------|-----|-----|----|----------------------------------------------|----|-----------------------------------|------|
| Severe            | ... | ... | 23 | ...                                          | 19 | 4                                 | 2    |
| Coma              | ... | ... | 3  | ...                                          | 2  | 0                                 | 1    |
| Mild and moderate | ... | ... | 8  | ...                                          | 8  | 0                                 | —    |
| Recent onset      | ... | ... | 6  | ...                                          | 6  | 0                                 | —    |

Some of these cases were more severe than the first case. The procedure had had to be altered as it was not possible to keep the patients in hospital so long owing to the number of other patients awaiting admission. In the case of private patients it was essential to send them out of the nursing home as soon as possible, on the ground of expense. The dose had therefore been increased cautiously, starting with 10 units in the morning and increasing it up to 25 or 30 units in the morning and 15 to 25 units at night. The diet had been kept constant as a rule and was the same as that used for the first patient. In order to work quickly in this way it was essential to have the patients either in hospital or in a nursing home where the nurses and house physicians understood the dangers of insulin administration. It was not safe to treat patients at home with these large doses, but it was possible to use a small safe dose and wait for the good result to take place gradually. The blood-sugar fell to normal after a variable period in all the patients except four. One of these was a girl, aged 5, whose blood-sugar after two weeks in a home and four weeks outside was still 0.22 per cent. Now, after four months, it had fallen to 0.13 per cent., although the dose was only 3 units a day. One patient, who was very thin and wasted, always had severe reactions although the blood-sugar was not very low and in this case the blood-sugar did not fall below 0.16 per cent. after six weeks' treatment. A third patient, also very thin, had improved greatly in health in many ways and had gained 18 lb. in weight. However her morning blood-sugar had never been lower than 0.19 per cent., although in her case the insulin was increased to 60 units in the morning and 10 units at night. This patient three times had severe symptoms, becoming unconscious, and had required an injection of adrenalin, 15 min., which always restored her to consciousness very quickly, and a small amount of sugar. It was clear that, in spite of her great improvement in health, the condition of the islands of Langerhans had not been improved. The fourth man had symptoms suggestive of hypoglycæmia with twitching of the hands and unconsciousness about three hours after a dose of insulin. He was given adrenalin, 15 min., and glucose intravenously. He recovered consciousness quickly but relapsed again in about three hours and again in eight hours, although he was passing sugar in the urine and had had no more insulin. The man died a week later and was found to have advanced tuberculosis of the lungs. Although, therefore, it was possible to bring down the blood-sugar to normal in most cases it was not possible to do so easily in every case and might perhaps be impossible. The correct treatment after the blood-sugar had fallen to normal was not yet by any means settled. The attempt was being made to reduce the dose of insulin cautiously so that the patient would only require a dose of 10 or 15 units once a day. When this had occurred the diet was cautiously increased, the carbohydrates being avoided at first. The indications for reducing the dose were the occurrence of symptoms of overdosage in a mild form. When this occurred, the evening dose was reduced by 5 units and later on the morning dose was reduced by 5 units. When the blood-sugar came down to normal quickly the dose might have to be decreased rapidly in order to prevent severe symptoms. The mild cases usually improved rapidly with quite small doses of insulin and the blood-sugar fell to normal. In this case the diet might be increased more rapidly.

One question of great interest was whether there was any real recovery of the islands of Langerhans. In the first patient treated there was no question that he had recovered a great deal. Another patient, who required 55 units of insulin to lower the blood-sugar to normal, now had symptoms of overdosage



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although the dose was only 15 units. Another who required 60 units and even then still had a blood-sugar of 0.14 per cent., and practically no symptoms of over-dosage, now had quite severe symptoms with 25 units, the diet being constant in each case. Another man who came into hospital in a state of coma, under Dr. Langdon Brown, required 100 units of insulin per day for some days, and now assimilated a diet of 1,800 calories and required 35 units of insulin. He thought that these results indicated that a considerable degree of recovery had taken place, even in very severe cases, though whether it would continue was uncertain at present.

In patients with coma the value of insulin was shown in a few hours; in patients with severe diabetes in a few weeks; but in the case of patients who had only had diabetes for a few weeks or months the value of insulin would not be known for several years. He had begun treating six such patients. The carbohydrate in the diet had been kept very low and a small dose of insulin has been given each day varying from 1 unit for a baby of 2 years to 3 units for a boy of 11 years and 10 units for an adult man.

Whether the disease would progress in these patients as it had done in similar patients treated with only dietetic restrictions could only be known in five to ten years, but the experiment was worth trying.

Professor MacLean stated that patients with renal glycosuria did not always pass sugar and that their condition could therefore be easily detected by giving them 2 oz. of sugar and by waiting to see whether sugar was present some three hours after the dose of sugar had been given. This was quite contrary to his (Dr. Graham's) experience as the great majority of such patients passed sugar all the time. Professor MacLean's test could not detect all cases of renal glycosuria and would often lead to the diagnosis of a mild form of diabetes mellitus.

### Professor J. J. R. MACLEOD (Toronto)

said he felt himself to be in a somewhat false position to try and take part in a debate on the clinical application of insulin; he would not touch that aspect, except to answer some of the remarks of previous speakers.

A point of considerable importance in connexion with the action of insulin in the treatment of diabetes was the glucose equivalent of each unit. By this was meant the number of grammes of glucose which were metabolized by each unit. Several clinical observers had tried to measure this in the case of diabetic patients by comparing the glucose balance (i.e. the difference between the intake of glucose from all sources in the food and the output in the urine), of different periods in which the dosage of insulin varied. When the dose of insulin was increased, the diet remaining constant, more sugar was utilized by the patient and if the amount of this improvement in grammes were divided by the number of extra units given, the glucose equivalent was obtained.

The results had been found to vary considerably, no doubt because of the varying extent to which the remaining islet tissue in the patient's pancreas could produce insulin. This suggested that it would be advisable to use de-pancreatized dogs for the determination of the equivalents, and Dr. F. N. Allen had completed a series of observations which had yielded results that were not only much more constant than those obtained in the clinic, but were also important because they showed that the equivalent varied considerably according to the amount of glucose that was being metabolized at the time. The animals after pancreatectomy had been kept alive on a diet of 500-600 grm. lean meat and 100-150 grm. cane sugar with injections of

insulin daily. Two of them survived in excellent condition for over four months, when they developed peculiar symptoms which ended fatally, extreme fatty changes being found, post mortem, in the liver. The results obtained in the case of one of the animals were the following:—

GLUCOSE EQUIVALENTS PER UNIT OF INSULIN IN DE-PANCREATIZED DOGS<sup>1</sup>

Dog 1. Fed 600 grm. meat and 100 grm. sucrose.

| Insulin per diem<br>(clinical units) | Glucose equivalents<br>(on different days) |
|--------------------------------------|--------------------------------------------|
| 20 ... ..                            | 5.8, 5.8                                   |
| 24 ... ..                            | 4.1, 4.0                                   |
| 32 ... ..                            | 3.5, 3.5, 3.6, 3.3, 3.7, 4.1               |
| 36 ... ..                            | 3.0, 3.1, 3.1                              |
| 38 ... ..                            | 3.4                                        |
| 40 ... ..                            | 3.0, 3.3, 3.1 3.0                          |

Fed 600 grm. meat with varying quantities sucrose.

(A) 20 units insulin given daily.

| Sucrose per diem | Glucose equivalents |
|------------------|---------------------|
| 50 grm. ... ..   | 3.5, 3.9            |
| 100 " ... ..     | 5.8, 5.8            |
| 150 " ... ..     | 8.4, 8.1, 6.9, 6.9  |

(B) 32 units insulin given daily.

|              |          |
|--------------|----------|
| 50 " ... ..  | 2.4, 2.6 |
| 100 " ... .. | 3.7, 4.1 |
| 125 " ... .. | 4.9, 4.8 |
| 150 " ... .. | 4.5, 4.9 |

Three conclusions seemed to be justified:—

The glucose equivalent of the same dose of insulin given on different days varied within a comparatively narrow range.

(2) The equivalents for small doses of insulin were larger than for large doses.

(3) The equivalents became larger when the amount of ingested glucose was increased.

By plotting these results on ruled paper a curve was obtained which would probably be useful in the assay of insulin, and it was remarkable how closely the results obtained on different dogs had corresponded. From a clinical standpoint it was of interest that the high equivalents of small doses of insulin would lend support to the practice of combining a control of the diet with the administration of small doses of insulin in mild cases of the disease. The explanation of the result was, no doubt, partly that when relatively large doses of insulin were given, much was excreted without having an opportunity to act on glucose. But this could not be the only explanation, since, as was shown in the table, the equivalent also rose when, with constant dosage of insulin, the amount of ingested glucose was caused to increase. This would seem to indicate that the action of insulin on glucose metabolism proceeded according to the same laws as those governing enzyme action, namely, that the amount of substrate acted on was not a linear function of the amount of enzyme unless when there was a large relative excess of substrate. When this was not the case, the extra amount of substrate acted on by each unit-increase of enzyme became progressively less and less, following a logarithmic curve.

It was clear that these results also showed that it was useless to attempt to measure the glucose equivalence of insulin on patients unless the level of the glucose balance was taken into account.

On comparison of the average of all the clinical results with the lowest obtained in the observations on dogs, a decided difference was observed, namely,

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two for patients and over three for de-pancreatized dogs. The difference was possibly due to the fact that relatively much more of the total glucose was derived, in patients, from protein. Wilder, Boothby and others had given other evidence to show that glucose from protein was not so readily metabolized as that derived from carbohydrate sources.

He (Professor Macleod) would call attention to the interesting fact, pointed out by Banting, Campbell and Fletcher, that treatment with insulin in long standing cases of diabetes removed the glycosuria but not the diuresis.

With regard to local irritation of the skin at the site of injection, there was some evidence to indicate that small traces of foreign protein in the insulin preparations might be responsible. It had sometimes been observed that such irritation occurred with insulin prepared from the pig but not with that prepared from the ox or *vice versa*, a fact which would indicate something of the nature of a specific susceptibility of some patients to certain animal proteins. When such irritation was observed with a certain brand of insulin it was at least worth while to change to another made from a different animal.

He had been interested in what he felt was a difference of opinion in the case of some speakers as to whether patients should be given sufficient insulin to keep them sugar-free and the blood-sugar normal, or whether they should be allowed to carry a certain load of glycosuria constantly. He would not presume to say which method was the correct one, but he would think it was better to proceed on the basis that the blood-sugar should be kept at about the normal. There was not sufficient evidence to justify the assumption that an excess of blood-sugar could be regarded as a physiological state, and it was somewhat risky with such a substance as insulin to allow the amount of the blood-sugar to deviate far from the normal.

In order that insulin might be used in general practice to the greatest advantage and to minimize the risks attendant upon overdosage, the policy had been adopted in Ontario of offering free of cost to practitioners a two-day course of instruction at the Toronto General Hospital. This had been arranged for by the Ontario Medical Association so that groups of practitioners attended in rotation. The instruction had been of value not only in showing how and when insulin should be used, but also in pointing out the possibilities of dietetic treatment and the management of cases without the aid of constant quantitative measurements of the percentage of sugar in the blood and urine.

### Dr. E. P. POULTON

said the problem of the use of insulin in general practice resolved itself into the question of what was aimed at; were we trying to get the blood-sugar to normal, or did it not matter if the patient was still passing traces of sugar and the blood-sugar was above normal? He thought it was safe, if the process were carried out slowly. It was best to begin with 5 units before breakfast, and again before the evening meal, and after a day or two, 10 units and then 15. After the sugar had disappeared from the urine—which might occur quickly—there was often a large margin of blood-sugar still remaining before there was anything in the way of symptoms, and one should look out for those symptoms to know when the level of blood-sugar aimed at had been reached. Sometimes symptoms would come on before the blood-sugar fell to the physiological level. By grading the dose so as just to avoid symptoms, the body would tolerate more and more insulin, and the blood-sugar would fall to the physiological level. Dr. W. W. Payne and he (Dr. Poulton) had determined the blood-sugar of a diabetic patient, and then

carried out the treatment by gradually increasing the dose of insulin, and after hypoglycæmic symptoms had been produced they had tested the blood-sugar and found that it had fallen to a satisfactory low level.

What Professor Macleod had said about a short course on the subject to the practitioners in Ontario was excellent, and if the same plan were adopted here it would enable a great number of patients to undergo the treatment safely.

As to diet, he thought it better not to give starch, but he had given as much as 32 ounces, 5 per cent. vegetables per day. That applied particularly to young subjects, because in their case there was always a slight possibility of regaining tolerance. Further, on the score of expense, it was desirable to keep down the insulin dosage by means of diet restriction. In older subjects, especially if there was high blood-pressure, it was desirable to allow rather more carbohydrate and less protein.

Professor MacLean seemed to think the only unsuitable cases for this treatment were those of renal glycosuria, which could not be regarded as diabetes mellitus at all. With that he (Dr. Poulton) agreed. He had come across a case of glycosuria with quite high blood-pressure in which the patient had been benefited by the insulin treatment. The feeling of ill-health and tiredness disappeared, even in elderly patients treated with insulin, in whom the diabetes might be regarded as secondary. An elderly patient with blood-pressure of 200, passing a moderate quantity of sugar, was kept sugar-free on 5 units of insulin a day, and he said his general feelings were much improved.

As to separating renal glycosuria from diabetes mellitus, the only safe way was to determine the blood-sugar tolerance curve, but the practitioner would be safe in treating any case of glycosuria with insulin, if the patient had had definite symptoms of diabetes mellitus at some time or other.

In treating diabetic coma, it was best to begin with 40 units, repeating this every four to six hours, and having every specimen of urine tested for sugar. An intravenous injection might be given at the onset, say 20 units. Fluid in bulk should be given by the nasal tube. In one case the patient never fully regained consciousness and eventually died. The blood-sugar remained quite high (0.45 per cent.), so there was no hypoglycæmia. However, the blood urea was considerably above normal; yet, naked eye, the kidneys looked normal. In another case to which he had been called, the urea in the cerebro-spinal fluid was high (0.15 per cent.). He suggested that there was a mixed condition of uræmia and ketosis in some cases of diabetic coma.

**Professor MACLEAN (in reply),**

said there appeared to be some slight misapprehension as to his views on diet; it arose from his remarks at an earlier period, when he was forced to say something about insulin, and did not want to be very definite in case the general practitioner should take the remedy up and get bad results. He therefore said, at that date, it was better for the practitioner to keep some sugar in the urine. What he did now was what he did many years ago in the case of the Allen treatment, he got the urine sugar-free, and then got the blood-sugar as near normal as possible.

In the method he had roughly sketched, what happened was that the patient became sugar-free on gradually increasing doses of insulin. If that did not happen, one cut the carbohydrate down to a minimum, and reduced the protein, and then one arrived at a point at which the urine was sugar-free. He did not know what was meant by indicating "normal blood-sugar" as a

definite amount. In all cases of normal people he had examined, the blood-sugar varied from 0.09 to 0.17, and fluctuated up and down after a meal. Probably a reason for that was stimulation of the pancreas and such-like. After carrying out many hundreds of observations he could say there was no such thing as a straight line indicating the blood-sugar. The suggestion he made was to ensure safety.

He agreed that as regards the administration of insulin it would be excellent for something to be carried out here in instructing the general practitioner on the lines adopted in Canada.

As to whether carbohydrate should be used in the diet, there was some difference of opinion apparent; he used it as a safety measure to begin with, and he advised the general practitioner to do so. But if he did not get results he cut down the carbohydrate and reduced the protein, and eventually the patient had no sugar in the urine. He was convinced that on the lines mentioned the general practitioner could get good results with the treatment without blood-sugar estimations.

With regard to renal glycosuria, he had examined nearly 200 cases of renal glycosuria in the last five or six years. That day, for instance, he had examined a man's urine when he came to the hospital; there was no sugar. He was given a dose of sugar, and he then passed 2 per cent. sugar in the urine; two and a half hours afterwards a specimen of urine passed did not contain any sugar. It was very common for this to take place. After a meal the blood-sugar index was higher. Whether or not a patient had glycosuria depended on the fact that at a certain point the sugar might pass through the kidney. A patient with renal glycosuria might be constantly passing sugar, and that meant that his threshold value was much lower than normal. If a patient, after a dose of sugar passed sugar in the urine two and a half to three hours afterwards, and if subsequently the sample passed did not contain sugar, it was not likely to be a case of diabetes; it was probably a case of what was known as renal glycosuria. At any rate it would be most unsafe to give insulin to such a patient. This type of renal glycosuria was exceedingly common—much more common than was generally recognized.

## Section of Therapeutics and Pharmacology.

President—Professor A. J. CLARK, M.D.

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### Some Aspects of Cardiac Dyspnœa: a Review.

By Professor FRANCIS R. FRASER, M.D.

AT the present time the problem of cardiac dyspnœa is much more in need of carefully ascertained facts than of a review, but if some of the aspects of the problem are briefly considered it may stimulate the collection of facts and aid in the understanding of their significance. It is probably unwise to attempt a definition of what is meant by dyspnœa. As ordinarily used the word has an objective significance, but it is possible that those conditions in which the individual has the sensation of difficulty in breathing, even in the absence of objective disturbances, should be included in the term. In the meantime we are in a position to discuss only the conditions in which the normal sequence, rhythm and character of the acts of respiration are disturbed, whether the subject is conscious of the disturbance, or of distress, or of neither.

#### *Dyspnœa in Healthy Subjects.*

To approach the consideration of cardiac dyspnœa intelligently, it is necessary first of all to recall what is known of the normal control of respiration and of the causes of dyspnœa in healthy subjects. The work that has been done has followed the general plan of investigating the influences that affect the respiratory centre in the medulla of healthy subjects, and this centre is regarded as being capable of automatic activity and of rhythmically sending the efferent impulses to the muscles that perform the movements of respiration. The activity of this centre is known to be affected by four important influences that are independent of volition: (1) The pressure of carbon dioxide in the blood supplied to the centre; (2) the H-ion concentration of the blood supplied to the centre; (3) the pressure of oxygen in the blood supplied to the centre; and (4) the afferent impulses reaching the centre.

(1) As shown conclusively by Haldane and Lorrain Smith [1] increase in the carbon dioxide pressure in the air breathed, and hence in the arterial blood, is more powerful than oxygen deficiency to cause hyperpnœa. The hyperpnœa that results is associated with increased depth of the respiratory movements rather than with increased rapidity. It is still a matter of doubt as to whether this hyperpnœa is due to the specific action of increased carbon dioxide pressure on the respiratory centre, or to the increase in the H-ion concentration that results from the increased carbon dioxide pressure, and to the action of this raised H-ion concentration on the centre. That acids added experimentally to the circulation fluid supplied to the centre, so that the H-ion concentration is raised, will cause increased activity of the centre and hyperpnœa, suggested that increased carbon dioxide pressure acts merely as any other acid. A number of investigators, notably Hooker, Wilson and Connett [2], have however shown that when a given rise of H-ion concentration is produced

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by carbon dioxide the centre is stimulated more effectively than when it is produced by an acid such as hydrochloric acid. It is possible, as is suggested by the work of Jacobs [3], that this is due to the greater permeability of cell membranes at the centre to carbon dioxide than to other acids. In the meantime it is necessary to recognize that the centre reacts to raised carbon dioxide pressure by the production of hyperpnœa, that this hyperpnœa is due to increased depth rather than increased rapidity of respiration, and that it is associated with acidœmia and increased carbon dioxide pressure in the arterial blood.

(2) An increase in the fixed acids in the blood from whatever source must reduce the alkali available for carrying carbon dioxide, and so raise the pressure of the carbon dioxide and produce hyperpnœa. This hyperpnœa will lower the carbon dioxide pressure in the alveolar air, the frequent renewal causing the carbon dioxide pressure to approach that of the atmosphere, and so in the arterial blood, until a balance is struck between the increased pressure due to diminished alkali in the blood and the lowered pressure due to the hyperpnœa. The hyperpnœa due to addition of fixed acids is similar to that of carbon dioxide excess in that it consists of increased depth rather than rapidity of respiration, and is also associated with acidœmia, but it differs from it in that it is associated with lowered carbon dioxide pressure.

(3) Deficiency of oxygen in the air breathed causes in healthy subjects increased ventilation of the lungs, and this increase, at any rate in long continued oxygen deficiency, seems to be due to an increased rapidity rather than to an increased depth. Haldane, Meakins and Priestley [4] have shown that if severe oxygen deficiency is maintained for any length of time rapid shallow breathing results. This hyperpnœa of oxygen deficiency is accompanied by a fall in the carbon dioxide pressure in the alveolar air, and so also in the arterial blood, and was at one time thought to be due to the production of lactic acid in muscles functioning with an insufficient oxygen supply, as was suggested by Boycott and Haldane [5]. Such an increased production of fixed acids would, as we have seen, result in acidœmia and reduced carbon dioxide pressure in the alveolar air and arterial blood. Low carbon dioxide pressures are actually found, but more recent work, notably that of Haldane, Kellas and Kennaway [6], has shown that in the hyperpnœa accompanying uncomplicated oxygen deficiency the condition is really one of alkalœmia and not of acidœmia, and that therefore oxygen want must be able to stimulate the centre directly. The hyperpnœa that results will lower the carbon dioxide pressure in the alveolar air and arterial blood so that there is an alkalœmia. Uncomplicated oxygen want, therefore, in healthy subjects results in hyperpnœa with lowered carbon dioxide pressure and alkalœmia, and the hyperpnœa is associated with increased rapidity rather than increased depth of breathing.

(4) That afferent nervous impulses can affect the activity of the centre, apart from any influence of the consciousness, and cause rapid breathing, has been shown in animal experiments by the effect of exposure to phosgene gas [7], and by the effect of oil globules or starch grains [8] carried by the blood stream to the lung. In these examples the rapid breathing was dependent on the integrity of the vagus nerves. That the centre can be influenced by impulses from higher centres is seen in the effect of volition and of the emotions, and possibly also in the disturbed rhythms noted after encephalitis lethargica which disappear during sleep. The most important effect of afferent impulses is seen, however, in the Hering-Breuer reflex,

whereby towards the end of each inspiration an impulse is conveyed to the centre so that this movement is inhibited and the centre initiates an expiration, and towards the end of expiration a similar impulse causes the expiratory movement to be inhibited and inspiration results. The afferent limb of this reflex is in the vagus nerves. The Hering-Breuer reflex appears to regulate the extent of movement and so to modify the effect of the rhythmic automatic activity of the centre. It depends on the carbon dioxide pressure at the centre, for an increase in the carbon dioxide pressure causes a more extensive movement in inspiration before this phase is inhibited and expiration sets in, while in the absence of sufficient carbon dioxide shallow breathing or apnoea results. Haldane [9] has suggested that the variations in the extent of movement necessary, before the inhibition affects the centre, to produce the next phase in the respiratory movements, depends on the activity of the centre rather than on the strength of the inhibition conveyed to it, so that a centre which is stimulated by carbon dioxide will produce more extensive movements before the reflex can inhibit them, while a centre failing from long continued lack of oxygen, or less active because of lowered carbon dioxide pressure, will be more easily affected by the inhibitions, and shallow breathing will result.

It is obvious that gross lesions in the region of the respiratory centre may affect the activity of the centre, and although this may be of little importance to the subject of cardiac dyspnoea, it is necessary to remember that the condition of the centre itself may be altered as the result of disease, or of drugs, so that the response of the centre to chemical and nervous influences may be altered. This is probably seen in the slow shallow breathing of morphia poisoning.

Mechanical interference with the full completion of the acts of respiration must alter the depth and rhythm of the breathing, and the work of Haldane, Meakins and Priestley [10] has shown that, as the depth is mechanically limited, the frequency is increased. In cardiac dyspnoea such mechanical interference has to be considered.

To sum up, then, it is recognized that, if mechanical interference is eliminated, disturbances of the respiratory mechanism can result from: (1) Alteration in the pressure of carbon dioxide in the blood supplied to the respiratory centre, and that increase in this pressure causes increased breathing, with acidæmia and raised carbon dioxide pressure; (2) alteration in the fixed acids in the blood, so that increase in the acids causes increased breathing, with acidæmia and lowered carbon dioxide pressure, and (3) alteration in the oxygen in the blood supplied to the centre, so that deficiency causes increased breathing, with alkalæmia and lowered carbon dioxide pressure. Further, oxygen lack causes mainly increase in the frequency of breathing, while increased carbon dioxide pressure and increased fixed acids cause mainly increase in the depth of breathing. It is obvious that changes at the centre in the carbon dioxide pressure, H-ion concentration and oxygen pressure will be effective, no matter how these are brought about in pathological conditions. It further appears probable that the form that any particular disturbance takes, depends on the combined effects of the Hering-Breuer reflex and the activity of the centre.

#### *Cardiac Dyspnoea.*

On turning to the work on cardiac dyspnoea, among the investigations reported on clinical cases are to be found the three types of chemical change that have been considered. Peters and Barr [11], and Campbell, Hunt and



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Poulton [12] have reported cases of the common form of cardiac dyspnœa, accompanying heart failure with mitral valve disease, in which they found an increased carbon dioxide pressure in the arterial blood and an acidœmia. The carbon dioxide pressure was higher in the arterial blood than in the alveolar air, and Poulton and his co-workers make the suggestion that this is due to changes in the lungs, such as œdema and fibrosis, which prevent the free interchange of carbon dioxide between the blood and the alveolar air. They find a similar condition of the blood in the dyspnœas due to pulmonary or bronchial conditions. The authors do not maintain that the carbon dioxide retention is the sole cause of the cardiac dyspnœa, and it would be surprising if it were, in view of the fact that the breathing in these cases is of the rapid shallow type, while simple carbon dioxide excess would produce increased depth. It is possible that the mechanical effect of œdema of the lung tissue in preventing full expansion may be the cause of the type of breathing being modified. Peabody [13] has suggested that the lowered vital capacity seen in these patients is due to the engorgement of the pulmonary circulation resulting in an erectile condition of the engorged lung. Such a condition might also mechanically restrict the respiratory movements, and cause shallow rapid breathing when increased depth is to be expected. Although the methods used both by Campbell, Hunt and Poulton, and Peters and Barr may be open to criticism, Poulton's results in pulmonary cases, which also show acidœmia and a raised carbon dioxide pressure, are so convincing that there can be little doubt that if there are marked lung changes in cardiac dyspnœa, raised carbon dioxide pressure and increased H-ion concentration may be expected, and would then be factors in the production of the dyspnœa.

Lewis and Barcroft [14] and their co-workers investigated the fixed acid content of the blood in cardiac and cardio-renal dyspnœa. They did not find any indication of increased fixed acids in the blood of purely cardiac cases, but in the cardio-renal cases, which were usually of more advanced age and in which the dyspnœa was often paroxysmal in type, they found an increase in the fixed acids to which they attributed the dyspnœa. They did not prove that there was an actual acidœmia in the blood supplied to the respiratory centre, nor that the condition present was the sole cause of the dyspnœa. Many investigators have shown that in frankly urœmic cases there is an acidœmia and lowered carbon dioxide pressure, and it is probable, therefore, that in elderly cardio-renal patients, in whom the renal functions may well be deficient, there is an increase in the fixed acids of the blood, such as was found by Lewis and Barcroft, although in their cases there was no rise in the urea content of the blood. The type of dyspnœa, however, that results from acidœmia, such as is seen in urœmia, is of the deep-air hunger type, while the dyspnœa in cardio-renal cases, even when paroxysmal, is often more rapid and shallow. It would appear, therefore, that even if cases of cardiac dyspnœa are accompanied by an acidœmia and either a raised carbon dioxide pressure, as in pulmonary diseases, or a lowered carbon dioxide pressure, as in renal diseases, these changes may be the result of concomitant pulmonary or renal conditions, and not necessarily the essential factors in causing the dyspnœa. In a group of cases similar to those investigated by Lewis and Barcroft, Campbell, Hunt and Poulton found acidœmia, but with a raised carbon dioxide pressure to which they attributed the acidœmia.

Fraser, Ross and Dreyer [15] have reported an alkalœmia in the arterial blood of simple cardiac cases with dyspnœa, which suggests an analogy with the condition seen in pure oxygen deficiency. Poulton, using a different

method, found a similar condition in one or two cases only, accompanied by a lowered carbon dioxide pressure, and Straub and Meier [16], using yet another method, found in cases of dyspnoea with renal disease that some had an alkalæmia with lowered carbon dioxide pressure in the alveolar air. These reports, even allowing for the possibly faulty methods, would indicate that in some cases of dyspnoea, and particularly in some cases of cardiac dyspnoea, neither raised carbon dioxide pressure nor increased H-ion concentration in the arterial blood are the cause of the dyspnoea. From analogy with experimental work on healthy subjects, it would appear that oxygen deficiency might be the cause. A lowered oxygen pressure in the arterial blood in many cases of heart failure has been shown to be present by several investigators [17], but in many cases no such deficiency is present. Some degree of diminished circulatory efficiency is present, however, and the venous blood is more unsaturated than usual, so that the tissues and the respiratory centre are exposed to oxygen lack through stasis. It would seem probable, therefore, that the oxygen lack in cardiac cases causes, in some, dyspnoea with rapid shallow breathing, which results in lowered carbon dioxide pressure in the alveolar air, lowered carbon dioxide pressure in the arterial blood and alkalæmia.

It is suggestive that this is the mechanism responsible for all cases of cardiac dyspnoea of the rapid shallow type, and that where the lung is much involved, a raised carbon dioxide pressure instead of a lowered is found, and that where there is accompanying renal insufficiency, acidæmia and a lowered carbon dioxide pressure may complicate the picture. To unravel the various factors involved in any one case will not prove easy, but improved methods and improved technique for obtaining adequate supplies of arterial blood should add very much to our knowledge in the near future.

*Treatment of Cardiac Dyspnoea.*—If this explanation of cardiac dyspnoea should prove correct, any treatment that is to affect the fundamental cause of the dyspnoea, that is to say, oxygen deficiency through stasis, must be directed towards improving the efficiency of the circulation. Administration of oxygen can only be useful when the arterial blood is deficient in oxygen. The rapid shallow breathing of cardiac dyspnoea is peculiarly ineffective to correct the disturbance, as it permits of stasis in the bases of the lungs and deprives the respiratory movements of much of their value in aiding the circulation. It is possible that this form of dyspnoea is even harmful. It has been suggested that carbon dioxide, added to the oxygen administered, might overcome some of the disadvantages of this type of breathing by deepening the respiration, and so raise the oxygen pressure throughout the lungs and raise the oxygen saturation of the arterial blood. Experience with a very limited number of cases has shown that, while some patients benefit from the administration of carbon dioxide and like it, others object to it strongly.

That morphia is of great value in relieving the distress in some cases of cardiac dyspnoea is undoubted, and it would appear to do more than simply to check a symptom, as the general condition of the patient and of the circulation seem to be improved clinically as the respirations become slower. Further, it is an accepted line of treatment in paroxysmal dyspnoea with progressing œdema of the lungs, and in this condition not only do the respiratory disturbance and distress diminish, but the œdema of the lungs may clear. If such paroxysms, with or without the development of œdema, are due to an exacerbation of the failure of the left ventricle, as seems most likely, then the essential cause of the dyspnoea will be the same as in simple cardiac dyspnoea, namely oxygen lack at the centre. This will result in lowering of the carbon

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dioxide pressure and alkalmæmia, and since the dyspnœa continues, the centre must be acting at a lowered threshold to H-ion concentration. This is the conception of Haldane [18] in the condition of oxygen lack in healthy people. Now morphia depresses the centre, so that it reacts at a raised threshold. In treating cases of dyspnœa with morphia, the effect would therefore be to raise the threshold so that the centre reacts only to a higher H-ion concentration than before. This would have the effect of restoring the carbon dioxide pressure throughout the body and in the medulla to a more normal level.

Dale and Evans [19] have shown that a lowering of the carbon dioxide pressure has the effect of depressing the vaso-motor centres in the bulb and spinal cord in cats, and that these recover their activity when the carbon dioxide pressure is restored. This might be the explanation of the beneficial results obtained from morphia in cases associated with lowered carbon dioxide pressure, but it is unlikely to be the explanation where a raised carbon dioxide pressure is already present. It is possible that some explanation will be forthcoming that involves the action of morphia on the vagus afferent impulses, so that the inhibitory action of reflexes, such as the Hering-Breuer reflex, is altered. It is apparent from the work of Pembrey and Allen [20] on Cheyne-Stokes breathing that the state of the respiratory centre is of importance in the control of dyspnœa. They, and others, showed that the periodicity is abolished either by the administration of oxygen or of carbon dioxide, and the accepted explanation is that, in periodic breathing, the centre is in a state of exhaustion from lack of oxygen, so that it requires a higher pressure of carbon dioxide than normally to affect it and to maintain its rhythmic activity. Whatever the explanation of the beneficial effect of morphia, it would appear that the condition of the centre and of the afferent impulses, and the effect of drugs on them, must be considered in any therapeutic measures for the relief of cardiac dyspnœa, other than those directly influencing the circulatory efficiency.

It may be conceded that something is now known about the chemistry of dyspnœa in healthy subjects, but that much more knowledge is required on the chemistry of large numbers of individual cases of cardiac dyspnœa. At present different schools of investigators use different methods, and there is considerable doubt as to the soundness of many of these methods, so that much preliminary work is still needed before reliable data can be obtained. Even when the facts are clearer than at present as to the chemical changes involved, the use of drugs to affect the dyspnœa directly with benefit to the patient cannot be clearly understood until the conditions of the centre and the afferent impulses, and the actions of such drugs on the centre and on the afferent impulses, are elucidated.

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## Section of Therapeutics and Pharmacology.

President—Professor A. J. CLARK, M.D.

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### Bronchial Asthma in Relation to Climate.

By Professor W. STORM VAN LEEUWEN.

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Holland.)*

DURING recent years it has become generally acknowledged that certain cases of asthma are due to hypersensitiveness to certain substances which either occur in the air and act after inhalation, or are present in food-stuffs and act after ingestion of the food. Some of these so-called allergic substances or allergens are known. Cases of asthma due to inhalation of ipecacuanha or emanations of horses, and cases due to ingestion of eggs or strawberries, have been frequently described.

During recent years our knowledge of these matters has been considerably extended by the work of Chandler Walker, Cooke, Coca, Freeman, Frugoni, Widal and others. A survey of the literature might give one the impression that, as a result of these investigations it would now be possible to determine the specific causative agent of attacks in nearly all cases of bronchial asthma. Since the treatment in these cases has to be a specific one, i.e., to consist of subcutaneous injections with small amounts of the specific allergen, this would imply that nearly all cases of asthma and allied diseases can be treated successfully in a specific way. We will investigate how far this really holds good.

The determination of the specific causative agent is nearly always made by applying so-called diagnostic skin tests, two methods of which are in use, viz., the scarification and the intradermal methods.

Under the scarification method small scarifications are made on the forearm of the patient and the material to be tested is applied either in dry form (with a drop of alkaline solution added), or in the form of an extract. If hypersensitiveness to this substance exists, a large wheal will form. This test always gives a positive reaction if a hay-fever patient is tested with the specific pollen. In other cases of hypersensitiveness to known allergens, such as ipecacuanha, strawberries, the action may also be positive, but in many asthmatics it will be found negative.

Therefore, according to our findings, a positive reaction with the scarification method is an indication of hypersensitiveness to the substance which has been applied, but a negative reaction does not prove the reverse.

*Intradermal* tests with extracts of allergens give positive reactions if hypersensitiveness exists, but the reaction is not specific enough, i.e., the finding of a positive reaction is not a proof that the substance tested has any relation at all to the allergic symptoms.

When some years ago my fellow workers and I applied specific skin tests to a considerable number of asthmatics, we were struck by the fact that so very few cases of true specific hypersensitiveness were found. In 100 patients observed, we found two or three really positive cases. On the other hand, application of a number of *intra-dermal* skin tests revealed a fact interesting to us, viz., all our asthmatics showed positive reactions to several allergens (sometimes as many as thirty or more), whereas normal individuals did not. Particularly, we found that more than 90 per cent. of our asthmatics showed a positive skin test with an extract of human dandruff (prepared in a way described elsewhere by us), whereas normals did not. Our conclusion, then, was that practically all our asthmatics showed signs of hypersensitiveness, whereas we were only able to determine the real causative agent of the attacks in a small number of cases.

This induced us to plan experiments to determine the unknown causative agent in the majority of our cases.

Early in the course of this work our attention was drawn to a fact which is known to all physicians, but the importance of which in relation to the ætiology of asthma has not yet been emphasized, viz., that in the majority of the asthmatics the occurrence of attacks is dependent on the locality in which they are residing. It is known that many asthmatics lose their attacks as soon as they come into the mountains, whereas attacks at once reappear on their return to low-lying countries. It is equally well known that most asthmatics are free from attacks as soon as they are taken into a clinic. On further investigation we obtained the impression that, even in our small country, great differences exist between various places with regard to the prevalence of asthma. This impression was confirmed by a research made on one of the islands (Zuid-Beveland) in the south-west part of Holland (Zeeland). We here found a large number of asthma cases among the rural population; in some villages as many as  $\frac{1}{2}$  to 1 per cent. of the whole population suffered from asthma. Since in other parts of Holland no such prevalence of asthma exists, it could be considered as proved that, even in a small, flat country such as Holland, great differences in the occurrence of asthma exist, which was in complete accordance with the impressions obtained by many physicians and by ourselves. This fact proves that the difference of "place," which has to be ascribed to differences in "climate," cannot be due to differences in barometric pressure, temperature, humidity of air, and similar known atmospheric influences, since these factors differ only slightly in the various parts of our country. Hence we concluded, that the causative agent of the attacks, in the majority of the asthmatics in Holland, must be substances present in the air in Holland and other low countries. The presence of these substances, which are presumably of colloidal nature, is due to peculiar conditions of climate. They occur in the air in large amounts in some places (which are "bad" for asthmatics) and are lacking, or almost lacking, in other places which are "good" for asthmatics. The nature of these substances is unknown; they may be indicated by a name frequently in use before Pasteur's time, viz., miasmata. As will be stated later, we have found two substances which may serve as types of allergic substances due to climate.

The miasma theory explains all the known facts. In many places miasmata occur in the air; consequently individuals who belong to the group which easily sensitize, will become hypersensitive to the miasmata. Also, those asthmatics who have been sensitized to other substances will presumably have been sensi-

tized to miasmata as well. This explains why specific and isolated sensitization to *one* allergen is so rare in Holland. It also explains why most of our asthmatics feel much better in one place than in another, and why in Switzerland, above a certain altitude, they have no attacks at all.

In connexion with our theory it is noteworthy to relate, that one of Frugoni's co-workers, Ancona, found an epidemic of asthma in a certain village, near Florence. He noted that the grain in this region was in a very bad condition that year; it had been fermenting and contained a number of parasites, amongst them one particular parasite, *Pediculoides ventricosus*. Ancona found that every man who for a long time had manipulated this grain acquired eczema, urticaria and asthma. The allergic substance present in this grain was due to the presence of the above-mentioned parasite. This fact is important, as it shows that in those cases where an allergic substance is very active, practically *everyone* who comes in contact with it becomes affected with an allergic disease. It is generally accepted that, as a rule, a certain predisposition is needed for an individual to become sensitized. Ancona's cases show that, in the presence of a very active allergen, no predisposition is necessary; our cases in Zeeland presumably form a transition between the ordinary cases of asthma and those of Ancona.

It need not be said that we have tried to find the causative agent of asthma due to climate, the substance being not only of importance for Holland, but also for all low-lying countries, especially those with a moist climate. We have not yet found the principal miasmatic substance, but, on the other hand, we have discovered two substances which may serve as types of the unknown substances of climate.

We observed a year ago, that grain, infected with *mites*, contains substances, due to the presence of the mites, which produce asthmatic attacks in a certain number of asthmatics. We were able to show that these substances also produce allergic symptoms in guinea-pigs and rabbits. If a certain number of guinea-pigs are placed during a certain time daily for three or four hours in a cage, at the bottom of which is put some grain infected with mites (and sterilized so as to exclude a direct action of the living mites), some of the animals will show allergic symptoms on the first contact with the grain and others will not. But if the experiment is continued, those animals which did not show symptoms the first day, will do so after the lapse of some days. The symptoms will gradually increase in intensity, and after some weeks decrease again; one portion of the animals, therefore, seems to have become immunized. But before this happens some of the animals will have died, either in a condition of shock or from some intercurrent illness. We noticed that very young guinea-pigs for the most part manifest symptoms on first contact; older animals usually do so only after prolonged contact. The allergic symptoms mentioned consist of scratching, sneezing, dyspnoea and general malaise. Young rabbits show a peculiar symptom; they stretch their front legs forwards and their hind legs backwards and yawn violently. The peculiar point of interest is, that *all* animals tested up till now (more than forty guinea-pigs and six rabbits) have shown symptoms of "allergic reaction" when treated in this way.

Besides this substance others were tried. Grain infected with common fungi (*Penicillium glaucum* and *Aspergillus fumigatus*) showed a similar, though less distinct and less regular action.

The experiments mentioned are of some importance, since they are made with a material which causes asthma in a number of asthmatics. The first material mentioned is a non-artificial one, since it may be obtained from a

peasant's granary or a grocery shop. The second material mentioned (grain infected with fungi) can be artificially made. Oatmeal was sterilized, infected with a culture of *Penicillium* or *Aspergillus fumigatus* (the culture being obtained from other grain) and after growth during some weeks was sterilized again, so as to exclude a direct action of the living fungi on the animals.

One of our experimental results, obtained with grain containing mites on an asthmatic peasant, must be mentioned. A Dutch peasant, who *always* shows symptoms of asthma in Holland, and who, in addition to that, gets acute attacks if he comes into contact with grain infected with mites, was taken to Switzerland. At St. Moritz he was completely free from symptoms, for the first time for many years. Inhalation of the dust of grain infected with mites (which for this purpose he had brought from his own granary) produced asthmatic symptoms after eight minutes; these symptoms lasted for twenty-four hours. Three days later the experiment was repeated with the same result in Davos, where by the courtesy of Professor Loewy the experiment could be carried out with such precautions that psychic influences were excluded.

This experiment is a further proof that the beneficial influence which high mountain air exerts on asthmatic symptoms is due to the *absence* of miasmata. As soon as dust which contains allergens is present in mountain air, asthmatics will have the same attacks there as they have in lower countries.

We will now consider how far, on the basis of the theory proposed, the therapy of asthma is possible.

In those few cases in which a specific causative agent of asthmatic attacks is found, specific treatment may be applied, injections being given with very small doses of extracts of the specific allergen and the dose being gradually increased. This point will not be discussed extensively, as we agree with the current opinion about it, with this difference, that we prefer to conduct this treatment with the smallest doses of allergen possible, whereas some other authors try to increase the dose as far as possible, attempting in that way to obtain immunization.

Since, as has been stated above, a specific causative agent for allergic attacks is only forthcoming in 2 or 3 per cent. of our cases, another method of treatment will have to be found for the great majority of patients. In these cases non-specific treatment will have to be applied.

This treatment is based upon the following two facts: (a) If in a case of hypersensitiveness to a certain substance, small quantities of this substance are injected, usually the sensitiveness decreases, but if small quantities of another allergen are injected, the sensitiveness to the first mentioned substance also usually decreases. (b) It is known that the condition of asthmatics and other allergics may undergo considerable alteration if they succumb to infectious diseases. Sometimes the onset of one disease (bronchitis, for example), may render the condition worse, but sometimes on the other hand certain diseases, such as tonsillitis, pneumonia, or even influenza may ameliorate the condition, or even produce a temporary cure. The same thing may happen if a sufferer from an allergic disease is injected with a foreign protein; in certain quantities it may improve the patient's condition, or in other quantities it may make it worse.

The aim of non-specific treatment, then, must be to find a substance which, if injected in a certain quantity into a certain allergic, will produce as much amelioration and as little increase in symptoms as possible.

The statement made above implies that in all cases of non-specific treatment

of allergic diseases great care is necessary in the choice of the right dose. As a rule small doses—if effective at all—will have salutary effects, whereas the larger doses may do harm. Since the sensitiveness of various asthmatics to the same allergens differs very considerably, it is impossible to assign a fixed limit for small or large doses. For the most part the policy to be adopted should be to begin with a dose so small that it will certainly do no harm, and gradually to increase it till the effective and beneficial dose is discovered.

For this reason anti-allergic treatment of asthma and allied conditions is much more difficult than treatment of "ordinary" diseases in which the active dose is known—within certain rather narrow limits. This fact explains why various authors, using certain substances for the treatment of allergic conditions report good results, but usually experience less favourable results if they try one of the substances used by another investigator. Generally the substance to which the investigator is accustomed will yield him the best results. For reasons stated elsewhere, we have for the past three years applied tuberculin injections as a routine treatment in nearly all cases of allergic diseases. We have found that all these patients are sensitive to tuberculin, and we made use of this sensitiveness to obtain a "reaction" with tuberculin, which is very often beneficial. Sometimes we employ injections with milk or sulphur as additional therapeutic measures. Peptone injections, introduced by Dr. Auld, yielded us less favourable results—perhaps for the reasons stated above—than they yielded in Auld's hands.

Of 300 cases treated by us during recent years, about 50 per cent. were beneficially influenced, so that the patients may be considered (and in fact consider themselves) as cured, or almost cured; 15 to 20 per cent. were not influenced at all. In the remainder of the cases there was distinct improvement.

Without dwelling further on this point, I would only draw attention to the fact, that of the 50 per cent. of cases in which there was either no improvement or only partial improvement, a great number belong to the group in which influences of place or of climate play a very predominant part. The question arises, what can be done for those cases?

I have repeatedly called attention to the fact that in many instances a sufferer from an allergic disease will be completely free from attacks as soon as he goes to a place with a different climate, or even if he enters a hospital. This not only holds good for cases of Dutch origin, but also for the majority of cases in England, North Germany, America, and the English and Dutch Colonies. Probably in many other countries the same phenomenon exists, but I have no definite experience of it.

It is almost certain that any patient who manifests this peculiarity, viz., freedom from attacks in a hospital, will also be quite normal and will not show any sign of allergic attacks as soon as he is taken to places higher than 1,500 to 1,800 m. above sea level. But it must at once be stated that a treatment which consists in sending a patient for some months to the mountains will as a rule only end in causing disappointment. Certainly the patient will be much better and will feel like a normal man up in the mountains, but after his return home he will, in a very short time, be in exactly the same state as he was before he went away. In many cases the improvement will not even last till the patient has reached his home; his attacks may recur on the way back. Nor is it certain that the patient's condition will be what it even was before he went away; we have often noticed that, during the first few weeks after return home, the patient's state of health has become even worse than it was before. All



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this is quite intelligible. As soon as the allergic patient arrives at a place where the air is free from those allergens which are noxious to him, he will be free from attacks, but he will be by no means cured, so as to have lost his hypersensibility. This hypersensibility, this allergic state, remains unchanged for many months and even for many years, although during all that time no attacks may have occurred. I have seen numerous instances in which hypersensitiveness in such cases appeared to be in no way reduced even after six or ten years. On the other hand, a complete absence of allergens may even set up a temporarily increased hypersensitiveness, as a certain degree of resistance present may become lost owing to the absence of any reaction.

A short residence in a mountain climate, then, will as a rule bring about no change in the patient's condition; it will only give him a temporary respite. If a patient is financially, and also in other ways, independent, there is nothing against his securing a respite from attacks, or even many respites, every year. If, however, he is without independent means, and if a temporary residence in a mountain climate can only be achieved with great difficulty and heavy financial sacrifice, I very firmly dissuade the patient from taking this step. In such cases everything should be done to treat the patient at home, and, as has been discussed above, even taking the patient temporarily into a hospital during this treatment should be avoided. Only in very severe cases, where death is threatening, must there be a departure from this rule; such cases may have to be admitted to hospital or sent to the mountains.

The question, however, assumes quite a different aspect if it is possible for the patient to change his residence *permanently*. For many people this is impossible, but on the other hand there are a certain number of allergies whose health might quite well be improved by a change of residence, if only they knew to what place they should go. A physician who has treated a number of cases of allergy will easily be able to give general indications about places which, as a rule, are "bad," and about others which, as a rule, are "good" for allergies. In every case, however, the patient himself will have to test whether a certain place is really suitable for him. Before taking measures for establishing himself permanently in a certain town, the allergic should temporarily, for some weeks at least, stay in that place, and as close as possible to the house in which he will probably have to live afterwards, in order to test whether he is indeed free from attacks there or not.

One important point relative to the choice of a permanent dwelling-place should be mentioned. The patient must choose a place which is just at the limit at which he is able to live in comfort. If, for example, a patient knows that he is quite free from attacks at a level of about 1,500 m., it would be very unwise to choose as a permanent or temporary residence a place at 1,800 m. for the following reason: Allergenic substances are never completely absent from the air; there are more of these substances at lower levels and less in the mountains, but they are never completely absent. Now we have seen that a complete lack of allergen in the air may lead to a loss of resistance in the patient. His sole chance of being more or less de-sensitized consists in coming into frequent contact with subliminal doses of the allergen. Hence it will be useful for him to choose a place not much above the level of his tolerance; this will enable him to inhale his allergen daily in small subliminal amounts and may tend to increase his resistance.

Has a permanent residence in the mountains to be really *permanent*? In many instances this is certainly the case. I have seen people who had lived in some asthma-free place in California for ten years and more, and who

succumbed to attacks almost the same day that they arrived in Holland. We have observed the same thing in children who had been educated at schools in the mountains of Switzerland.

On the other hand it must be admitted that, especially in the case of children, there is the possibility that, after a residence of some years in the mountains, the allergic condition may have passed away. However, when children or adults are sent from home to other climates in order to avoid allergic attacks, all calculations and plans should be made on the supposition that the change will have to be permanent. If, later on, it can be shown that the allergic state has really passed away, this should be considered in the light of a fortunate chance which was not to be anticipated.

Till now I have been speaking of mountain climate, but of course a climate which is salutary for an asthmatic need not always be a mountain climate. Sometimes a seaside or any other climate in which allergens happen to be scarce will do just as well. Even in Holland, where there are no mountains at all, change of residence within the limits of the country will often suffice. Only in very severe cases will mountain climate be indispensable.

It is superfluous to state that the term "allergens" used in this connexion is not a fixed entity. Substances which may be allergic for a sensitive patient born in Holland, need not be allergic for one born in America. Still, it is remarkable that asthmatics from very widely separated countries often agree fairly well as to the places, all over the world, which have to be considered as "bad" or "good." And if climatic factors play any part at all in the case of a certain asthmatic, it may be predicted with almost absolute certainty that mountain air at about 1,500 m. will be of great benefit to him.

#### MOUNTAIN CLIMATE COMBINED WITH ANTI-ALLERGIC TREATMENT.

The statement that change of climate of short duration does not materially improve the patient's condition, need not hold good if the patient is treated anti-allergically during his residence in a mountain climate. This is a point which needs to be emphasized. Physicians in mountain sanatoria should make themselves acquainted with the technique of anti-allergic treatment. If they could give proper treatment there would be a chance of really benefiting the patients.

In a certain respect these physicians are in a difficult position, since their patients do not show attacks in their sanatoria, and consequently it is very difficult for them to judge whether the anti-allergic treatment has gone far enough. In other respects, however, the physician in a mountain sanatorium is in a very favourable position. For those who apply the anti-allergic treatment at home there is always the possibility of injecting too much allergen, since they do not know how much of the, often unknown, allergen will at a certain moment occur in the air. In the mountains this quantity is negligible so that the physician there has much less risk to encounter.

Apart from these considerations there appear to be two other possibilities for the treatment of allergic diseases due to climatic influences. Since only preliminary investigations have been made in this respect, I am not yet in a position to communicate definite results; I only want to mention these possibilities in order to point out in what direction improvement, in the treatment of these severe forms of allergic disease due to climatic factors, may be found.

If it is true that many asthmatics only suffer from their attacks because a substance toxic to them is present in the air of the low-lying countries, it

might be presumed that they would be free from attacks if they stayed in an air-tight room, into which only air could pass which had been filtered, so that all allergic substances were kept out. Filtration of air may be accomplished by passing the air through cotton or through a layer of fluid—glycerol for example. Both methods, and combinations of it, have been tried by us. It is not yet advisable to publish these methods in detail. I will only state that it has been proved that in principle our surmise is correct, i.e., patients suffering from violent attacks of asthma in a certain house may be free from attacks, if they stay in the same house in an air-tight room, ventilated in such a way that only air, free from allergens or miasmata, can enter.

If it is possible to arrest miasmatic substances by filtering the air through cotton, it must be presumed that the cotton used as a filter in the ventilation system of the room above mentioned, should contain the miasmatic substance characteristic for the country in which the experiment is made. Since it is a general rule that injections of small quantities of an extract of a causative agent of allergic attacks ameliorates the condition, whereas larger amounts are noxious, it was to be expected that extracts of the cotton mentioned would act in the same way. This surmise also has proved to be correct. Details of these investigations will be published later.

### Clinical Experience with Powdered Leaves of Digitalis.

By E. COWLES ANDRUS and EDWARD P. CARTER.

To the physician dealing with heart disease it is extremely important that he have digitalis at hand in a form which is at once stable and, in so far as is possible, standardized. With regard to stability there is much to be said in favour of the dried powdered leaf. In this form the preparation retains its potency for a considerable period, whereas the tincture or infusion is known to deteriorate with age.

The pharmacological standardization of digitalis has been the subject of careful study during recent years and various methods of so-called "biological standardization" have been put forward. Into the details of these procedures we need not here enter, but it is sufficient to say that they involve the determination of the amount of some form of extract of the drug required to stop the action of the heart of the cat or frog. Certain features of these methods make them appear unsatisfactory from a clinical point of view. In the first place no process of extraction has been evolved which ensures the complete removal of all the component glucosides. In addition, the evidence is far from conclusive that the elements which arrest the action of the heart of the cat or the frog are those upon which the physician depends for the therapeutic effect in cases of heart failure.

It has, therefore, seemed advisable to attempt a clinical standardization of some form of digitalis. To this end a careful study has been carried out upon a large group of patients in the wards of the Johns Hopkins Hospital, Baltimore, during the past two years. The series was made up of cases of syphilitic, rheumatic and arterio-sclerotic heart disease all exhibiting signs of advanced cardiac insufficiency. It did not include cases of acute cardiac disease. The same details of treatment, apart from digitalis therapy, were carried out in each case, i.e., with regard to rest, limitation of fluid-intake, salt-free or salt-poor diet, and purgation. Morphia was used when indicated.

[April 8, 1924.]

Excepting those obviously *in extremis* all patients were treated without digitalis for four or five days in order to obtain, as nearly as possible, constant conditions. It was ascertained in each case that no digitalis had been taken during the three weeks preceding admission.

The dry powdered leaf of digitalis was chosen as the most stable preparation. A large quantity was obtained through a local firm and was kept thoroughly dry. It was put up as required in gelatine capsules by the hospital pharmacy. It was found that, given in this form, the drug was readily absorbed and was far less disagreeable to the taste than the tincture or infusion.

The scheme of dosage was based upon two principles similar to those advanced by Eggleston:—

(1) That the indication for digitalis therapy is the degree of cardiac failure regardless of the cause; and (2) that, given the indication for its use, the drug should be administered in quantity sufficient to produce either conspicuous therapeutic effect or some of its minor toxic symptoms.

Digitalis was given, therefore, to patients with auricular fibrillation or sinus rhythm alike, and in the following dosage: 0·8 grm. as an initial dose and 0·2 grm. every subsequent four hours. This dosage was maintained until there was evidence of therapeutic effect or until some one of the minor symptoms of digitalis intoxication appeared.

What, now, is to be taken as evidence of therapeutic effect? An answer to this question must involve a brief consideration of the physiology of heart failure. The normal circulation is governed by a system of reflex mechanisms which effect an adequate blood supply to the various portions of the body. In health, in the event of a sudden strain upon one portion of the circulation, compensatory mechanisms come into play to increase the blood flow in that region, decreasing it, perhaps, elsewhere in the system. Into this scheme there fits with perfect exactitude the device for the maintenance of proper gaseous exchange in the lungs, the circulatory stimulation of the respiratory centre.

In the patient with cardiac disease it is these mechanisms which are first called upon to prevent circulatory insufficiency. But as the circulatory failure progresses these outposts, so to speak, become inadequate—they do not necessarily collapse but they are no longer competent to meet the demands of the situation. Finally the bed-ridden, œdematous, orthopnoic individual with signs of congestion of the splanchnic circulation and an engorged liver, the advanced case of cardiac insufficiency, represents the capitulation of the last line of defence; the heart has failed.

The therapeutic effect of digitalis is dependent upon its support of this same "last line." First, through its action upon the vagus centre and later by a direct effect upon the myocardium it slows the ventricular rate—particularly in cases with auricular fibrillation—and increases the force and magnitude of ventricular systole, resulting in a more adequate circulation. Gradually there ensues a return of function in the other compensatory mechanisms with consequent relief of respiratory distress, subsidence of the splanchnic congestion, and, in œdematous cases, diuresis.

In this study therefore evidence of relief of respiratory symptoms, and increase in vital capacity, diuresis and reduction of liver enlargement were considered to indicate a therapeutic effect of the drug. It must be granted that this is not a sharp end-point but it is of sufficient accuracy to gauge the comparative potency of the preparation to be tested. It has, too, the conspicuous advantage that it coincides with the result which the physician desires to produce when he prescribes digitalis.

Electrocardiograms were taken in each case upon admission, preceding and during the administration of digitalis. It was thus possible to observe the course of the therapy by comparison of the P-R intervals and T-wave changes in successive records. The development of anorexia or nausea, of any conspicuous degree of A-V block, or the appearance of extrasystoles in a bigeminal sequence were taken to indicate minor intoxication, and the drug was withheld forthwith.

Pharmacologists, notably Eggleston,<sup>1</sup> have called attention to the relation of the weight of the individual to the amount of digitalis required to produce a given effect. Eggleston has developed a formula by which to calculate the therapeutic requirement of the galenical preparations of digitalis. Of the tincture this amounts to 15 c.c. per 100 lb. body-weight. While it has not been possible, in this series of cases, to establish a standard dosage for tinctures prepared from the same leaves, it has been found that in 95 per cent. of the cases there was conspicuous evidence of a therapeutic effect of the drug after 1.5 grm. of the powdered leaf per 100 lb. body had been administered. In cedematous cases the excess of body-weight due to fluid was roughly estimated and the estimation checked by weighing, after the subsidence of the cedema. Moreover, with the particular specimen used, symptoms of intoxication have not appeared with dosages below the above amount.

It has then been possible to establish with reasonable accuracy the amount of a given preparation of digitalis necessary to relieve the symptoms of cardiac insufficiency, in short to standardize it clinically. It may well be that in the near future pharmacological standardization of the drug may be perfected and its relation to the therapeutic requirement more clearly established, but until that time clinical standardization would appear necessary. And it may be urged with reason that for such standardization the most simple procedure is to test the efficacy of a stable preparation, preferably the dry powdered leaf, for the use to which the physician intends to put it, i.e., the production of a therapeutic effect in cases of heart failure.

<sup>1</sup> Cary Eggleston: "Digitalis Dosage," *Arch. Int. Med.*, 1916, xvi, p. 1.

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### DEMONSTRATIONS.<sup>1</sup>

Lieut.-Colonel J. A. ANDERSON: (1) Types of improvised sanitary appliances for tropical use; (2) Measurements of heat and sunlight in environment.

W. BROUGHTON-ALCOCK, M.B.: (1) A vibriothrix simulating *Vibrio cholerae* in the faeces; (2) Renal abscess demonstrating a mycelium occurring in a *Bacillus coli* infection of the urinary tract.

J. B. CHRISTOPHERSON, C.B.E., M.D.: (1) Series of photographs illustrating the distribution, bionomics and clinical features of *Schistosoma japonicum* in Japan (from Dr. Akira Fujinami); (2) Cercariae in liver of intermediate host of *Schistosoma japonicum* (from Dr. Nakamoto); (3) Section demonstrating male and female worms in vein, and deposition of ova, from a case of Egyptian schistosomiasis (from Major W. R. O'Farrell).

Professor L. S. DUDGEON, C.M.G., C.B.E., F.R.C.P. (President): Slides and drawings illustrating certain points in the pathology of appendicitis.

### A Case of Infestation of Human Liver with *Hepaticola hepatica* (Bancroft, 1893) Hall, 1916; with Sections from the Liver.

By Lieut.-Col. W. P. MACARTHUR, R.A.M.C.

THE subject of this infection was a British soldier who died in India in September, 1923, after three years' service in that country. His symptoms suggested pyæmia, and on post-mortem examination an apparent suppurative condition of the liver with areas of spongy consistence was found. Microscopical examination of the affected liver tissue showed the presence of large masses of eggs. Major G. H. Dive, R.A.M.C., forwarded sections of the affected organ to the Royal Army Medical College, and also provided the information which I have just given.

The eggs are indistinguishable in size, shape and structure from those of *Hepaticola hepatica*. A comparison of the sections with the similar preparations

<sup>1</sup> Laboratory Meeting held at the Royal Army Medical College, Grosvenor Road, S.W., May 5, 1924.



## 84 Ramsbottom: *Life-histories of Yeasts; Orchid Mycorrhiza*

of a naturally infected rat's liver, shown under the neighbouring microscopes, leaves no doubt of the identity of the eggs from these two sources. *Hepaticola hepatica* is a parasite of rats. Ripe eggs when ingested hatch in the intestine and the liberated larvæ pierce the gut wall and are carried to the liver by the blood-stream. There the worms mature, pair, and lay their eggs. Heavy infections cause an intense reaction in the liver and many rats die as a result.

The present is the first known instance of the occurrence of this parasite in man.

### Some Points in the Life-histories of Yeasts.

By J. RAMSBOTTOM, O.B.E.

A SERIES (twenty) of photomicrographic transparencies taken by ultra-violet light illustrating the life-histories of several yeasts were demonstrated. The work of K. Kruis and J. Satava has shown that there is apparently an alternation of generations in this group. When endospores are isolated and grown in culture they may copulate immediately or begin to bud. In the former case normal yeasts arise; in the latter a dwarf form is produced which differs in macroscopic appearance from the normal colony and even in its fermentations. Such forms have been cultivated for as long as three years without altering their characters. It may well be that we have here a type of "plus" and "minus" strains which occur also in Zygomycetes, Basidiomycetes and Ustilagineæ, and that "species" of *Torula* are dwarf forms which require to copulate with a strain of the opposite "sex" for the production of endospores.

### Orchid Mycorrhiza.

By J. RAMSBOTTOM, O.B.E.

A SERIES of preparations were exhibited showing the method in which orchid seeds germinate. All orchids have a fungus in the cortical cells of their roots. The well-known difficulty in germinating orchid seeds under ordinary conditions has been found to be due to the fact that the seed will not germinate unless infected by the root fungus. The slides showed a naturally infected root, the fungus isolated, sections of uninfected and infected seeds, stages of germination illustrating the manner in which the mycelium balls up and is later killed ("phagocytosis") and how the young root is at first free from fungus and later becomes infected from the soil. A culture of the fungus and seeds germinated by sowing them on the culture were exhibited.

J. GORDON THOMSON, M.B.: (1) A series of photographs illustrating houses in Rhodesia in which blackwater fever has occurred. (2) Films demonstrating *Plasmodium falciparum* in blackwater fever cases.

Lieut-Col. H. MARRIAN PERRY, O.B.E., R.A.M.C.: (1) Specimens illustrating the development of scolices of *Tænia serrata*. (2) Sections demonstrating the pathology of lesions in the lung, caused by *Paragonimus* infection.

The following is a report from Dr. Cullen on a case of *Schistosoma japonicum* in the Northern Shan States of Upper Burma, of which a liver section showing well-marked schistosomiasis (ova) was exhibited for Dr. Cullen by Dr. Christopherson.

### Case of Asiatic Schistosomiasis.

By J. P. CULLEN, M.D.

(Chief Medical Officer Burma Corporation, Namtu).

*Schistosoma japonicum* has, according to Professor Faust, a well-marked distribution in China, being present throughout the Yangtze valley from Ichang, 350 miles above Hankow to the sea.<sup>1</sup>

Professor Faust in the same article mentions that it seems probable that isolated spots of endemicity may still be reported, or even areas of mild infection. Dr. Christopherson informs me that he does not think *Schistosoma japonicum* has been reported west of Singapore. Under these circumstances the following notes may possibly be of some interest:—

The patient, a Chinaman, aged 40, was admitted into Namtu Hospital on February 2, 1924, in a very debilitated condition, complaining of abdominal pain and constipation of four days' duration.

On examination, the abdomen was moderately distended and had a doughy feeling suggestive of chronic peritonitis. The spleen was enlarged and the area of liver dullness showed no alteration from the normal. The heart was of normal size and the cardiac sounds feeble, with no adventitious murmurs. Over both lungs numerous scattered moist râles were audible both anteriorly and posteriorly, the sputum being scanty and muco-purulent. No tubercle bacilli were found.

The temperature on admission was subnormal, and no malarial parasites were present in the blood smears.

On February 4 he vomited about half a pint of coffee-ground material. His temperature rose slightly to 100° F., the pulse became quick and weak, and during the same night he died.

The previous history, so far as could be ascertained, was as follows: He had lived in Namtu for three years, preceding that he had resided for an indefinite period in the frontier village of Myitkyina in the Northern Shan States, and before that in the Chinese Province of Yunnan, which borders on the Northern Shan States.

Post-mortem Examination revealed a very extensive chronic peritonitis most marked in the upper part of the abdominal cavity, the organs in this region being matted together in a dense mass of fibrous tissue obliterating and distorting their normal relationships. The liver was of normal or slightly lessened size, with a greatly thickened capsule resembling the "Zuckergussleber" or sugar iced liver of some authors. On section it was found to be cirrhotic. The spleen was enlarged and its capsule thickened, and the œsophagus showed varicosities of the veins at its lower end. Pleuritic adhesions were present on both sides, and no naked-eye abnormalities were discovered in the heart.

Microscopically.—Sections of the liver, hardened in formalin and stained

<sup>1</sup> Professor Ernest Carroll Faust, "Schistosomiasis in China, Biological and Practical Aspects." *Proceedings*, 1924, xvii (Sect. Trop. Dis. and Parasit.), p. 37.

with hæmatoxylin and eosin, showed numerous ova scattered throughout the section, perhaps most numerous in the region of the thickened capsule. Some of the ova were embedded in strands of fibrous tissue radiating from the capsule into the substance of the organ, others were surrounded by a well marked round-celled infiltration, whilst in a third variety there appeared to be no reaction on the part of the host. In some places the ova were represented by a striated and distorted capsule; such were no doubt dead. Leiper's lateral spine could be made out in some, but by no means all. Pigment-carrying cells appeared throughout the section, in some places being aggregated into dense masses.

Major Jolly, C.I.E., Acting Director of Public Health, to whom I am indebted for kindness in writing to the Director of the Zoological Survey of India, informs me that, as a result of his inquiry:

"No special work has been done on the snails of Burma with special reference to the carriers of *Schistosoma*. *Blanfordia nosophora* is not found in Burma, but *Indoplanorbis exustus*, which is suspected as being the likely host of a species of *Schistosoma*, is quite common all over Burma and India. No helminthological work, however, has so far been attempted on the species in Burma."

It is unlikely that the affection was contracted in Namtu, but it seems just possible Myitkyina may have been the seat of infection, and more probable still Yunnan. The flow of Yunnanese labour always sets in to the west into Burma, and it seems unlikely that the disease should have been contracted in the endemic foci mentioned by Professor Faust.

Major Jolly informs me that no case has so far been reported in Burma, and no doubt when attention is directed to it more cases will be discovered.

*Schistosoma* infection when it invades a locality does not, like other tropical diseases, herald its onset by dramatic developments, it quietly digs itself in and is correspondingly difficult to eradicate.

I desire in conclusion to express my thanks to Dr. Christopherson and Mr. Johnson for sending me Professor Faust's most interesting article, sections of *Schistosoma japonicum*, intestines, shells of *Blanfordia nosophora*, &c.

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**VOLUME THE SEVENTEENTH**

SESSION 1923-24

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SECTION OF TROPICAL DISEASES & PARASITOLOGY



LONDON  
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1924

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## Section of Tropical Diseases and Parasitology.

President—Professor L. S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

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### Certain Problems which concern Intestinal Infection and the Intestinal Flora.

#### PRESIDENT'S ADDRESS.

By L. S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

THIS subject, which I propose to bring before the Members of this Section this evening, is of equal importance in tropical and temperate regions, although owing to hygienic, climatic, and dietetic conditions, intestinal disorders assume greater importance in tropical and sub-tropical countries.

The abnormal presence of various bacteria in the intestinal canal and fæces, their mode of entry, the influence of various diets on the bacteriological findings, the relation of these bacteria to infection of the intestinal tract or to a general infection of the host, are among the most interesting, yet difficult, problems for investigation. The term "intestinal infection," unfortunately, is often employed without any clear understanding as to its significance. The presence of abnormal bacteriological findings in the fæces is not evidence of intestinal infection, because in the healthy subject the most efficient means for the discharge of bacteria is by the fæces.

Harvey Cushing and Livingood<sup>1</sup> many years ago classified the intestinal flora into (1) permanent or obligatory intestinal bacteria; (2) transient or facultative forms. They considered that the members of the first group are invariably present, while in the case of the second group the bacteria appear only after they are introduced with food, or by similar means. These transient bacteria do not find in the presence of the permanent flora cultural conditions which favour their perpetuation, unless they encounter or produce a pathological lesion of the mucosa. The obligatory forms, in their opinion, are also dependent to some degree upon dietetic influences.

It is of interest in connexion with these views of Cushing and Livingood, to refer to the experiments of Moody and Irons<sup>2</sup> who introduced by the stomach tube suspension of *Bacillus pyocyaneus*, *Bacillus prodigiosus*, and *Strepto-*

<sup>1</sup> Cushing, Harvey, and Livingood, Louis, *Johns Hopkins Hospital Report*, ix, "Experimental and Surgical Notes upon the Bacteriology of the upper portion of the Alimentary Canal, with Observations on the Establishment there of an Amicrobic State as a Preliminary to Operative Procedures on the Stomach and Small Intestine."

<sup>2</sup> Moody, W. B., and Irons, E. E., "Invasion of Body by Bacteria from Intestinal Tract," *Journ. Infect. Dis.*, xxxii, March 3, 1923.



## 2 Dudgeon: *Intestinal Infection and the Intestinal Flora*

*coccus hæmolyticus*, but who were unable to cultivate these bacteria from the blood, chyle, or viscera.

Lembke,<sup>1</sup> in 1896, found from an examination of eighty-nine cases that only *Bacillus coli* remained constant and independent of diet.

This observation, however, was made at a time when it was not possible to differentiate colon bacilli and thus determine the presence of any abnormal strain, introduced from without, as distinct from the normal inhabitants of the patient's intestine.

Dudgeon, Wordley and Bawtree<sup>2</sup> have distinguished two groups of colon bacilli, hæmolytic and non-hæmolytic, and by serological methods they have been able to group to a large extent hæmolytic strains, but much less efficiently the non-hæmolytic. The non-hæmolytic strains are the common inhabitants of the normal intestinal tract of man, as they occur alone in about 85 to 90 per cent., as contrasted with the hæmolytic, but under certain abnormal conditions, such as diarrhœa and colitis, there is a striking rise in the percentage of cases with hæmolytic colon bacilli (35 per cent.).

From personal experience derived from material obtained from various sources other than human, the presence of hæmolytic strains has been found to be uncommon. A pure growth of hæmolytic colon bacilli has been obtained from the fæces at the onset of diarrhœa in some cases, in others a limited growth of hæmolytic colon bacilli; while an abundant growth of a non-hæmolytic strain has been obtained from the fæces during an attack of diarrhœa, which was not found previous to the illness. The identification of non-hæmolytic strains in the fæces is often, however, a laborious and difficult problem.

In the case of a man, under the care of Mr. Nitch, suffering from intestinal symptoms, dating from his long residence in India, and due to ulcerated granulomata of the small intestine, pure cultures were obtained from scrapings from the ulcerated surfaces of the granulomata of a non-hæmolytic colon bacillus which reacted with one of my anti-colon sera, and the examination of the patient's fæces resulted in similar findings on each occasion.

During recent years, the bacteriological methods employed for the study of the intestinal flora have been considerably modified and elaborated, more especially by American workers. Results based on such methods of investigation, however, are open to countless fallacies unless the various factors detailed below are fully appreciated.

(1) The patient's diet, and the length of time such diet has been in force.

(2) The condition of the mouth and throat as to the presence of pyorrhœa, or other inflammatory lesions.

(3) The presence of a pathological process in the intestinal or respiratory tract.

(4) Whether there is diarrhœa, constipation, or normal action.

(5) The influence of purgatives.

(6) The time interval between the collection of the specimen from the patient and the bacteriological examination.

(7) Whether contamination of the specimen of fæces with urine, or from

<sup>1</sup> Lembke, W., "Beitrag zur Bacterienflora des Darms," *Archiv f. Hygiene*, 1896, xxvi, p. 293.

<sup>2</sup> Dudgeon, L. S., Wordley, E., Bawtree, F., "On *Bacillus coli* Infection of the Urinary Tract, especially in relation to Hæmolytic Organisms," *Journ. Hygiene*, xx, No. 2, October, 1921, and xxi, No. 2, December, 1922.

unclean vessels, or from exposure to atmospheric conditions—especially in the Tropics—has occurred. It is largely due to the lack of knowledge of the bacterial flora of the intestinal tract in man, in normal and under abnormal conditions, and of the numerous circumstances which influence the flora, more especially dietetics, that common errors are so often repeated and inaccurate deductions made. To quote but one example, it is not an uncommon occurrence for a patient whose diet consists of cow's milk, to be treated with a streptococcal vaccine made from the fæces.

Morris, Porter and Meyer<sup>1</sup> have introduced a very elaborate technique for the bacteriological analysis of the fæcal flora of children, which is the following :—

- (1) Stock dilution made from a known weight of fæces.
- (2) Microscopical examination and differential counts.
- (3) Plating procedures.
- (4) Inoculation of fermentation tubes of dextrose, lactose, saccharose, and milk.
- (5) Inoculation of gelatine and Loeffler's serum.
- (6) Inoculation of acetic acid glucose broth—N/5, N/10, N/20.

By means of these methods they recognize three types of intestinal flora, namely: (1) Fermentative or saccharolytic; (2) putrefactive or proteolytic; (3) facultative or normal. Each of these types is characterized by a certain definite group of bacteria.

Gelatine and Loeffler's serum are especially favourable for organisms in the putrefactive class, while the amount of liquefaction and digestion of the respective media is an indication of the numbers of live putrefactive bacteria.

Samples of fæces with pronounced putrefactive properties cause complete liquefaction of gelatine in seventy-two hours.

Fermentative stools, as a rule, are buff-coloured, foamy, semi-formed. possess a sour odour, and are strongly acid. Putrefactive stools may be dry, solid, and dark brown in colour, or pale and moist, with mucus and undigested food, possessing a putrid odour, or dirty green in colour and semi-formed. These authors lay stress on the fact that the fæcal flora of a child fed on cow's milk, although not suffering from any intestinal disturbance, or symptoms of intoxication, may resemble the putrefactive stool, due to direct milk contamination.

It is of interest here to record the observations made by Frost and Bachmann,<sup>2</sup> who isolated hæmolytic streptococci from high grade milks. The milk was obtained from 412 cows from four dairies. In 28 per cent. hæmolytic colonies were obtained. Morris, Porter and Meyer consider that *Bacillus perfringens* plays only a very subordinate rôle in summer diarrhœa, but may be responsible for some of the grave symptoms of intoxication in infantile dysentery.

The importance of the constant presence of strongly proteolytic Gram-positive spore-bearing aërobes in putrefactive stools was recognized, and these authors consider that these bacteria are introduced with certain foods which are grossly contaminated.

As a result of these elaborate investigations, they consider it to be an established fact that the microbic flora of the intestinal canal is in direct relationship with the diet of the host, and that the absence of carbohydrates in a diet produces a predominance of proteolytic bacteria, resulting in putrefactive stools and symptoms of intoxication from intestinal absorption.

<sup>1</sup> Morris, G. B., Porter, R. L., and Meyer, K. F., "The Bacteriological Analysis of the Fæcal Flora of Children," *Journ. Infect. Dis.*, 1919, xxv, p. 349-377.

<sup>2</sup> Frost, W. D., and Bachmann, Freda, "Hæmolytic Streptococci in High-grade Milks," *Scientific Proc. Amer. Bacteriologists*, Dec. 28, 29, 30, 1922 (Detroit, Michigan).

#### 4 Dudgeon: *Intestinal Infection and the Intestinal Flora*

They believe that in children with strongly putrefactive stools, a special syndrome occurs, as in the case of adults: tired look, dark circles round the eyes, anorexia, cyanosis, laxity and stupor. A strict carbohydrate diet will effect a cure, and the stools will become fermentative. It is this change in the faeces which must be aimed at and persisted in, although it may take from ten to forty days; and of all foods, the most valuable is lactose.

Torrey<sup>1</sup> classifies intestinal bacteria into predominantly fermentative or putrefactive, and places *Bacillus coli* intermediate between these groups, as it may give rise to either type according to diet.

In adults the importance of chronic intestinal putrefaction has led to various methods of treatment being employed, such as: vaccine therapy, intestinal antiseptics, removal of the colon, and the at one time popular remedy of implanting antagonistic organisms, such as *Bacillus bulgaricus*. Cannon,<sup>2</sup> in the *Journal of Infectious Diseases* for 1921, refers especially to the question of the value of *Bacillus bulgaricus* as follows: "Within the last few years observations have shown that it is impossible to implant *Bacillus bulgaricus* in the intestinal tract, since the organism is apparently not an inhabitant of the normal intestine and cannot adapt itself to its new environment."

Torrey<sup>3</sup> has claimed good results in the treatment of typhoid fever by a high carbohydrate diet, which is best obtained by the administration of lactose. In dogs he has shown that lactose and dextrose added to the diet completely suppress the proteolytic types of faecal bacteria, while of the proteins responsible for intestinal putrefaction milk casein was far less formidable than meat protein. It is of interest here to record that Winternitz,<sup>4</sup> in 1892, attributed the value of a milk diet to the contained lactose.

Cannon's observations agree with those of other observers who claim that the reduction of intestinal putrefaction can be effected by the substitution of carbohydrates in the diet for proteins. The controlling force of the intestinal bacteria in the normal intestine is the food which is consumed. Cannon found that *Bacillus bifidus*, present in the faeces of children, lives on the lactose in human milk. From his experience mammalian proteins, especially, increase the number of indol-producing bacteria, while vegetable, fish and milk proteins do so much less; vegetables may have anti-properties, and at the same time he believes that vegetable proteins encourage the growth of a non-gas-producing acid flora. Absorption in the intestines is increased by raising the intra-intestinal pressure, and, therefore, distention of the intestines leads to increased absorption of bacterial by-products.

Pane's<sup>5</sup> observations agree with those of the American workers already referred to. He found that when a full meat diet is taken, certain proteolytic anaërobic and aërobic bacteria constitute the chief intestinal flora, and that their products cause the toxæmia; but if a milk diet is now given, the bacteria of acid fermentation proliferate at the expense of the others, because an unfavourable environment is established. It must be fully realized that acid fermentation is only a temporary measure to destroy proteolytic activity.

The adoption of the detailed method of study of the intestinal flora, for

<sup>1</sup> Torrey, *Journ. Infect. Dis.*, 1915, xvi, p. 72-108.

<sup>2</sup> Cannon, P. R., "The Effects of Diet on the Intestinal Flora," *Journ. Infect. Dis.*, 1921, xxix, pp. 369-385.

<sup>3</sup> Torrey, *Journ. Med. Research*, 1919, xxxix, p. 415.

<sup>4</sup> Winternitz, *Ztschr. f. Physiol. Chem.*, 1892, x, p. 306.

<sup>5</sup> Pane, N., ["The Normal Intestinal Flora in Adults,"] *Riforma Medica*, Naples, August 16, 1919, xxxv, No. 33.

which American workers are largely responsible, has led us to appreciate the following facts :—

- (1) The importance of diet, and the length of time a certain diet must be employed to effect a change in the intestinal flora.
- (2) The effect of active intestinal movements on the fæcal flora.
- (3) The advantage of employing various media in each individual investigation.

Dalyell,<sup>1</sup> when studying the obligatory anaërobes in the alimentary canal of infants, also found that marked differences occurred when various media were employed. I will refer to one example from my series to show the value of employing several media when investigating the intestinal flora.

The fæces of a small boy, with an intestinal infection, consisted largely of mucus and pus, and the pus cells contained Gram-negative bacilli. A large amount of the liquid fæces was dried by my method and the powder obtained was well mixed. This was added to various media, as shown, together with the results, in the accompanying table. The observations were in duplicate.

| No. | Medium                     | Result.                                                                                                |
|-----|----------------------------|--------------------------------------------------------------------------------------------------------|
| (1) | Litmus-lactose agar ...    | Abundance of streptococci. Few <i>Bacillus coli</i> colonies.                                          |
| (2) | Agar ...                   | Pure growth of <i>Bacillus coli</i> .                                                                  |
| (3) | Blood agar ...             | Abundant growth of hæmolytic <i>Bacillus coli</i> . Scarce growth of streptococci.                     |
| (4) | Loeffler's blood-serum ... | <i>Bacillus coli</i> . Streptococci. No liquefaction.                                                  |
| (5) | Milk ...                   | Ditto. No foaming.                                                                                     |
| (6) | Robertson's medium ...     | Streptococci and Gram-negative bacilli numerous. Gram-positive bacilli abundant. No rupture of medium. |

For some considerable time I have followed along the lines advocated by the American workers, and I entirely agree with their improved methods, provided the diet of the patient and the clinical condition are fully controlled. There are cultural differences between my technique and that used by the American workers, and I employ at the outset my own method for the preparation of the fæces for the bacteriological examination, which has been described in detail by Wordley.<sup>2</sup> This consists essentially in drying the fæces on sterile porous tiles and working with a mixture of the dry powder which is obtained. By this means you are able to study the bacterial content without any of the difficulties experienced with the ordinary methods, and you are able to work with a very large quantity of fæces, and this in itself is of considerable importance.

As we have now considered the influence of diet on the bacterial flora of the intestinal tract, more especially as regards the influence of a milk diet, it will be most satisfactory to discuss the bacteriological findings in sprue.

The bacteriological observations made on the fæces in cases of sprue are not convincing. One body of investigators believes that this disease is due to yeast-like organisms, and others believe that it is due to streptococci and colon bacilli. If we consider these observations for one moment we learn that the *Monilia*

<sup>1</sup> Dalyell, E. J., "Obligate Anaërobes in the Alimentary Tract of Infants," *Journ. Path. and Bact.*, 1914-15, xix, p. 276.

<sup>2</sup> Wordley, E., "A New Method for the Isolation of Organisms for Fæces and Sputum, with some observations on Hæmolytic Streptococci in Fæces obtained by this Method," *Journ. Hyg.*, July 15, 1921, xx, No. 1.

## 6 Dudgeon: *Intestinal Infection*; Le Bas: *Bilharziasis*

of Ashford<sup>1</sup> has been found in the fæces of sprue cases, that it has also been isolated from the tongue and sputum, and has been found to be pathogenic to guinea-pigs; while Krauss states that he isolated this organism from Porto Rico bread. Dodd's<sup>2</sup> observations were based on the examination of one hundred samples of fæces from cases of sprue from which he considered that there was a great increase of blastomyces. When white mice were fed on cultures of these organisms they developed diarrhoea similar to that met with in sprue, showed marked emaciation, and died within a few weeks, but, in my opinion, such findings require most careful confirmation.

Then we have the observations of Sir Leonard Rogers<sup>3</sup> who has suggested that sprue is essentially a streptococcal infection originating in the mouth and spreading down the intestinal canal. He also claimed good results from streptococcal vaccine therapy. I have made full bacteriological investigations of the fæces from twenty-four cases of sprue since 1919; all these patients were suffering from relapses, and many were acutely ill.

I regret, however, that my findings give no support to the bacteriological origin of this disease advanced up to the present time. Streptococci, which were abundant in the fæces, were mostly non-hæmolytic, but neither the hæmolytic nor the non-hæmolytic showed distinctive characteristics.

The streptococcal content of the fæces was no more abundant than might occur in patients on a purely milk diet; and in certain pathological conditions quite apart from sprue. Torrey<sup>4</sup> has shown that milk and a high casein diet stimulate a vigorous growth of saprophytic streptococci and enterococci, a statement which I can fully endorse. It would be well for this fact to be more fully appreciated, owing to the indiscriminate use of streptococcal vaccines made from the fæces. Yeasts occurred in the mouth and fæces, but no positive evidence was obtained that these organisms were the cause of the condition, or even related to it. My findings were in no way abnormal for patients on a milk diet and with fermentative stools.

In conclusion, it must be fully realized that the most elaborate bacteriological investigation of the fæces is wasted labour, unless taken in complete conjunction with the clinical condition of the patient, the diet, and the length of time the diet has been in force. It is well known that secondary organisms may play an important part in cases of typhoid fever, bacillary and amebic dysentery, but it is not understood how far the activities of these secondary bacteria are related to the patient's diet.

### A Note on the Employment of *Fasciola hepatica* as an Antigen for the Serum Diagnosis of Bilharziasis.

By GERALDINE Z. L. LE BAS, B.Sc.

HOPPLI (1921) published results showing that he obtained specific positive complement fixation for infections of both *Schistosoma mansoni* and *Schistosoma bovis*; employing as the antigen alcoholic extracts of adult *Fasciola hepatica*. He suggested that this might be of value in countries where the intermediate

<sup>1</sup> Krauss, W., "Studies of *Monilia* in connection with Sprue," *Amer. Journ. Trop. Med.*, 1921, i, p. 119.

<sup>2</sup> Dodd, M., "Bacteriological and Experimental Researches on the Etiology of Sprue," *Med. Record*, 1917, xci, p. 191.

<sup>3</sup> Rogers, Sir Leonard, "Vaccine Treatment of Sprue," *Ind. Med. Journ.*, liii, April 18, 1918.

<sup>4</sup> Torrey, *Journ. Med. Research*, 1918-19, xxxix, p. 415.

hosts of the schistosomes were not found. Bettencourt (1922), repeating Hoppli's experiments with cases of *Schistosoma hæmatobium* in Tavira, South Portugal, was unable to confirm this fact. But he was in agreement with Hoppli that these alcoholic extracts fixed complement in the presence of human sera which give a positive Wassermann reaction.

In the present series of experiments, sera from three species of schistosomes infesting man have been investigated. The technique employed was similar to that for similar experiments on antigens given by Le Bas (1923) [8]. Table I (p. 8) shows that both 90 per cent. alcohol and absolute alcohol extracts give more fixation with a syphilitic serum than with the sera from two cases of *Schistosoma hæmatobium*, or with a treated case of *Schistosoma japonicum*. All three cases yielded negative Wassermann reactions. Table II (p. 9) illustrates an experiment carried out with serum from a man infected with *Schistosoma mansoni* who also had syphilis, for which he was receiving treatment. Under performance of the Wassermann reaction the serum was found to be moderately positive—yielding a completely positive result with cholesterinized Noguchi antigen and incomplete inhibition of hæmolysis with ordinary Noguchi. The syphilitic pooled sera were divided into those yielding fully positive results with the cholesterinized antigen and nearly negative results with ordinary Noguchi, and those yielding fully positive results with Noguchi in the absence of cholesterol.

It will be seen from the table that the serum *m* from the case of *Schistosoma mansoni* gave the degree of fixation which would be expected from the degree of fixation due to syphilis, and that there is no evidence that increased fixation due to the double infection was present.

The fixation of the schistosome sera investigated appears to be weak and of little diagnostic value, and quite valueless when syphilis is also present.

This property of fixing complement with sera which give positive Wassermann reactions—that is, human cases of syphilis, for the reaction of sera from rabbits showing pseudo-fixation does not appear to have been investigated—seems to be very widely present amongst alcoholic extracts of both helminthic and molluscan tissues. Violle and Le Saint-Rat (1920) working with tapeworm, Israel (1910) with desiccated hydatid fluid and with hydatid endocyst, Brauer (1911) with hydatid fluid, and Le Bas (1922) with snail's liver, all found that the alcoholic extracts yielded positive results with human sera which gave a positive Wassermann reaction. On the other hand, Meyer (1904), Thiele and Embleton (1913) with tapeworms, Usami (1919) with hookworm, Fairley with snails' livers, and Setsuza Rhynji with *Clonorchis sinensis*, all found that alcoholic extracts did not give Wassermann antigens. Table III (p. 9) shows that *Tænia saginata* is capable of yielding a Wassermann antigen of moderate efficiency, and, further, in the experiments carried out with *D. latus* material by Le Bas (1923) (1), the alcoholic extracts prepared yielded positive results with Wassermann positive sera.

#### CONCLUSIONS.

(1) Alcoholic extracts of *Fasciola hepatica* are capable of fixing complement in a greater degree with sera from persons infected with schistosomes than with normal sera.

(2) Alcoholic extracts of *Fasciola hepatica* show a positive complement fixation with sera giving a positive Wassermann reaction.

(3) *Schistosoma mansoni* infection superadded to syphilis gives no greater

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degree of fixation than is accounted for by the degree of fixation due to the capability of the serum for fixing complement in the presence of a Wassermann antigen.

(4) Alcoholic extracts of helminthic and molluscan tissues very often give Wassermann antigen and great care should be exercised on this account.

### ACKNOWLEDGMENTS.

The work has been done under a grant made to Professor R. T. Leiper, by the Medical Research Council.

Thanks are due to the following for sera from their cases: Dr. P. H. Manson-Bahr (*Schistosoma japonicum* and *Schistosoma hæmatobium*); Dr. W. Y. Turner (*Schistosoma hæmatobium*); and Dr. Louws, of the Dermatological Clinic of Amsterdam, who most kindly sent serum over by air-post on two occasions from a case of *Schistosoma mansoni*. I am much indebted to Dr. F. H. Teale for control normal and syphilitic sera and other material, which he has always most kindly and unstintingly supplied for all the complement fixation tests which I have carried out during the last year and a half.

TABLE I.—COMPLEMENT FIXATION WITH *Fasciola hepatica* EXTRACTS.

| Quantity of antigen | A     |                |                | C      |                |                |
|---------------------|-------|----------------|----------------|--------|----------------|----------------|
|                     | j     | h <sub>1</sub> | s <sub>1</sub> | j      | h <sub>1</sub> | s <sub>1</sub> |
| 200 c.mm. ...       | ----- | -----          | -----          | +----- | +++--          | +++--          |
| 100 c.mm. ...       | ----- | -----          | -----          | -----  | -----          | ++---          |
| 50 c.mm. ...        | ----- | -----          | -----          | -----  | -----          | +-----         |
|                     | n     | h <sub>2</sub> | s <sub>2</sub> | n      | h <sub>2</sub> | s <sub>2</sub> |
| 200 c.mm. ...       | ----- | +-----         | +++--          | -----  | -----          | +-----         |
| 100 c.mm. ...       | ----- | -----          | ++---          | -----  | -----          | -----          |
| 50 c.mm. ...        | ----- | -----          | -----          | -----  | -----          | -----          |

j = serum from treated case of *Schistosoma japonicum*.  
h<sub>1</sub> = " " Case I of *Schistosoma hæmatobium*.  
h<sub>2</sub> = " " Case II " "  
s<sub>1</sub> = pooled positive syphilitic serum I.  
s<sub>2</sub> = " " " " II.  
n = normal serum.

A = Absolute alcohol extract of *Fasciola hepatica* emulsified in physiological saline.  
C = 90 per cent. alcohol extract of *Fasciola hepatica* " " "

± - - - Almost complete hæmolysis.  
+ - - - Very much hæmolysis.  
+ + - - Much "  
+ + + - Slight "  
+ + + ± Trace of "  
+ + + + No "

TABLE II.—COMPLEMENT FIXATION WITH EXTRACTS OF *Fasciola hepatica*.

| Quantity of antigen | A   |     |     |                | B   |      |     |                | C   |      |     |                |
|---------------------|-----|-----|-----|----------------|-----|------|-----|----------------|-----|------|-----|----------------|
|                     | n   | m   | s   | s <sub>c</sub> | n   | m    | s   | s <sub>c</sub> | n   | m    | s   | s <sub>c</sub> |
| 200 c.mm.           | --- | +   | --- | ±+++           | --- | ±--- | --- | ±+++           | --- | ±--- | --- | ++---          |
| 100 c.mm.           | --- | --- | --- | ±+++           | --- | ---  | --- | ±+++           | --- | ±--- | --- | ++---          |
| 50 c.mm.            | --- | --- | --- | ±+++           | --- | ---  | --- | ±+++           | --- | ---  | --- | ++---          |

A = Absolute alcohol extract of *Fasciola hepatica* emulsified in physiological saline.

B = *F. hepatica* dried in acetone, extracted with absolute alcohol and extract emulsified in physiological saline.

C = 90 per cent. alcohol extract of *Fasciola hepatica* emulsified in physiological saline.

n = Normal saline.

m = Serum from case of *Schistosoma mansoni* infection coupled with syphilis.

s = Pooled serum giving a positive Wassermann with Noguchi's antigen.

s<sub>c</sub> = Pooled serum giving a positive Wassermann with Noguchi's antigen in the presence of cholesterol only.

± - - - = Almost complete hæmolysis.

+ - - - = Very much " "

+ + - - = Much " "

+ + + - = Slight " "

+ + + ± = Trace of " "

+ + + + = No " "

TABLE III.—SHOWING FIXATION COMPLEMENT WITH SYPHILITIC SERUM AND *T. saginata* EXTRACTS.

| Quantity of antigen in c.mm. | Antigen A |     |     | Antigen B |      |      | Antigen C |      |     |
|------------------------------|-----------|-----|-----|-----------|------|------|-----------|------|-----|
|                              | t         | n   | s   | t         | n    | s    | t         | n    | s   |
| 200 c.mm.                    | ---       | --- | --- | +++       | +++  | +++± | +++       | +++  | +++ |
| 100 c.mm.                    | ---       | --- | --- | ±---      | ±--- | ++++ | +++       | +++  | +++ |
| 50 c.mm.                     | ---       | --- | --- | ---       | ---  | ---  | ±---      | ±--- | +++ |

A = *Tænia saginata* segments extracted with absolute alcohol, extract emulsified in physiological saline.

B = *Tænia saginata* segments dried in acetone, extracted with absolute alcohol and extract emulsified in saline.

C = *Tænia saginata* extracted with 90 per cent. alcohol, extract emulsified in saline.

t = serum from case of infection with *Tænia saginata*.

n = pooled normal serum.

s = pooled positive syphilitic serum.

± - - - = Almost complete hæmolysis.

+ - - - = Very much " "

+ + - - = Much " "

+ + + - = Slight hæmolysis " "

+ + + ± = Trace of " "

+ + + + = No " "



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### DISCUSSION.

Dr. R. T. LEIPER, commenting on the paper, emphasized the importance of a complement fixation test for schistosomiasis which could be easily prepared and carried out in countries like England, where materials for the Fairley test were not available.

Dr. J. B. CHRISTOPHERSON said that the paper was important from the clinical side, for a test was needed to determine when sufficient antimony tartrate had been given as a cure for schistosomiasis. For this there was no guide at present except experience. The various broods of bilharzia worms varied very much in their powers of resisting the antimony given. It was at present necessary, in order to be fairly certain of a cure, to give as much as 30 gr. (total) antimony tartrate. In some cases probably all the worms were killed before this amount was reached.

Dr. BROUGHTON-ALCOCK pointed out the somewhat limited value of the relative eosinophil cell count in bilharzia infections before, during, and after treatment with sod. antimony tartrate. Though a relative increase was in the great majority of cases determined preceding and during the first six months after beginning treatment, there were occasional cases in which the relative number was normal at one count, and increased at a later count in the early months. A relative increase was an important aid to diagnosis, also counts should be repeated when normal on the first occasion. There was a tendency to a fall in the number of eosinophils after beginning treatment.

Lieutenant-Colonel MARRIAN PERRY said the relative eosinophil cell count should not be regarded lightly. He quoted the case of four officers who had been shooting, and who had pyrexia; they were thought to be suffering from malaria, but examination of the blood disclosed eosinophilia 30 per cent. On examination of the faeces the ova of *Schistosoma japonicum* were found in them.

## Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

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### DISCUSSION ON SPRUE AND CÆLIAC DISEASE.

Sir LEONARD ROGERS, M.D., F.R.S.

IF the number of theories on sprue is any indication of our ignorance of its true ætiology we have much to learn regarding it. The varying views regarding its causation may be due either to the combination of symptoms we call sprue being produced by several different factors, or to its most essential cause being still undiscovered. I propose, therefore, first briefly to discuss the principal theories regarding its ætiology, and then to consider how far they can be reconciled or combined to furnish a working hypothesis which will help us in dealing with this obstinate and incapacitating result of prolonged residence in humid tropical climates.

#### INFECTIVE THEORIES OF SPRUE.

Sprue has been attributed to two main classes of organisms. First, yeasts have been known for twenty years to be present in large numbers in the disease, and whilst most observers consider them to vary widely in their species and to be a secondary infection, Ashford, with an exceptionally wide experience of sprue in Porto Rico, has described a single variety which he calls *Monilia psilosis*, and states that it is almost constantly present. The patients' sera gave complement deviation tests with its cultures, and vaccines made from it proved of value in treatment, so he considered it the true cause of the disease, although in his last papers he concludes that physiological glandular deficiency due to climatic and dietetic conditions predisposes to this infection. Secondly, as the result of the successful treatment of a considerable number of sprue cases while still residing in the tropics, with autogenous oral streptococcal vaccines, I suggested that this class of organism plays an important part in the ætiology of the disease, and Nichols, in Ceylon, confirmed the value of this line of treatment and also identified the offending streptococci as belonging to the viridans group. Here again the incriminated organism is not constantly present, and relapses may occur after the vaccine treatment, although comparatively seldom in my experience in cases in which the patients have been given a prolonged course of several months' treatment on these lines. It must, however, be admitted that the streptococcus is also very possibly a secondary infection, as I found in my Calcutta cases that the intestinal derangement usually preceded, often by several months, the oral and tongue affection from which the cultures were made.

## DIGESTIVE DEFICIENCIES IN SPRUE.

We may next turn to the digestive disturbances which constitute such a characteristic and important feature of sprue, with regard to which investigations have also yielded variable results. The pallor of the stools is now known not to be due to deficiency of intestinal bile, but to an alteration of bilirubin into leuco-urobilin, and although the liver dullness may be reduced, the functions of the organ are probably less affected than used to be thought. The gastric juice shows minor and inconstant changes, but the pancreatic ferments have frequently been reported to be greatly deficient or completely absent from the stools by T. R. Brown and by Bovaird, although J. D. Thomson found them to be normal, and the administration of pancreatin is said to be sometimes of value in treatment. The principal result of this deficiency is seen in the excess of fats in the bowel, mainly in the form of fatty acids instead of as neutral fats, as found by both H. H. Scott and J. D. Thomson, together with the formation of acid, fermented stools, and loss of body weight; in this way the disease is brought into close relationship with cœliac disease in children. These serious digestive changes may well predispose to secondary infections with yeast fungi and streptococci, which in their turn aggravate the fermentative and toxic symptoms and render the disease much more intractable.

## EARLY STAGE OF SPRUE.

In typical advanced sprue it is impossible to decide the relative importance of the digestive and the infective factors in the production of the disease, and we must turn to the beginnings of the disease for further light on its actual causation. Sir Patrick Manson enumerated no less than twenty predisposing causes of sprue, ranging from prolonged residence in an endemic area, which he regarded as the most important factor, to fistula or miscarriages. There is, however, one very common preceding condition in India which I met with in 20 per cent. of my cases, namely, hill diarrhœa, and this I think throws important light on the commencement of sprue, because the digestive disturbances in the hill disease are essentially similar in nature to those of the more serious and obstinate sprue into which it so often imperceptibly passes, namely, light-coloured stools in the early morning only, with much formation of gas, the evacuations being followed by relief. Nevertheless, hill diarrhœa is essentially a physiological deficiency of intestinal digestion, for, if it has not too long been neglected, a change to even a slightly lower elevation with little climatic alteration commonly at once checks the disease, while at elevations at which it occurs every gradation between the mildest flatulent dyspepsia and obstinate hill diarrhœa are met with; and further the administration of simple digestive ferments and other adjuvants after meals will relieve the less severe forms of hill diarrhœa, which is thus clearly a physiological digestive deficiency and not an infective process. As the other predisposing causes of sprue are all of a debilitating nature, the onset of the disease is probably due to such digestive weakness, which may later be greatly aggravated through secondary infection by such organisms as those which have already been mentioned. This combination may result in the intractable typical sprue with sore mouth and tongue, as well as obstinate "diarrhœa alba," as the condition was originally called, with far more reason than can be adduced for the present generally accepted title of sprue.

#### VITAMINS IN SPRUE.

It remains to be considered whether there are any other factors in the production of sprue. It is inevitable at the present day that in the discussion of any obscure disease the question of the rôle of vitamins should be raised, and not without some reason in this case. For Ashford has recently stated, on the basis of a study of 700 cases of sprue, that the mortality in half of them on a one-sided diet was 8·3 per cent., but in the later half on a liberal balanced diet the death-rate was but 0·6 per cent. I had independently come to the same conclusion from a study of a few cases in my ward in the London Hospital for Tropical Diseases, but found that it was only after my patients had improved on the streptococcal vaccine treatment with regard to their bowel condition that a rapid increase of weight, in several cases amounting to 1 lb. a day, followed a diet rich in vitamins, such as tomatoes and marmite. The latter contains an extract of yeast with much vitamin B, and as in recorded animal experiments a deficiency of this vitamin had produced atrophy of the intestinal mucous membrane somewhat similar to that which has been described in sprue, I was led to try this line of treatment. As Arthur Powell has recorded a number of sprue cases on a varied diet in a Bombay club, in which vitamins could have been little if at all deficient, I would suggest that in sprue there may be diminished absorption of these substances owing to intestinal atrophy and greatly reduced digestive ferments.

#### CALCIUM DEFICIENCY THEORY.

Lastly, during the present year H. H. Scott has pointed out the resemblance between the symptoms Vines has shown to be due to calcium deficiency and those to sprue, and has recorded very striking and rapid recoveries in a few typical cases of sprue treated with calcium lactate and parathyroid administered on the basis of this theory. If this be confirmed by further experience it may well prove to be an additional very important, but hitherto unsuspected, factor in the ætiology of sprue. Scott also ascribes the value of vaccine treatments in sprue to the removal by this means of the main cause of toxæmia, in this way relieving the overworked detoxicating function of the thyroid, and he thus furnishes a rational explanation of the combined action of primary digestive deficiencies of tropical origin and secondary intestinal infections. I trust his theory will prove correct, and thus will both elucidate the ætiology of, and furnish a successful therapy for, this obstinate disease.

#### Dr. REGINALD MILLER.

I have been asked to speak about celiac disease. Among pædiatricians there has never been any agreement on the question as to whether sprue and celiac disease are the same clinical entities occurring at different ages and in different climates. Cheadle certainly grouped them both together under the term "acholia" but others have regarded them as separate diseases. Of late years this question has been rather dropped. In my opinion we do not yet know enough about either condition to settle this question. For my own part I would not go further than to say that there are such notable differences between the two conditions as to make it unlikely that they will prove to be the same. Rather than dwell upon this particular problem, I think I am likely to be of more use to you if I give a rough sketch of celiac disease, and if time permits, proceed to the questions of its treatment and pathogenesis.

## COURSE.

Taking first cœliac disease in its broadest outlines it shows certain very definite characteristics. (1) Its first symptoms are always seen in childhood, usually in infancy. (2) It has not yet been traced as originating in adult life. (3) Even without treatment the symptoms usually pass off during the later years of childhood, and the patient nearly always grows to a fair stature and keeps free of symptoms on an ordinary diet. (4) It is very seldom fatal. I have never myself seen a cœliac case die except from some intercurrent disease, and the number of undoubted post-mortems on subjects of the disorder is extremely small. (5) It does not occur in epidemics.

The essence of the disease is a mal-absorption of the fat in the diet. Cœliac disease is not a form of intestinal disease in which diarrhœa becomes so severe that fatty stools are passed. Such a condition we see in tuberculous enteritis, chronic dysentery, lamblia infections in children, but cœliac disease is not of this type. On the contrary, the primary factor in cœliac disease is a digestive fault leading to mal-absorption of fat, and such symptoms of enteritis as occur are secondary in form.

This important fundamental conception of the disease is proved thus: (1) Mild cases may show no diarrhœa throughout their course. (2) In severe cases, in their quiescent intervals and during convalescence, there is no diarrhœa but the stools will still show on analysis a large excess of fat. (3) Post-mortem evidence shows neither enteritis nor intestinal atrophy.

## SYMPTOMS.

These consist of an excess of fat in the stools, abdominal enlargement without signs of organic disease, lack of growth (the result of toxæmia), anorexia, anæmia, and some degree of fever. Rare symptoms are tetany, convulsions, œdema, megacolon, urinary calculus. The tongue is very seldom ulcerated. Only in one case have I known this to be a troublesome symptom. We do not regard tetany or stomatitis as in any way proper to cœliac disease, for any child with prolonged and severe indigestion may show both these features.

The stools are of two varieties. The classical type, simulating the stool of sprue, is large, pale, unformed, frequent, offensive. It contains a great excess of fat, which may form from 30 to 80 per cent. of the dried stool, and the bulk of this fat is present in the form of fatty acid. The non-diarrhœic stool is also large, but it is formed, coloured, and not offensive. It contains a smaller excess of fat, which is chiefly in the form of soaps.

## TREATMENT.

The treatment adopted consists chiefly in prescribing a diet containing the optimum amount of fat for the particular case. This will always be considerably lower than in a normal diet. By this one means alone we are able to keep the child free from symptoms and restored to apparent health. In addition I usually prescribe alkalies—soda, magnesia or calcium—in the hope of assisting in turning the irritating fatty acids into non-irritating soaps. Lastly I give courses of bile-salts which in many cases lead to an increased absorption of fat.

Our treatment cures the patient's symptoms but it does not apparently render fat-absorption normal. In the diet it is not what you give but what you do not give that is important.

# PATHOGENESIS.

I have already shown why coeliac disease is not considered to be an enteritis. Bacteriological research leaves us without help. Pancreatic disease we can exclude. Other suggested causes are boric acid poisoning, deficiency disease and lacteal obstruction. To sum up, it is generally held that coeliac disease is a digestive fault and not an organic disease. Of the glands concerned in fat-digestion the liver seems under chief suspicion. We know that the disorder is not due to a persistent inhibition of bile-flow, but, speaking for myself only, I think it is not unlikely to be due to some abnormality in bile-salt secretion.

Dr. CASTELLANI, C.M.G.

# ETIOLOGY.

I am afraid we have to come to the conclusion that as yet we do not know the real causation of sprue, but there are several interesting theories regarding it.

(1) *That it is a Disease of Streptococcic Origin* as suggested at one time by Sir Leonard Rogers. I must frankly state that I doubt whether the streptococcus is the real cause of the malady; it is probably a secondary invader which may play a certain part in the causation of some symptoms, for instance, inflammation of the tongue.

(2) *That it is a Disease of Monilial Origin.*—Kohlbrügge, in 1901, in scrapings from the tongue and intestinal mucosa found fungi which he identified with *Monilia albicans*, in those days better known as *Oidium albicans*. As he noticed that the fungus had invaded the deep strata of the mucosa he came to the conclusion that the organism was the true cause of the malady. His findings were speedily confirmed by a large number of French and Dutch observers and recently his investigations have been greatly extended by Manson-Bahr and Ashford. I am inclined to think, however, that the fungus, like the streptococcus, only produces certain symptoms of the malady and is not the real causative agent.

(3) *That it is an old manifestation of Dysentery, especially Amœbic Dysentery.*—This theory is very interesting, as according to it sprue might be regarded as a disease of amœbic origin although the symptoms of dysentery are completely absent. Sprue would therefore, in a certain way, correspond to the tertiary stage of syphilis.

(4) *That it is a Deficiency Disease.*—At present this theory is most popular; without doubt many symptoms of the malady are in favour of it, equally so the fact that after all the most successful treatment is the dietetic method.

# TREATMENT.

As just indicated the principal part in the treatment of sprue is dieting, and here we encounter two opposite opinions: (1) that strict milk diet should be given, and (2) advocacy of an exclusively meat diet in cases of sprue. I think that we should individualize; each case must be treated on its own merits.

As regards drugs, I have never seen any striking results from the administration of santonin. In certain cases the administration of large doses of bicarbonate of soda and of an ethereal extract of strawberries is useful. The latest method of treatment, introduced by Dr. Scott, namely, the administration

of calcium lactate and parathyroid gland, is still on trial. I fully believe that calcium lactate is useful in certain cases—as a matter of fact, three years ago, without knowledge of Dr. Scott's investigations, Dr. Wilkinson and myself gave large doses of calcium lactate in two cases of sprue with apparently good results, but the improvement was only temporary. As regards the administration of parathyroid extract, I have had two cases treated in this way; one patient has greatly improved, but the other unfortunately is getting worse.

Dr. H. H. SCOTT.

My remarks will be limited to the subject of sprue only as my experience of celiac disease is too small for me to discuss it usefully.

This evening various theories as to the causation and modes of treatment of sprue will probably be stated. My only reason for bringing forward another method of treatment is that it possesses a rational basis, whereas, other methods of treatment have hitherto been almost entirely empirical; and, again, I am in the fortunate position of being able to produce at least one genuine cured patient for your inspection instead of merely recording cases. The best way of indicating briefly the line of reasoning on which the theory first originated is to report the history of the patient, a consideration of whose symptoms and progress formed the basis of the theory.

The patient in question contracted the disease in China, in 1920. The case was a severe one, with the usual symptoms which I need not recapitulate, except to say that the loss of weight was rapid, sometimes 3 lb. in a week, and the mouth symptoms were very severe. On more than one occasion there were over thirty aphthæ in the mouth so that the ingestion even of milk was painful. These spots are spoken of as aphthæ, but they differ from what we usually recognize under this term. A bout of "acidity"—burning pain in the stomach with acid rising into the œsophagus—would be followed in twenty-four hours or so by the appearance of a crop of four or five very tender spots on the tongue, the frænum, or the level of the second molar, or on the gums and cheek. There was nothing visible at this early stage, but in a few hours small red patches would develop which in another twenty-four to thirty-six hours would break through or become abraded, and intensely painful little ulcers would appear.

Another symptom, not mentioned in the books but very marked in this case, and in many others where I have questioned the patients, was that of cramps, or rather carpo-pedal spasms. Tetany is spoken of, but the former is, in my opinion, much more common. During the night this patient frequently had to spring out of bed and pace the floor to relieve these cramps of the legs and feet; when writing, cramps of the hands would come on so that the penning of more than a brief letter became impossible.

This patient suffered many things of many physicians. He took nearly all the yellow santonin there was in the Colony, he was given autogenous vaccines—a *Streptococcus salivarius* and others—emetin injections, was dieted with milk at one time, by the Salisbury method at another, with a diabolical preparation to which sprue itself is, in my opinion, preferable, namely liver soup, at a third. Finally, he was sent on a voyage to Canada with the idea that a "spell of cold weather would drive out the sprue." After some months, during which he became progressively worse, he managed to crawl with aid on to a vessel for England and arrived in March, 1922. He was taken into hospital the following day, fortunately under the care of one who had for years been studying the disease both at home and in its endemic centres abroad. Confinement to bed and

a milk diet for some weeks led to considerable improvement and the patient went to the country to convalesce. Any attempt, however, at passing on to even decent invalid food led to more sore mouth, more flatulence, acidity, large stools, loss of weight, in fact to a return of the typical symptoms. Two relapses occurred during the ensuing nine months and the patient was invalided from the Service in consequence. At each of the relapses the cramps returned as badly as ever. One now began to wonder whether it was not possible to trace these various symptoms to some common origin. Dyspepsia and large pale, pasty motions are amongst the earliest manifestations. The stools show a relative excess of fatty acids and a relatively large excretion of calcium. The urine also in some cases contains more calcium than normal. The flatulence, acidity and diarrhoea, the aphthæ, languor and loss of weight, might all arise from states of intestinal intoxication. Next came to mind the peculiar association of carpo-pedal spasms with calcium deficiency, the possible correlation of these with the diarrhoea and intestinal toxins, their reference to the group of organs which have been credited with the control of both these conditions—namely, the parathyroids. A two-fold function is believed to attach to these organs, namely, (1) the regulation of calcium metabolism and (2) de-toxication, in particular of the poisons of intestinal origin. In addition, there is the condition of hyperchlorhydria, but this is subsidiary and does not operate in all cases. If it is present, the result may be an excessive production of secretin by the action of the acid of the gastric juice upon the prosecretin of the mucous membrane of the duodenum. Excess of secretin leads to stimulation of the pancreas with an increased splitting up of fats, so that the normal ratio of neutral fats to fatty acids is diminished, as I have detailed in a former paper. This, however, is but a side issue and does not come into play in all cases, for in sprue the gastric contents may show hyperchlorhydria, normal acidity, or hypochlorhydria.

Here was enough to begin upon. Examination of the blood revealed the fact that the total calcium was little, if at all, diminished, except in later stages or in very severe cases, nor was the coagulation time prolonged. The inference, therefore, was that in spite of the loss of calcium in the excreta, sufficient was being absorbed to maintain the normal total calcium in the blood.

Now, calcium is present in the blood, according to the researches of Vines and Groves, in two forms—the ionic or free and the combined or coagulative. Seeing that the latter was not deficient although the symptoms pointed to calcium deficiency, one inferred that the ratio between the two was possibly upset. In the blood plasma there is normally between 10 and 11 mgm. of calcium per 100 c.c., 6.5 mgm. of which, or thereabout, is in the ionic state and the remainder combined. This latter fraction is concerned with the process of coagulation and during the clotting this 4 mgm. of “combined” calcium becomes changed to free calcium, due to the breaking down of a calcium-lipoid complex. In normal persons the whole of the 10 mgm. should, after the blood has clotted, be in the free state in the serum, that is, there should be no residual combined calcium. In conditions of so-called “calcium deficiency” the blood, after clotting, is found still to contain some calcium in the combined form, and there is thus a relative deficiency of ionized calcium.

In continuance of the history of the patient whose case I was describing: administration of calcium in the form of lactate was then prescribed and the symptoms improved, but, in spite of continuing this drug, relapse took place. Further increase of the calcium led again to improvement followed again by relapse. Perhaps the augmented intake brought the ionic calcium temporarily



to the normal and the relapse would then be explicable from the fact of the ratio again becoming, or still remaining, disarranged. The amount of the calcium was then reduced and parathyroid extract was given. I am not sure now whether calcium need be given at all, as the food as a rule contains sufficient calcium for the needs of the body. As the result of this a wonderful change took place. Developed aphthæ rapidly healed, commencing lesions, such as sore papillæ and tender spots, receded without becoming aphthæ, the flatulence was diminished, the stools became fewer and smaller, the colour began to return, and the general sense of well-being was regained. Within so short time as a fortnight all the more acute symptoms had disappeared; food was increased, calcium was stopped, and in five weeks the patient was on ordinary diet. The parathyroid was gradually discontinued and in six weeks from its first employment was stopped altogether. Nearly eleven months have elapsed and he has lived a normal life, working hard Sundays and week days, paying no regard to diet, taking alcohol on occasion, and he has not had a day's illness since.

This is not altogether an exceptional case. I have records of others in which the patients have done nearly, some quite, as well; one even better.

Let us briefly review the symptoms associated with calcium deficiency, or, as it is preferable to call it, irregularity of calcium metabolism, and see how they apply to the disease we are discussing.

When the European goes to the East, for instance to India, he continues to indulge his preference for a meat diet and partakes also of some of the native dishes, such as curries and highly seasoned rice. His thirst is slaked by iced drinks, often sweetened, and sometimes containing a considerable quantity of alcohol. His diet there is thus in several ways unnatural, and hyperchlorhydria is common. The effect of this I have mentioned elsewhere (*Transactions of Royal Society of Tropical Medicine and Hygiene*, February, 1923; *Lancet*, October 20, 1923).

Gastric and intestinal disturbances are commonly associated with stomatitis and aphthæ. The excess of acids in the food also plays a notable part in sprue; the saliva itself is in many cases acid.

In other countries where sprue is endemic, China and Porto Rico for example, to instance places in the Far East and in the West, the food and the methods of cooking lead to an increased ingestion of fats. Kochmann and Petzsch showed that a considerable loss of calcium occurred by adding fats to the diet of healthy dogs, while Rothberg and others noticed that in children to whom an excess of fats was given there was a reduced retention of calcium; this they explained by the increased formation of calcium soaps in the intestine, unabsorbed fatty acids being eliminated in the stools as soaps of potassium, sodium and calcium, this elimination leading to the impoverishment of the body in respect of bases.

Again, Korenchevsky found that in animals on a diet deficient in calcium, diarrhœa was noticed, together with such symptoms as loss of weight and increased nervous excitability. Korenchevsky adds (and this may be of interest in connecting this with celiac disease):—

"The mechanism of the development of this diarrhœa is not clear and yet it is undoubtedly worthy of special investigation. In my opinion this may be of special importance in elucidating the ætiology and therapy of some forms of infantile diarrhœa. It is possible that there is a connexion between calcium starvation and some digestive disturbances in children."<sup>1</sup>

<sup>1</sup> V. Korenchevsky, "Aetiology and Pathology of Rickets" (*Medical Research Council, Special Report Series*, No. 71), 1922, p. 64.

How far vitamin action comes into play I do not venture to say, though, according to Aulde, a lack of calcium when vitamins are supplied induces the same changes in the endocrine glands as are seen in conditions where calcium in the diet is abundant but the vitamins are deficient. In other words, vitamins are unable to fulfil their function in the absence of inorganic salts.

In calcium starvation (accompanied by deficiency of fat-soluble factors in the diet) there is a considerable loss of calcium and phosphorus from the organism. Korenchevsky found in his experiments that administration of calcium salts under these conditions led to a temporary improvement in the balance of calcium, but as soon as the extra intake ceased the retained calcium was soon excreted. Before I came across the account of Korenchevsky's work I had stated that this was what probably occurred in sprue. The taking of calcium led to improvement of symptoms, but the dose had to be increased because, so I inferred, it was being improperly regulated.

Now, if we think for a moment of the nutrients recommended for sprue—certain fruits, milk, carrots, young spinach, potatoes—we see that all are characterized by an excess of bases and a richness in calcium. The amount of calcium in the blood does not depend solely, or even in the main, upon the amount of calcium salts taken in the food or as medicine, but on the efficiency of the calcium-regulating mechanism—the parathyroids with their twofold function of de-toxication and regulation of calcium metabolism. The mechanism of this regulation would naturally be upset if the toxins were more than could be dealt with, and the absorption of certain toxins appears to lead to such disturbance; the ionic calcium suffers diminution and becomes relatively deficient first, while in the later stages there is an absolute deficiency. Calcium deficiency is therefore regarded as an indication of toxin absorption.

In sprue cases, therefore, we have one of two conditions. In the one where fats are in excess, we have an excessive excretion of calcium in addition to intoxication of intestinal origin; in the other protein excess with intestinal toxin formation. In both, the parathyroid de-toxicating function is overburdened, with a resultant disorganization of its calcium-regulating function, while there may be in addition a diminished calcium absorption. Both functions, therefore, of these glands are interfered with.

This, then, is the theory. I will now place before you the subjoined table (p. 20) giving a few figures of the calcium content of the blood of a normal person, and of patients suffering from sprue. The specimens were sent to Dr. H. W. C. Vines, of the Cambridge Medical Schools (Fellow of Christ's College), who very kindly undertook to examine specimens of blood sent up with regard to their calcium content. He was not told the histories of the cases when the specimens were sent and I think you will agree that the results obtained are remarkable in corroboration of the theory which I have brought before you. I wish I could have prevailed upon Dr. Vines to be present this evening to explain his method but he could not spare the time. I take this opportunity, however, of expressing publicly my indebtedness to him for carrying out these analyses.

In my former paper records were given of individual cases and also an explanation, or possible explanation, of the rationale of the modes of treatment at present in vogue. There is no need to repeat these, but as Sir Leonard Rogers believes strongly in the efficacy of vaccines in the treatment of this disease and has reported many good results, I may, perhaps, be allowed to say a word or two more on that head, as it bears on my own theory. According to what I have advanced the de-toxicating function of the parathyroids becomes

## CALCIUM CONTENT OF THE BLOOD SERUM OF SPRUE PATIENTS.

|                                                                                                                                                                                                                     |        | Free Ca | Combined Ca | Total in mgm.<br>per 100 c.c. serum |
|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|--------|---------|-------------|-------------------------------------|
| Normal (after clotting)                                                                                                                                                                                             | ... .. | 10.7    | 0.0         | 10.7                                |
| McG.—Fairly severe case:                                                                                                                                                                                            |        |         |             |                                     |
| Before starting treatment                                                                                                                                                                                           | ... .. | 6.1     | 3.8         | 9.9                                 |
| After one week                                                                                                                                                                                                      | ... .. | 8.1     | 1.8         | 9.9                                 |
| After two weeks (good progress), (still<br>in hospital)                                                                                                                                                             | ... .. | 10.1    | 0.0         | 10.1                                |
| S.—Average case. Blood not taken on<br>arrival. Weight, 118 lb.                                                                                                                                                     | ... .. |         |             |                                     |
| After one week (weight, 121½ lb.)                                                                                                                                                                                   | ... .. | 7.0     | 2.9         | 9.9                                 |
| After two weeks „ 127 „                                                                                                                                                                                             | ... .. | 7.4     | 2.7         | 10.1                                |
| After three weeks „ 133½ „                                                                                                                                                                                          | ... .. | 10.8    | 0.0         | 10.8                                |
| After four weeks „ 142½ „                                                                                                                                                                                           | ... .. | 10.8    | 0.0         | 10.8                                |
| C. W.—Mild case. Nine months ill.                                                                                                                                                                                   |        |         |             |                                     |
| Bombay                                                                                                                                                                                                              | ... .. |         |             |                                     |
| Before starting treatment                                                                                                                                                                                           | ... .. | 8.0     | 1.9         | 9.9                                 |
| After two weeks                                                                                                                                                                                                     | ... .. | 8.9     | 1.2         | 10.1                                |
| After three weeks                                                                                                                                                                                                   | ... .. | 10.4    | 0.0         | 10.4                                |
| Was allowed full ordinary diet, meat,<br>beef and mutton, vegetables, &c.<br>After four weeks reported "quite<br>well, gained another 3 lb., now<br>2 lb. over my normal, smoking<br>pipes and cigarettes all day." |        |         |             |                                     |

overtaxed: it has been working overtime, as it were, for probably a considerable period and has finally had to succumb or at least take a temporary rest. The organism cultured from the mouth or stool is only one of several which may have had a hand in the toxin production and the effect of the vaccine would be to render one of the foes *hors de combat* and reduce the inimical influences to such an extent that the parathyroids can now cope with them, especially when the treatment is combined with mental and bodily rest in bed and an addition of calcium by a diet consisting largely of milk. This explains, I think, the good results obtained from so many different vaccines, provided they are autogenous.

" There are twenty different ways  
Of writing tribal lays,  
And every single one of them is right,"

says Rudyard Kipling. It may be the same in regard to sprue. Twenty different men might find twenty different organisms, make vaccines from them and report good results in each case, for they might each have hit off an organism responsible, in part at least, for the toxin produced in each individual patient, and by combating it relieve the overworked parathyroid and enable this gland again to take up its duties. But no single vaccine would prove effectual in all cases, and we may thus explain the individual good results of streptococci in some of Sir Leonard Rogers' patients, of monilia in Ashford's, of coliform bacilli in various others.

Another form of treatment, highly commended by some in the Far East, is the administration of powdered crabs' eyes and cuttle-fish shell. I do not know the chemical composition of the former but the latter contains much calcium. Another favourite medicine is pulvis Bataviæ co. What is this but concentrated calcium?

It has already been stated that the foods found best for sprue patients contain quantities of this base. In a word, the best diet and the most

successful drugs hitherto used have supplied abundant calcium, a base which serum analyses have shown to be deficient (without exception in those tested so far), sometimes absolutely, more often relatively. The constituent parts are ill-balanced and the disease, therefore, calls imperatively, not so much for more abundant supply of calcium, as for support in regulating its disposal, that is, for parathyroid. The rest in bed which is important in the early stage of treatment puts the whole system under the best conditions for recovery of function.

In conclusion, when I wrote the last communication on this subject I could only speak of patients treated in England, but I stated that it would be of interest to know whether such felicitous results could be obtained abroad as I had had here with cases which, by the great kindness of the physicians to the Hospital for Tropical Diseases, I had been permitted to see, in addition to those whom I myself had treated outside. I have, since then, had reported to me eight cases, all in the Far East, all of whom had been under the usual methods of treatment for one and a half years or more. One patient had come over to this country for treatment, and, having been kept for a long time in hospital, had been obliged to return. He was assured that he would not live through the voyage and, in fact, was carried ashore on a stretcher. He began to undergo the calcium and parathyroid treatment (he had been refused it in England) and in three weeks was up and about, and in five weeks was carrying on his business.

If such fortunate results can be obtained in countries in which the disease is endemic—and in most of these it appears to be increasing in prevalence—much sickness and invaliding will be avoided and not a few deaths prevented, a marked contrast to what obtains at present—a miserable voyage home on ship's food, a stay of several months in hospital on the diet of a toothless infant, followed by months of food restriction, an ever-present haunting dread of a recurrence of symptoms, and a veto on any idea of a return to a tropical climate—in other words, the ruin of many a young man's career.

Dr. ARTHUR POWELL

I have little experience of coeliac disease, but in the hope of obtaining information on the subject of sprue, I have read all the available literature since Gee first wrote. Gee does not restrict his disease to children but includes a probable case of sprue in "an Englishman returned from India."

To me the term "coeliac disease" seems useless and incorrect. "Coeliac" is merely Greek for "abdominal" or "pertaining to the belly." Etymologically it should, therefore, include all diseases of the abdomen or of any of its contents. It has no advantage over the Latin "abdomen" or the English "belly."

Gee's infantile type of coeliac disease differs from sprue in its post-mortem signs. He says: "Nothing unnatural can be seen in the stomach, intestine or other digestive organs." Gibbons says: "Post mortem there is no sign of wasting of the mucous membrane or of ulceration throughout the whole intestinal tract." "There is no wasting of Lieberkühn's follicles." The liver was normal in half his cases, enlarged in the other half. The spleen also was enlarged in half his cases which were presumably Scotch and uncomplicated with malaria. Herter says, "Abnormalities of the liver have not been detected."

In sprue, on the contrary, I have never failed to find the liver greatly shrunken post mortem. The whole alimentary canal resembles the condition of those dead of famine; the wall of the intestine is thin, pearly, translucent;

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the mucous coat is abraded, and in parts the epithelium is as completely shed as if the gut had been prepared for use as a sausage-skin. From many of Lieberkühn's follicles the epithelium completely disappears. In most cases the pancreas did not seem shrunk out of proportion to the other organs of the wasted body, but in three cases I found it atrophied and sclerosed, the fibrous tissue being very dense around the arteries in a manner suggesting a syphilitic periarteritis. The weight of the pancreas in these three cases was 32, 26, and 24 grm. respectively. In celiac disease Gee says: "The microscope shows pus in the stools. In rare cases pus is so abundant that the stools consist of hardly anything else." This is not the case in sprue.

The pale, frothy, bulky stools containing a great excess of fat are common to both diseases. I was much interested in Dr. Miller's observation that fatty acids predominate in the stool at an early stage of celiac disease, and that an excess of fat persists after apparent cure, but chiefly in the form of soaps. This was also the case in two of my patients who had recovered from sprue. One neurotic individual insisted on having his stools analysed at intervals even three years after he was free from all symptoms and was taking the ordinary diet of his family which included no excess of milk or fat. The last few analyses showed over 50 per cent. of fat, chiefly in the form of soaps. In all the analyses I have made, or seen, of sprue stools the stools have never become dark on heating as Dr. Miller has found to occur in celiac cases.

In their course the two diseases differ, as shown in the following table.

| CÆLIAC DISEASE.                                                                                                                 | SPRUE.                                                                                                                                                                                                                                                 |
|---------------------------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Naturally tends to recovery.                                                                                                    | If untreated, death is almost certain.                                                                                                                                                                                                                 |
| "The face often remains plump."                                                                                                 | Shrivelling or withering of the body is the rule.                                                                                                                                                                                                      |
| "A tendency to dropsy is a constant symptom." (Gee.)                                                                            |                                                                                                                                                                                                                                                        |
| The liver is normal or enlarged.                                                                                                | In every definite case I have seen shrinkage of the liver has been a very striking sign. In some cases psilosis of the mouth may be absent or slight, but shrinkage of the liver is constant and its return towards normal most valuable in prognosis. |
| Gibbons reports: "Tenderness on handling the abdomen." "Great pain," "spasms of the abdomen," "vomiting is a striking symptom." | These are exceptional symptoms.                                                                                                                                                                                                                        |
| Attention is often first drawn to the disease by the intensely fœtid odour of the stools, "the stench is horrible."             | The stools have a characteristic mawkish smell but seldom merit such strong adjectives as Gibbons applies.                                                                                                                                             |
| "The appetite is poor or capricious."                                                                                           | Is usually so good as to interfere with treatment.                                                                                                                                                                                                     |

From the above accounts it is obvious that these diseases are two distinct entities.

To those who seek the causation of sprue in a vitamin deficiency I would point out that in India the disease is very rare in the poor underfed coolie.

It is most common in the wealthier and often over-fed European. In a regiment we find the officers, not the rank and file, attacked. Among the police superintendents and their families there are many victims, among the sergeants and constables few. A certain club, perhaps the most celebrated in India for its cuisine, has become notorious for the number of its resident members attacked by sprue. None of them suffered from deficiency or lack of variety of food. If an exclusive diet of milk for months has restored more sprue patients to health than any other treatment, the vitamin in question must be found in milk. The Englishman is almost the only inhabitant of India who takes milk and butter uncooked; almost all Indians will only drink milk after boiling, and eat butter in the boiled condition, "ghee," when the vitamins have deteriorated.

Lunn says pyorrhoea is almost invariable in sprue. This was very rare in my cases.

Sir Leonard Rogers truly says we must examine our cases at the very beginning if we are to find a causative organism, but the difficulty is that it is almost impossible to diagnose sprue at its very onset. Luys has said of gonorrhoea, "any fool can tell when a 'clap' has begun, but God alone knows when it is finished." God alone knows where sprue begins.

It is astonishing to notice how long the yellow santonin myth has survived when its author bases the use of the drug on the "fact" that it is a germicide or antiseptic. I have found *Bacillus coli* and *Monilia* grow in media saturated with yellow santonin which had been exposed to a tropical sun for years, just as freely as in control media. An astounding claim made by Begg is that "within twenty-four hours of administration of the first dose, the size of the liver will be found to be normal."

#### Colonel MACARTHUR

drew attention to a line of treatment which he said had given seemingly good results in a dozen cases. It consisted essentially in the daily addition of 2 drachms of marmite and the juice of two lemons to the diet ordinarily used in sprue. Most of the patients so treated gave a history of dietetic deficiency preceding the onset of symptoms, and it was thought that this factor might have determined the onset of the disease in these cases.

He differed from the opinion expressed by some of the speakers that there is necessarily a large quantity of fatty acids as compared with neutral fats in sprue stools, and he said that he considered that the fatty constituents varied according to the stage of the disease, quoting results of analyses in support of his view.

#### Dr. G. C. Low.

I agree with the view expressed by all the previous speakers, namely, that we do not yet know the exact cause of sprue. As so many points have been already raised, I will endeavour to limit myself to other points not yet touched upon, but still of some importance.

Without doubt the pathological condition of the disease is a sub-acute to chronic inflammatory process of the intestinal tract, this leading to gradual destruction of function and general atrophy of the bowel. What is the cause of this condition? Food deficiency can hardly cause it and it looks certainly more like a bacterial or organismal infection, but as yet no such organisms have been found. Monilias and streptococci certainly play a secondary part, and in many instances a very important part. Recently, in a severe case of

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sprue, the patient being practically on the point of death, removal of all the patient's teeth, which were in a very septic condition, led to a wonderful result—the patient at once beginning to improve and finally becoming apparently cured. This was before the introduction of the calcium lactate treatment and no such drug was given in this case.

Two other important points which have not been mentioned this evening are the following:—

(1) The occurrence of the disease in people who have long been out of the endemic area. I met with two such cases recently. Sir Ronald Ross, who saw one of the patients, said that it could not be sprue, but as all the clinical symptoms were perfectly in accordance with that disease, it will have to be assumed, then, that one can get in England, in adults, a disease exactly similar to sprue, which is not in accordance with generally acknowledged facts.

(2) When can a case of sprue be said to be cured? Many of the speakers to-night have mentioned wonderful cures of the disease, but if those cases be followed up over a period of some years, many of them will be found not to be cured at all, the disease really being only in abeyance. As we grow older we become more sceptical about specific cures for disease. Dr. Scott's calcium parathyroid theory is very interesting and one hopes that it is the solution of the problem, but still the only way to prove this definitely is to take two series of cases approximately alike, and apply the treatment to one of these series, keeping the other as a control. This would require time. At the present moment I have one case under treatment in which the calcium parathyroid treatment has done no good at all, and in fact it seemed to be doing harm. It has, therefore, had to be stopped.

The discussion we have had will be useful in the way of stimulating thought and encouraging further research into the ætiology of the condition. Without doubt sprue is one of the most important diseases of the tropics, and its frequency of occurrence seems to be on the increase.

Dr. P. MANSON-BAHR.

It seems to be generally agreed that the causative organism of sprue, if there be one, has not yet been found, and, so far as I can gather, the best informed opinion rather inclines to the view that sprue is neither a microbic nor a protozoal disease. I should very much like to know what is the essential pathological lesion? Manifestly, from what we know of the biochemistry of the disease and from what we know of autopsy findings in advanced cases, the pancreas is not at fault. But in order to discover the essential lesion it is necessary to examine early cases of the disease at autopsy. This is not easy, but in recent years two such opportunities have come my way. Both cases were those of comparatively young men, and both of short duration—under a year. Both died of general peritonitis secondary to perforation of the ileum. The small gut in both cases was extensively ulcerated, some ulcers being superficial, others deep. I hope soon to publish a pathological study on this subject, for a third similar case has recently been reported by Fischer.<sup>1</sup> Personally, I believe an ulceration of the ileum is the essential pathological lesion, and that the lesions are similar in extent and origin to those which occur in the mouth, and that with persistence of this ulceration a destruction of the villi takes place with consequent atrophy of the mucous membrane.

<sup>1</sup> Fischer u. von Hecker, "Beitrag zur Kenntnis der Sprue," *Virchow's Archiv*, 1922, ccxxxvii, pp. 417-448.

This process can be well seen in the specimens I have made from these two autopsies. I believe, then, that it is to the involvement of the small gut in this manner that the peculiar diarrhoea and the peculiar stools of sprue are due.

Dr. J. A. RYLE

spoke of a possible common anatomical basis for the fatty stools and calcium deficiency in coeliac disease, sprue, and certain other conditions.

He referred to two cases of copious fatty diarrhoea in adults, in one of which there was also tetany, and in both of which the symptoms were shown, in the course of a laparotomy, to be due to obstruction of the lacteals by tuberculous mesenteric glands. He also referred to a third case of a child with infantilism, late rickets, excess of fats (mainly as split fats) in the stools, a deficiency of blood calcium, and clinical and radiographic signs of tuberculosis.

On the basis of these cases he put forward the tentative suggestion that in coeliac disease and sprue there might also be some obstruction of the mesenteric lymphatics, probably of an inflammatory nature. The calcium deficiency in these cases would seem to be due to the excessive excretion of calcium in the form of soaps.

Professor DUDGEON (President)

asked Dr. Scott if he was satisfied that the preparation he employed was pure parathyroid extract. These glands are by no means easy to identify with certainty.

He related the case of a patient who had the symptoms of sprue, and who had not been out of England for seventeen years.

Dr. REGINALD MILLER

(in reply) said that the discussion had emphasized in his mind the extreme unlikelihood of sprue and coeliac disease being the same condition. No mention had been made of the occurrence of sprue in children.

Dr. H. H. SCOTT

(in reply) said that the parathyroid extract employed was pure, and that this was essential. If mixed thyroid and parathyroid extract were administered the result would be detrimental to the patient.

### **A Small Outbreak of Dysentery associated with an Unusual Bacillus.**

By J. BAMFORTH, M.D., D.P.H.

*Assistant Bacteriologist, St. Thomas's Hospital.*

(ABSTRACT.)

IN January, 1923, a small outbreak of dysentery occurred amongst the nursing staff of a certain institution. In all there were six cases. Only one case, the first which occurred, showed symptoms of any great severity. In the remaining five cases the symptoms were slight, and the patients were able to return to duty in seven or eight days from the commencement of the illness.

[December 3, 1923.]



In each of these five cases the onset was marked by fever, malaise, pain in the abdomen, frequency of motions and some tenesmus. For the first forty-eight hours or so the temperature was raised to 102° F. or 103° F., but subsequently quickly returned to normal. At the commencement of the disease there were, on the average, five to eight motions in the twenty-four hours, but in two or three days this frequency ceased and the number became normal. Blood and mucus or mucus alone were present in the fæces in the early stages.

The symptoms in the first case were of greater severity. The patient had a more protracted convalescence, and did not return to duty until more than a month had elapsed. There was fever for the first week. For the first three or four days the number of stools per twenty-four hours was six to eight, and blood and mucus were present. After this period the frequency gradually abated and the blood and mucus almost disappeared from the fæces, but there was some recurrence of this symptom during the early part of the second week, although at this period the temperature had returned to normal. The abdominal pain, a diffuse pain over the lower part of the abdomen, was a prominent feature in this case, and, with the tenesmus, lasted considerably longer than in any of the succeeding five cases.

Reviewing the symptomatology as a whole I think that this outbreak of dysentery would appear to have shown a distinct resemblance in severity to the milder cases of Flexner dysentery, as seen in Eastern countries, where dysentery occurs in epidemic form.

An investigation into the source of infection did not reveal anything. The cases did not occur simultaneously, but followed one another at intervals of a few days. All the nurses who contracted the disease took their meals in the same room, but all had been working in different wards. None had been employed in the nursing of sick children.

#### BACTERIOLOGICAL EXAMINATION.

Specimens of fæces were examined from four cases, namely, from the first case with severe symptoms (No. 1), and from three of the remaining cases (Nos. 2, 3 and 4). From three of the cases, Nos. 1, 2 and 3, a similar organism was obtained.

The fæces were plated on litmus lactose agar. The lactose used was chemically pure, and was unaffected by organisms of the typhoid, paratyphoid, and dysentery (Shiga and Flexner) groups.

The first specimen from case No. 1 was obtained on the third day of the disease. Blood and mucus were abundant, and an almost pure culture of the organism was obtained. A second specimen ten days later was examined with negative result.

Specimens of fæces from No. 2 (blood and mucus present) and from No. 3 (mucus present, but no blood macroscopically) were also obtained early in the disease. About a dozen colonies of the organism were found in No. 2, and a large number of colonies in No. 3 on the plates at the end of twenty-four hours' incubation.

Two specimens of fæces were received from No. 4 at an early stage of the infection, but examination yielded negative results.

The organism obtained from the first three cases was found to be a Gram-negative non-motile bacillus, showing the size and shape and the long involution forms seen in many bacilli of the coli-typhoid group. The colonies on litmus lactose agar at the end of twenty-four hours were definitely blue and their edges were round. They were much larger than colonies of the Shiga and Flexner

dysentery bacilli. At the end of forty-eight hours a further increase in size had occurred: the edges were becoming uneven and the blue colour intensified in their centres. The reactions of all three on carbohydrates, using 1 per cent. carbohydrate with 1 per cent. peptone, and a Lemco broth basis, and 1 per cent. of 0.06 phenol red as indicator, were similar with slight variations. Acid was formed in glucose, mannite and maltose at the end of twenty-four hours, and remained constant for a period of ten days. In saccharose, acid was formed about the fourth day, and in lactose about the seventh day. Though a slight preliminary acidity might occur in dulcitol, this medium became definitely alkaline on the third day, a reaction which was increased at the end of ten days' incubation. Inulin and salicin were also rendered distinctly alkaline on the fourth day, with an increased reaction later. No gas was formed in any of these media. Milk became acid at the end of twenty-four hours and more acid in forty-eight hours. This acidity remained unchanged. In the case of No. 1, slight clotting was observed on the ninth day, and was complete on the tenth day. No. 2 showed partial clotting on the twelfth day and complete clotting on the thirteenth day. No. 3 showed slight clotting on the tenth day and complete clotting on the eleventh day. Examined after ten days' incubation in 1 per cent. peptone all three organisms showed only a trace of indol formation. They were non-hæmolytic and produced no liquefaction of gelatin after fourteen days.

No effects were obtained by feeding a rabbit with living cultures of the bacillus. Pieces of carrot soaked with an emulsion were given daily for one week. The animal showed no symptoms and there was no loss of weight. An examination of the serum at the end of that time failed to reveal the presence of any specific agglutinin.

Rabbits were also inoculated with increasing doses of twenty-four hour live agar cultures of organisms Nos. 1 and 2. No symptoms and no loss of weight were observed to occur in the animals during the period of these injections.

In the case of No. 1, six injections were given at intervals of five or six days commencing with a dose of 100 millions and concluding with 1,500 millions. In testing the serum for agglutinins it was found best to employ live agar emulsions. Suspensions in Dreyer's veal broth medium, both living and formalized, did not give such satisfactory results. After this series of injections the serum of this animal was found to agglutinate all three organisms in a dilution of 1 in 2,000.

In the case of No. 2, three injections of 250, 500 and 1,000 millions were given, and the serum obtained agglutinated all three organisms in a dilution of 1 in 1,000.

A slight difference was found, however, by absorption experiments, and this is illustrated in the following tables:—

TABLE A.—RABBIT ANTI-SERUM PREPARED BY IMMUNIZATION WITH  
BACILLUS No. 1.

| End point of untreated serum | End point after saturation with Bacillus No. 1 | End point after saturation with Bacillus No. 2 |
|------------------------------|------------------------------------------------|------------------------------------------------|
| On No. 1—1 in 2,000          | Nil                                            | Nil                                            |
| On No. 2—1 in 2,000          | Nil                                            | Nil                                            |

TABLE B.—RABBIT ANTI-SERUM PREPARED BY IMMUNIZATION WITH  
BACILLUS No. 2.

| End point of untreated serum | End point after saturation with Bacillus No. 1 | End point after saturation with Bacillus No. 2 |
|------------------------------|------------------------------------------------|------------------------------------------------|
| On No. 1—1 in 1,000          | 1 in 200                                       | Nil                                            |
| On No. 2—1 in 1,000          | Nil                                            | Nil                                            |

It will be seen that saturation of anti-serum No. 2 with organism No. 1 failed to remove completely all agglutinins for organism No. 2.

The rabbit anti-serum No. 1 was also tested against typhoid, Shiga and Flexner antigens. In the case of Flexner two strains were used. At the end of five hours' incubation no agglutination was obtained even in a dilution of 1 in 25. Conversely, artificial agglutinating sera of typhoid, Shiga and Flexner, were tried against a twenty-four hour live agar emulsion of bacillus No. 1. At the end of two hours there was no agglutination, but after five hours all the dysentery sera gave a faint trace of agglutination in a dilution of 1 in 25 and the typhoid serum gave in addition a faint trace in 1 in 50.

An examination of the sera of the patients for agglutinins was made. The serum from Case No. 1 was obtained twelve days after the commencement of the illness. With bacilli Nos. 1 and 2 it gave similar results, namely, marked agglutination in dilutions of 1 in 25 and 1 in 50, a trace in 1 in 100 and a faint trace in 1 in 200. With bacillus No. 3 there was only a trace in 1 in 25 and a faint trace in 1 in 50. The serum from Case No. 2 gave with bacillus No. 2 marked agglutination in dilutions of 1 in 25 and 1 in 50, and a trace in 1 in 100. With bacillus No. 1 there was only a faint trace in 1 in 25 and not even this was present with bacillus No. 3. The serum from Case No. 3 gave no agglutination with any of the bacilli. Agglutination experiments with Shiga and Flexner antigens were negative (lowest dilution used, 1 in 25).

#### COMMENTS.

The fact that the organisms were present in considerable numbers in the faeces of the first three cases examined (in the first case almost in pure culture), and that two of the three cases showed definite and specific agglutination, make it appear very probable that this organism was the cause of this small dysenteric outbreak. In addition the organism was not agglutinated by any normal sera that were tried. The exact identity of the bacillus is impossible to determine with certainty. It may be that the bacillus corresponds with the organisms described under the name of *Bacillus coli anaërogenes*—a rather loose term which might include many different organisms. From reference to the literature it appears to correspond closely with the paradysentery bacilli described by Mita [1] as occurring especially in children but occasionally in adults. Mita describes two types of paradysentery bacilli. He states that their colonies on agar are larger than those of true dysentery bacilli, that they are non-motile, do not liquefy gelatin and do not give rise to indol formation. He states also that the older cultures coagulate milk in ten to fourteen days, that acid is formed slowly both in lactose and saccharose, and that whereas type 1 forms acid in maltose and dextrin after one day's incubation, in type 2 the reaction is delayed from one to two weeks. The organism appears also to resemble closely that described by Sonne [2] and considered to be the most frequent cause of dysentery in Scandinavia. Sonne describes a bacillus with rather large colonies showing uneven irregular edges. He states also that mannite and maltose are acidified immediately, saccharose after some days, and lactose later still. There was no formation of indol. Under the title of "Another Member of the Dysentery Group" an organism was described by Duval [3], and it appears to show some resemblance to the organism I have described. Duval states that this bacillus, although it produced acid in lactose, failed to clot milk. The serum of the patient from which it was obtained agglutinated the organism to a titre of 1 in 400, the Flexner-Harris bacillus to 1 in 200 and *B. typhosus* to 1 in 80. The blood of patients suffering from

typhoid agglutinated the organism in dilutions of 1 in 80 and 1 in 200. Rabbits inoculated with typhoid bacilli and with the organism produced common agglutinins for both cultures. In the case of the bacillus isolated from this small outbreak no such affinity to the typhoid and dysentery groups, could, however, be demonstrated.

It would appear that, in the investigation of cases of diarrhoea and dysentery, organisms of the slow lactose-fermenting type are worthy of attention. Considerable confusion exists in bacteriological literature with regard to organisms of this class. Many bacilli showing cultural characteristics with only slight differences are encountered from time to time in the examination of both fæces and urine. So far those which I have isolated have shown no serological relationship to this organism.

In conclusion I desire to thank Professor L. S. Dudgeon for much help and advice in this investigation.

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Colonel H. MARRIAN PERRY (discussing the organism isolated in this series of cases) inclined to the opinion that it corresponded with the bacillus of Sonne, and drew attention to the fact that up to the present date the isolation of this organism had not been recorded in this country. He stated that W. S. Patterson had recently placed on record the occurrence of a series of cases of entero-colitis and summer diarrhoea in Southern Australia, from which he (W. S. Patterson) had recovered a bacillus identical in its biochemical reactions with the organism Dr. Bamforth had just described, and that Patterson had satisfied himself on serological evidence that the organism with which he was concerned corresponded with the bacillus of Sonne. He suggested that Dr. Bamforth might carry his investigation further by making a serological comparison between the organism under discussion and the bacillus of Sonne, and so determine its identity with or dissimilarity to this strain.



## Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

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### Schistosomiasis in China: Biological and Practical Aspects.

By Professor ERNEST CARROLL FAUST, M.A., Ph.D.

(From the Parasitology Laboratory, Department of Pathology, Peking Union Medical College.)

WHILE the disease, schistosomiasis japonica, has been known for more than forty years, and while the helminth which is the ætiological agent of the infection has been described for nearly two decades, without exception all of the important steps in the solution of the various related problems of ætiology, pathology, therapeutics and prevention of the infection, have been achieved on the Japanese variety of the disease. It is true that *Catto* (1905) described the worms from a Chinese of Fukien Province, but there is little evidence that the infection was incurred in China.

Following the discovery of the Oriental species of schistosoma, the *Schistosoma japonicum*, by *Katsurada* (1904) and *Catto* (1905), and the description of its ova as found in faecal examinations, several missionary physicians in China observed in their clinics the presence of similar ova in stools of patients who suffered from "Hankow Fever," "Kiukiang Fever," "Yangtse Valley Fever," "Urticarial Fever," or some other locally-named malady. *Katsurada* examined the ova from one of *Logan's* cases (1905) and pronounced them to be identical with those of the Japanese blood fluke. Later *Peake* (1909), in reporting on the disease in his vicinity in Hunan Province, demonstrated the presence of a lateral spine on the Chinese eggs. This was later confirmed by *Leiper* (1911) and *Looss* (1911) from Japanese material. While progress was made in determining the distribution of the infection in China and in studying the clinical picture of the disease, the Japanese form of schistosomiasis came to be accepted as an entity in China without actual proof having been established of its identity with the Japanese infection.

Some clinical microscopists in China have seen the egg in various stages of development and have observed the phenomenon of the hatching of the miracidium. A few have likewise secured the adult schistosomes from autopsies, but no account has been published of a comparison of adult forms with Japanese specimens. Up to the present time the molluscan host has not been found or identified and the cercarial stage has not been observed. Furthermore, experimental proof has been altogether lacking for the worm as it exists in infected individuals in China.

Subsequent to the discovery of *Schistosoma japonicum* by *Katsurada* (1904) and *Catto* (1905), various Japanese investigators undertook to attack the life-history problem of the organism. By 1909 *Fujinami* and his associates were able to show that infection ordinarily occurred in animals whose skin was

exposed to "infected water," while *Miyagawa* (1912, 1912a), traced the invading organism from the skin to the lungs of experimental animals and found both the venous and lymphatic systems to constitute paths of migration to the lungs. Miyagawa predicted a mollusc as intermediate host and by a series of exclusion experiments showed the mollusc commonly known as *Katayama nosophora* to be the species involved. He described the fork-tailed apharyngeal cercaria as the invasion stage for mammals. However, it was left for *Miyairi* (1913) and his associates to establish the first proof regarding the life-history of this species. Following this, *Leiper and Atkinson* (1915) verified the life-history for the worm in Japan, and later *Cort* (1919, 1921) described in detail from Japanese material the cercarial stage and the development of the fluke within the final host.

In setting out on certain practical aspects of schistosomiasis japonica in China, it occurred to me, in association with my colleague Dr. Henry Edmund Meleney, that the life-history of the Chinese form must be demonstrated and its identity with the Japanese worm established before further studies were contemplated.

This was possible of accomplishment and was actually concluded previous to our discovery of the intermediate host in China. However, the discovery of the Chinese mollusc, serving as intermediate host not only facilitated our study but afforded an immediate opportunity for attempting certain practical phases of the problem.

The course of events in the investigation which we carried out was as follows: (1) The development of the miracidium-sporocyst stage of the Chinese worm was first demonstrated in the Japanese snail. (2) The Chinese intermediate host was discovered in the Soochow area, and the experimental proof established that this was a true intermediate host of the worm by infecting mice with cercariæ from infected Soochow snails, and infecting uninfected Soochow snails with miracidia hatched from Chinese eggs. (3) The development of the Japanese worm in the Chinese snail was then demonstrated.

The proof of the ability of the Chinese worm to develop in the Japanese snail was made possible through the gift of a large number of infected *Katayama nosophora*,<sup>1</sup> which Professor S. O. Yoshida, of Osaka, Japan, sent me in December, 1921. The molluscs were practically all alive when they arrived in Peking. A preliminary examination of several thousand of them showed forked-tailed cercariæ to be present in less than 1 per cent. of the specimens. In February, 1922, we had under observation a Chinese patient with schistosome eggs in the stools. The stools of this patient were washed for several days, the miracidia hatched out, and a number of the Japanese snails subjected to infection. Previous to this time we had found that the ordinary molluscs of the Peking area, *Vivipara quadrata*, *Planorbis möllendorfi*, and *Lymnæa plicatula*, could not be infected with this species of miracidium. In the case of specimens of *katayama*, however, the miracidia became positively attracted to the snail as soon as they came within a few millimetres of it, and proceeded to attack the nearest part of the mollusc. In this way a series of *Katayama nosophora* was obtained infected with miracidia of *Schistosoma japonicum* from Chinese

<sup>1</sup> The discovery of the type specimens of *Blanfordia japonica* A. Adams in the Division of Molluscs, U.S. National Museum, and careful comparison of these specimens with those known to be the intermediate hosts in representative districts of Japan endemic for schistosomiasis, demonstrates that the latter are not *Blanfordias*. This fact was first pointed out to me by Dr. Nelson Annandale, Director of the Zoological Survey of India.

sources. By killing these in pairs every two days for seven weeks, the course of development within the mollusc was available for study. Examination of sections of forty-six of these molluscs showed only five to be uninfected, while one contained the natural Japanese infection as well as the experimental one. Although this experiment was not prolonged to a point to allow the cercariæ to complete their development, this demonstrated clearly that the Japanese intermediate host of *Schistosoma japonicum* was susceptible to an infection of schistosome miracidia from a Chinese source. This led us to postulate that the molluscan host in infected districts in China must be either the same species of molluscs or a closely related one.

In order, then, to know the conditions under which the snail was to be found in China, we studied the bionomics of the Japanese species, its preference for certain types of water, its relation to the various water levels and the damp soil stratum immediately above the water line, as well as the level of the water at which it was attacked by the miracidia. From our observations we predicted that the snail would be found only near quiet, clear, shallow water; that it would more likely be found on overhanging moist banks at the edge of the water than in the water itself; and that infection by the miracidia would ordinarily occur just beneath the water level. We then chose Soochow, an endemic centre in the lower Yangtse Valley, where conditions were favourable for study, and during the month of August, 1922, Dr. Meleney undertook to discover the intermediate host of *Schistosoma japonicum* for that area.<sup>1</sup> He made the Soochow Hospital his headquarters, and in co-operation with Dr. J. A. Snell, entered upon a search for the mollusc in the region immediately to the north and east of Soochow City, where clinical cases were known to occur. The party left Soochow by motor boat along the main canal, passed north to the railroad bridge, thence by way of a secondary canal into a region which had frequently sent cases to the hospital. After some search in the rice fields, where only dry shells were found, a man was encountered with marked clinical symptoms of schistosomiasis japonica. Examination of faecal specimens from persons inhabiting a small village in the vicinity revealed eggs of *Schistosoma japonicum*. The village was observed to be on a terminal canal, and examination along the overhanging banks of this waterway just above the water's edge disclosed many specimens of a minute tapering snail, in many respects resembling *Katayama nosophora*. The snails were taken back to the hospital laboratory and dissected in the usual manner. Twenty-eight per cent. of the snails contained schistosome cercariæ, while one snail contained an oculate fork-tailed cercaria. The former were placed in a museum jar in water, and laboratory mice were submitted to infection. Fourteen days later one of these mice was autopsied and about 100 immature schistosome worms were obtained from its liver and mesenteric veins. Another mouse was kept until the thirty-first day, when examination at autopsy revealed the presence of many mature male and female schistosome worms morphologically similar to Japanese specimens which we had previously studied. Some of this same collection of snails harbouring cercariæ were later taken to Peking and used to infect mice which had been born in Peking and had, therefore, never been near an endemic area. Another collection of the same species of snails within the city of Soochow, from the bank above the water's edge, was found to contain no infection, so these were submitted to an invasion of schistosome miracidia hatched

<sup>1</sup> See map at end of paper, p. 42.



out from local material. The snails were taken to Peking and kept in moist earth. After about three months they were placed in water, where they discharged large numbers of schistosome cercariæ, which were used to infect dogs.

A further survey of this infected district just outside of Soochow revealed the presence of this species of snail in other terminal and some lateral canals but never in any main canals nor in the rice fields. The fact that the rice fields are above the canals, and that they are wet only for a few months of the year when water is treaded up to them in sluiceways, probably explains why the snails are not found in the rice fields themselves. Their preference for quiet banks explains equally well why they are not encountered along the main canals which are common thoroughfares of traffic.

Many other species of snails from the Soochow area were collected and examined but none contained the schistosome cercariæ. When examined later, none of these subjected to invasion by miracidia hatched from eggs of *Schistosoma japonicum*, harboured schistosome larva.

In order to complete our proof of the ability of the miracidia of *Schistosoma japonicum* to infect any member of the group of closely related snails, certain of the Soochow schistosome snails that were known to be uninfected were submitted to an infection with miracidia hatched from eggs obtained from the fæces from dogs that had been infected with Japanese material. These were equally infectible; the miracidia readily attacked them. On examination about ten weeks later these snails contained mature and maturing cercariæ.

The mollusc harbouring the schistosome cercariæ from the Soochow area has been kindly identified for us by Mr. Bryant Walker, of Detroit, Michigan, who regards it as *Oncomelania (Hemibia) hupensis* Gredler.

More recently a search was made by me for the molluscs involved in the schistosome infection in Chekiang Province, directly to the south of Soochow, in the vicinity of Kashing and Shaohsing. In the former area *Oncomelania hupensis* was found to be present in certain terminal and lateral canals. In the Shaohsing region the form *Oncomelania* was not encountered, but a species of *Katayama* was implicated from a similar locality. While the *Oncomelania* has been found in waterways which constitute part of a low level canal system and while the *Katayama* has been recovered from protected coves of canals supplied directly by mountain streams, the *edaphic*, i.e., local requirements in both instances are the same, namely a protected quiet water supply which serves to moisten the humus and loose soil along the banks where the snails are found. Clayey or packed earth is incompatible with the existence of this group of snails.

In other centres of China endemic for schistosomiasis japonica, where the mollusc involved has not yet been looked for, it is expected that the snails harbouring the infection will be found to be of the same or closely related operculate species, with the characteristic habit of residing at times in the water but usually on the moist banks of quiet canals or ditches.

#### LIFE HISTORY OF *Schistosoma japonicum*.

The purpose of this portion of the study as worked out by us was two-fold: (1) To obtain certain biological and morphological data in respect to the worm which were doubtful or obscure, and (2) to determine certain phases of the life history which might be used as barriers in undertaking control of the infection.

The egg of *Schistosoma japonicum* when first laid by the female is a double

convex oval disc, with a thorn or abbreviated spine on one side near the anterior end. It is always present in the immature eggs, but is usually much less conspicuous in the mature egg. The mucoid secretions from the lateral glands of the miracidium within the egg-shell ooze out through the shell and serve as an outer sticky cover for the egg.

The eggs hatch best in a water medium where fermentative agents are reduced to a minimum. In pasty stools exosmotic phenomena soon set in and produce the death of the miracidium within the shell. The miracidium dies in a medium above 45° C. but will survive a temperature of 4° C. for months at a time. It will live in freezing temperatures for a brief period, as low as minus 17° C. for an hour or less, provided the medium is water and not faeces. It seems probable, therefore, that the eggs can survive all water conditions which obtain in endemic areas, but soon become non-viable in night-soil dumps which are exposed to the weather.

It is possible now to point out that *Schistosoma japonicum*, like *Schistosoma hæmatobium*, has two or more closely related molluscan hosts.<sup>1</sup> Furthermore, miracidia from Japanese sources will readily infect the mollusc ordinarily harbouring only the Chinese variety of the fluke, and miracidia from Chinese sources will readily infect the mollusc ordinarily harbouring only the Japanese variety of the fluke, a fact which I am not aware has yet been proved to hold good for the intermediate host species of *Schistosoma hæmatobium* or *Schistosoma mansoni*.

The miracidium of *Schistosoma japonicum* displays a chemotactic response not only to the susceptible mollusc when this chemotaxis is once established but also to the mucus secreted by the mollusc. Experimental data show that all soft parts of the susceptible snail are not only equally liable to attack but also to penetration. In other words, there is no selective penetration, since invasion through the tissues of the head is as common as penetration of the gill filaments. In the former case, as in the latter, the miracidium penetrates the tissues as a miracidium, with its full equipment of cilia and secretory glands. Several days elapse before these structures disappear. In the meantime the larva has either secured entry into the lymph spaces of the mollusc or has developed an artificial lymph space around itself: sooner or later it gains access to natural lymph channels. All schistosome miracidia which have actually secured penetration of the snail apparently have a chance of full development, although those that secure invasion through the direct route, i.e., the gill filaments, usually arrive in the liver lymph spaces first. The development of primary and secondary sporocysts, and the development of the fork-tailed cercariæ to maturity require a period of seven to nine weeks. Under natural conditions in the field, seven weeks is probably the period ordinarily employed.

It appears that the mature cercariæ accumulate within the snail until a certain tension of the host tissues develops, when they erupt spontaneously. When infected snails which have been dry for some time are placed in water they discharge their brood within a few hours. If they are taken out of water and placed in dry earth for a period, then later returned to the water, a second discharge usually results, although this is not ordinarily as heavy as the first. One such group of snails was alternately changed from water to soil and returned to water several times until it was believed that all of the

<sup>1</sup> In a private communication Dr. H. Kobayashi states that there are two distinct varieties or races of *Katayama nosophora* in Japan, one found in Saga Prefecture and the other in Yamanashi Prefecture and along the Tone River (and perhaps in Lake Utsushima).

cercariæ had been discharged. The snails were then killed and sectioned and upon examination revealed large numbers of mature and immature cercariæ in the inter-hepatic lymph spaces. It is evident, therefore, that all cercariæ do not develop synchronously and that cercariæ do not all leave the mollusc upon reaching maturity. Cercariæ of *Schistosoma japonicum* ordinarily swim out of the snail shell tail first, employing an anchor-fluke movement. Soon after a brood has been discharged they are seen to congregate under the surface film of the water immediately above the snail, where they become pivoted by the attachment of the ventral sucker to the surface film. Any object which comes in contact with this surface-film is likely to remove cercariæ from the water. It seems probable, therefore, that the actual first contact of a schistosome cercaria with a susceptible mammal is a fortuitous one, and is not superinduced by a chemotactic stimulus as is that of the miracidium for the susceptible molluscan host.

Infection of an experimental mammal may be brought about by an intra-peritoneal injection of cercariæ, although this, of course, is likely to introduce bacterial contamination.

The normal route of invasion of the mammal, namely, that through the skin, may be described as follows:—

The cercaria attacks the skin with the capillary ends of its cephalic ducts, pouring out through them the proteolytic ferments secreted by the cephalic glands. During this period, if not before, the tail of the organism is dropped, so that the larva enters the mammal not as a cercaria but as an agamodistome. Penetration may be effected in a few hours or may require a longer time. The earliest stage at which we have been able to recover young living worms from experimental animals is twenty-three hours after infection, when specimens were obtained from the popliteal lymph node of the rabbit. It usually requires two days or more for them to reach the venous circulation. They pass immediately through the heart to the lungs, where a certain number, particularly in heavy infections, penetrate the tissues of the lungs, thereby producing lesions, and thus reach the pleural cavity. They become side-tracked here, however, and, being unable to continue their course, degenerate in a few days. The majority, however, pursue the normal course and pass through the capillaries of the lungs into the arterial circulation. They apparently have an equal distribution throughout the systemic circulation, since at this stage they are recovered from peripheral, renal and splenic arteries, as well as from the mesenteric system. But only those that get through the mesenteric capillaries into the portal system arrive at a suitable location for growth. The others behave as foreign-body emboli and are probably the cause of the dermatitis observed early in the infection. The course within the body is, therefore, one definitely involving blood-channels.

While a certain development takes place during the migration from the skin to the portal system, functioning of the digestive tract takes place only after arrival within the portal vessels. It seems highly probable that the young schistosomes first pass into the intrahepatic portion of the portal vessels, where they reside until sufficient muscular development has been attained, so that they may crawl back into the mesenteric veins. Here they mate (at about the seventeenth day) and become adult, some as early as the twenty-fifth day. Worms at this time lie in the smaller mesenteric veins and even in the capillaries, where the eggs are laid, and, by pressure, and later, by digestive ferments from the miracidium within, break through to the intestinal lumen.

During the migration from the skin to the portal system, but more particularly later, within the portal system, the worms respond to a definite growth stimulus. Our first experimental worms were recovered from the liver seventy hours after infection, but migration from the lungs continues through to the seventh day, after which time the worms are too large to pass the capillaries and, having used up all of their digestive gland secretions, are unable to migrate of their own accord.

#### DISTRIBUTION OF SCHISTOSOMIASIS JAPONICA IN CHINA.

The disease produced by the worm, *Schistosoma japonicum*, is now known to be present throughout the Yangtse Valley from Ichang, 350 miles above Hankow, to the sea. It is not a disease of the Yangtse River, meaning by that the main stream, but is confined to the backwaters and flood areas which in the rainy season constitute one tremendous inland sea, but which in dry weather are confined to lakes, marshes, swamps and canals. All the inhabitants throughout this area are rice farmers or river boatmen who are constantly exposed to infection. On the north side of the Yangtse the infection extends away from the river in Anhwei and Hupeh Province, but only within the limits of the overflow area. The disease is not found in the northern part of Hupeh and certainly not on the wheat-growing territory of Honan. South of the river the infection centres around the three large lakes, Tung-Ting, Poyang and the Tai-Hu, their tributaries and their overflow. The whole country around Tung-Ting Lake is infected. The disease is known as far up the Siang River (Hunan) as Yungchow and Peishui, from which places Peake reported his first cases (1909). From Yochow on the east side of Tung-Ting Lake, across to Kiukiang, a line may be drawn, between which boundary and the Yangtse River one solid section of infection exists. Poyang Lake has the infection on all sides except in the north-east corner, where the country is composed of uplands and not given over to irrigation. The Tai-Hu is also an important centre of infection, particularly on the east side between Soochow and Shanghai. The disease has been found within Soochow city, while it exists at the very gates of St. John's University, Shanghai. It extends as far south in this area as Hangchow, Shaohsing and up into the mountain streams of Central Chekiang.

The incidence of infection among this population has been estimated at from 1 to 7 per cent., but surveys in endemic farm villages show that 50 to 95 per cent. of the inhabitants of some districts are infected.

Other smaller centres of schistosomiasis japonica are known for China as follows: (1) Jenshowhsien, in a rice-growing centre of 800 ft. altitude in the Central Szechuan plain, West China; (2) Taching, near Wenchow, South Chekiang Province;<sup>1</sup> (3) Drongloh, across from the Anchorage below Foochow, Fukien Province;<sup>1</sup> (4) Kaying and Kityang, above Swatow; and (5) Shuichow on the North River above Canton. In other areas the disease is as yet unknown.

While there is little likelihood that any other heavy endemic area of schistosomiasis japonica will be demonstrated in China, it seems probable that isolated spots of endemicity may still be reported, or even areas of mild infection.

Altogether the endemic territory in China is much more extensive than the five known infected districts in Japan. The latter amount to considerably less

<sup>1</sup> Cases of schistosomiasis japonica in the pauper hospital at Singapore are invariably those of Chinese from Amoy, Foochow, or Swatow.

than one-tenth the known territory endemic for the disease in China, and are, in reality, minute replicas of the Chinese areas, although on the whole they are probably more intensively infected.

While foreign sport dogs are commonly found to harbour the disease in endemic areas in China, native dogs are less heavily infected, this being possibly due to their dislike for water. In China man alone is the important definitive host of the disease.

#### DIAGNOSIS AND THERAPY IN SCHISTOSOMIASIS JAPONICA.

Although all clinicians in centres of China endemic for schistosomiasis japonica are more or less familiar with the advanced symptoms of the disease, little careful study has been made in China of the early stages of schistosomiasis. The natives do not seek medical assistance for the infection until it is far advanced, while most early cases which are diagnosed by the presence of eggs in the faeces are usually afflicted with more serious complications.

From the time of exposure to the infection until the initial dysentery sets in actual knowledge of the disease is very limited. A dermatitis is described by some Japanese investigators immediately following the penetration of the young schistosomes through the skin. This I have experienced even on exposure of my arm to a non-mammalian schistosome cercaria, the rash developing about half an hour after the exposure was made and lasting for about three hours. The rash is probably explained on the basis of the microscopic lesions produced by the hollow boring spines which cap the cephalic ducts of the cercaria. The irritation is probably both mechanical and chemical in its nature.

After penetration of the schistosome larva into the body, the passage through the lungs *en route* to the systemic circulation undoubtedly causes a certain amount of pulmonary disturbance, particularly if the infection is a heavy one. Petechial lesions of the lungs, and penetration of a certain number of the young worms through the tissues into the pleural cavity, are characteristic of this phase (from the third to the eighth day) in experimental animals. Likewise, when the successful worms pass through the mesenteric capillaries into the portal system, irritation no doubt results. Those individuals which reach capillaries in unsuccessful channels, such as the capillaries of the peripheral circulation, become foreign body emboli, and as such may be the exciting cause of the so-called "urticaria" that has been ascribed to the infection. Lambert (1910), Logan (1912), Laning (1914), Mann (1916) and others, who have seen early foreign cases contracted in the Yangtse Valley, describe an urticaria from two to five weeks following the supposed exposure to the "infected water." The urticaria observed is usually a prodromal symptom of the dysentery, which develops some days later. During this period, as well as later, clinicians report a high eosinophilia, although analysis of these cases fails to show that the high percentage of eosinophil myelocytes may not be due to other accompanying helminthic infections<sup>1</sup>. It seems probable that in some cases a high eosinophilia is suggestive, but in no instance can it be regarded as diagnostic *per se*.

In experimental dogs the eggs first appear in the faeces from four to five weeks following infection. The earliest period at which I have found eggs is twenty-eight days after infection. In some experimental dogs a preliminary evacuation of blood and mucus in the faeces was observed a few days before the actual schistosomiasis dysentery set in.

<sup>1</sup> Recent clinical studies of Dr. H. E. Meleney have shown a high correlation between schistosomiasis japonica and high eosinophil count.

From this time onwards the clinical syndromes of the disease have been carefully observed.

Houghton (1910) has classified the advanced cases as follows: (a) Typical cases, with large liver and spleen, and with fluid in the peritoneal cavity and blood and ova in the stools. (b) Cases showing only splenic enlargement, with or without blood or ova in the stools. (c) Cases negative except for marked eosinophilia. (I have indicated my doubts about the specificity of a high eosinophil count under certain circumstances.) (d) Latent cases, showing ova in the fæces, but no bodily reaction.

The earlier treatment of schistosomiasis in China was entirely symptomatic. In Japan quinine and arsenic were tried without success. In 1913 Hutcheson used emetine in treatment of some of his cases in Kashing, and believed that the drug deserved further trial. Following the introduction of tartar emetic treatment for Egyptian bilharziosis by Christopherson in 1917 (1918) certain hospitals in China undertook to use this drug in their schistosomiasis clinics. In all cases within my knowledge the patients have been taken into the hospital and treated in the wards. This is necessitated by the fact that most hospitals are some little distance from the endemic villages, so that the patients cannot come for dispensary treatment. Two other factors enter into the problem in China. Patients who improve under treatment want to return home immediately, while those who do not improve immediately do not wish to stay for fear of dying in the hospital. This custom is not peculiar to the Chinese, but the Chinese are too philosophical to be induced to make treatment a fetish. In the second place, a large proportion of the cases observed in the clinics in China represent the terminal stage of the disease, with enlarged cirrhotic liver and spleen, tremendous distension of the abdomen due to ascites, emaciation, and body ulcers. For these there seems little, if any, hope.

Clinicians in China who have given tartar emetic a trial agree that "results are uniformly good when patients have stayed in the hospital until pronounced cured, and when complications have been avoided." Tootell (1923) of Changteh, Hunan, reports cures for 50 per cent. of all patients who received treatment, while 29 per cent. more were improved, although they still harboured eggs on leaving the hospital before treatments were completed. In other centres where the treatment has been given a fair trial similar success has been attained. Although Reed (1915) regards emetine as ineffectual, Tyau (1922) favours its use.

While it is too early as yet to make comparisons of Chinese schistosome cases with Egyptian bilharzia patients with respect to antimony therapy, it seems probable that under similar conditions the drug is equally potent.

In concluding this phase of the subject it may be pointed out that a simple specific biological reaction is needed to test the presence of the schistosome in the human body previous to the onset of the dysentery and later on in chronic cases and those under treatment where eggs are not found in the fæces. Some specific test less complicated than complement fixation must be found which can be used in hospitals not equipped to do Wassermann reactions.

#### CONTROL OF THE DISEASE IN CHINA.

During the past decade following the discovery of the molluscan intermediate host of *Schistosoma japonicum* in Japan, at least four of the five endemic centres in that country have been carefully surveyed in order to determine the distribution of the infection on the islands while the mollusc has been located in one area in which the disease is as yet unknown. Prophylactic measures

have been undertaken under government supervision to wipe out the molluscs in the endemic areas of Japan. The most successful and practical method yet devised consists in liming the banks along the infected or suspected canals and drainage ditches. It will be remembered that drawing off the water and allowing the ditches to dry out at intervals, in some such way as is recommended for Egypt, would serve no purpose in areas infected with oriental schistosomiasis, since the snail in the latter case is an operculate one and withstands a considerable amount of desiccation. In China, the simple but effective measures adopted by the Japanese are still impracticable. In the first case the intermediate host is known as yet only in the immediate vicinity of Soochow, Kashing and Shaohsing. While the distribution of the disease leads one to believe that the mollusc involved has the same habits of existence in all endemic centres in China, the actual proof yet remains to be demonstrated. In the second place, the whole endemic territory in China is a tremendous one. Neither the disease nor the mollusc is equally distributed over the Yangtse Valley. The infected areas are confined especially to certain districts, such as Soochow, Kiukiang and Changteh. A practical programme involves a search for the intermediate host in such areas of heavy endemicity, gradually working out from these centres to the districts of lesser infection. It must be remembered, however, that such a programme requires considerable skill on the part of the surveyor, since the mollusc is minute, is easily confused by the layman with immature specimens of the ubiquitous *Melania cancellata*, and is unknown to the natives. On the whole, the problem in China seems more likely to be solved on the basis of night-soil control. One more fact must be kept in mind in considering such an undertaking so intimately concerning the native farming class, namely, their suspicion that only harm can result from any stranger trespassing on their domain. Coupled with this is the improbability of securing any government co-operation at the present time in China. In spite of these unpleasant facts, the problem of eradicating schistosomiasis from China is ultimately hopeful. One must bear in mind that China is an ancient country, that she moves slowly and deliberately, but that she moves surely. The next fifty years will bring much in the way of reorganization and development in China. It seems not too much to expect that public health and preventive medicine will follow closely upon the steps of improved agriculture and commerce, and that in this scheme of affairs schistosomiasis will not long be allowed to remain an uncontrolled infection in the heart of the country.

#### SUMMARY.

In working out the problem of schistosomiasis japonica for China, the development of the miracidium-sporocyst stage of the Chinese worm was first demonstrated in the Japanese intermediate host, *Katayama nosophora*.

Following this the Chinese intermediate hosts were discovered in the vicinity of Soochow, Kashing, Shaohsing, Fatsan, and experimental proof of their right to this claim was established.

The development of the Japanese worm in the Chinese snail was then demonstrated.

The important factors in hatching of the egg are the maturity of the miracidium and lack of fermentative agents in the medium. The larva withstands cold but dies at temperatures above 45° C.

The miracidium may enter the tissues of the molluscan intermediate host either by way of the gill filaments or by penetration through the solid tissues of the head or foot.

The cercaria attacks the skin by pouring out proteolytic ferments through the capillary ends of its cephalic ducts, thus digesting and mechanically battering its way through the skin of the definite host.

Penetration once effected, the young worm utilizes the blood channels for its migration through the body to the portal vessels, normally passing through the capillaries of the lungs and thence by way of the left heart into the systemic circulation. While the chances are equal that the worms will pass to all parts of the systemic circulation, only those that pass through the mesenteric capillaries into the mesenteric veins are able to continue growth. Mating has been observed as early as the seventeenth day and mature eggs have been found in faeces twenty-eight days, or later, following infection.

The disease, schistosomiasis japonica, has a wide distribution throughout the Yangtse Valley, but is particularly well established in the regions around Soochow, Kiukiang, Tsaoshih, Wuchang, Yochow and Changteh. It has been located in isolated districts on the central Szechuan plain, in southern Chekiang, at Foochow, above Swatow and on the North River above Canton.

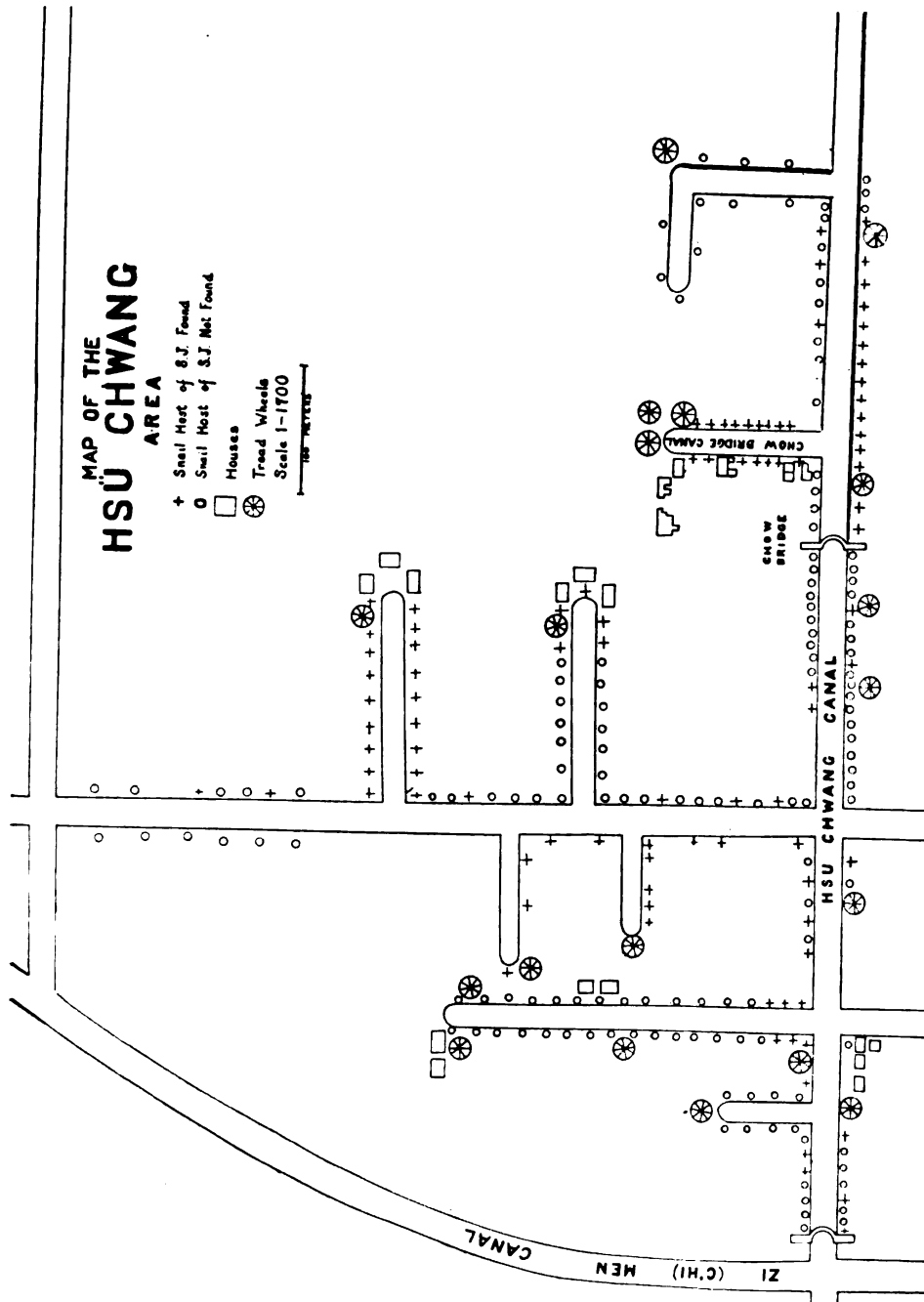
With a full course of tartar emetic treatment available about 50 per cent. of the patients suffering from schistosomiasis are cured, while another 29 per cent. are improved. Still others come for treatment too late to secure effective therapeutics. Antimony tartrate is, in most cases, specific for the infection.

While the methods adopted in Japan for eradicating the molluscan host are still impracticable in China except in certain limited areas, it seems reasonable to believe that in the course of a few years measures will be effected to stamp out the infection in China.

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Schistosome area surveyed  
near Soochow

## DISCUSSION.

Professor R. T. LEIPER remarked on the importance of the new observations on the etiology of schistosomiasis in China made by Dr. Faust and his colleagues. With regard to the identity of the Chinese and Japanese blood flukes it would be recalled that Catto's case came from Fukien, one of the endemic foci in China listed by Dr. Faust. Catto's parasites had originally been named *Schistosoma cattoi* by Blanchard but were later recognized by Looss as morphologically identical with *S. japonicum* from Japan. Later workers including the speaker had had opportunities of comparing adult worms from both countries and had accepted their identity. In any event the capacity of either species to develop in the related molluscan hosts of both countries was not proof of their specific identity, although a negative result would have been good evidence in favour of their recognition as separate species. The occurrence of a lateral spine in the egg was first recorded by Leiper (1911) on material received from China and from Japan. Peake in 1911 referred to the fact that in an earlier paper (1909) he had overlooked this character to which his attention was for the first time drawn by Leiper. The statement that Miyagawa described the fork-tailed apharyngeal cercaria as the invasion stage for mammals was puzzling, for the title of this author's paper (1912) represented that it dealt with the "Migration of the Miracidium in the Skin."

Professor L. S. DUDGEON (President) said the Section was greatly indebted to Professor Faust for the valuable paper which had been read. When Dr. Faust was in London, in July, on his way to the United States it was too late in the season for the ordinary meetings and circumstances did not admit of a special meeting being held at a short notice. Professor Faust kindly left the paper together with the very striking photographs which were shown to-night on the epidiascope. He wished Professor Faust had been present to-night to receive the thanks of the Section for the paper.



## Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

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### Case of Actinomycosis of the Parotid Region from Teheran.

By J. B. CHRISTOPHERSON, C.B.E., M.D.

THE patient, a male, aged about 30, in the telephone service. He went out to Persia three years ago, and two years later (in February, 1923) noticed a swelling in the left parotid region. Actinomycosis not being common in Persia,



various causes for the swelling were suggested, e.g., mumps, impacted wisdom tooth, osteo-periosteitis from a tooth. A month later the skin broke in several places along the line of the scar now seen on the face, and pus containing the little yellow "grains" was discharged. The diagnosis is not in doubt, but it is open to question as to how the fungus gained entrance.

My—T D 1

[February 4, 1924.]

My experience of the tropics was gained in the Sudan, where actinomycosis (mycetoma) is frequently seen. In cases occurring there the entry is made nearly always through an abrasion in the skin. The patient now shown is quite sure there was no cut, scratch or abrasion on the cheek before the swelling occurred. The gums are sound, the teeth are not under suspicion; there is nothing to point to the tonsil as a possible route for entry. There is a roundish, firm swelling, irregular in outline, in the neighbourhood of Stenson's duct, and it appears to me probable that the infecting fungus found an entrance from the mouth by this route; this swelling has appeared during the past month.

The patient is on potassium iodide by way of treatment; he has been taking for the last three weeks 40 gr. three times a day. There is no sign of any toxic effect and the swelling has diminished in size.

In the Sudan, where the human skin is not greatly protected by clothes and the feet are not protected by boots, actinomycosis (mycetoma) easily gains entry after the prick of a thorn or through the abraded skin with some foreign body. In England, owing to the protection of clothes the alimentary tract is the usual mode of entry.

### Case of *Filaria* without Clinical Symptoms: Embryos of *F. perstans* in the Blood.

By W. BROUGHTON-ALCOCK, M.B.

PATIENT, a surveyor, who after eighteen months' residence in the Cameroons, Calabar, and forest areas in Northern Nigeria, returned to England in July, 1921. He suspected he had had malaria; a blood film taken showed no evidence of this, but an eosinophilia of 8 per cent. was noted. Opportunity to trace the cause was not afforded until his return, after a further eighteen months in Northern Nigeria, three months ago. The eosinophilia now varied between 12 per cent. and 28 per cent. Repeated examinations of many large drops of blood taken at various hours of day and night showed rare embryos of *Filaria perstans* only. Urine normal; faeces, *Entamoeba coli* cysts on two of four examinations. No clinical symptoms and no Calabar swelling. Can the *Filaria perstans* independently give rise to marked eosinophilia or is there a latent infection with *Filaria loa* to which he has been exposed? Bassett-Smith raised this question in 1916. It remains unanswered. Sir Patrick Manson early pointed out the long period of time noted between exposure to infection with *Filaria loa* and appearance of embryos in the blood. In some recently published cases a much less interval of time has been observed and in one instance an interval of only six months is cited.

### DISCUSSION.

Dr. G. C. Low, in commenting on the case, thought the eosinophilia described by Dr. Broughton-Alcock was much more likely to be due to a coincident infection with *Filaria loa*. Eosinophilia was very slightly if at all increased in *Filaria perstans* infections. The fact of there being no *Filaria loa* in the blood meant nothing, as these parasites often did not appear till very late in the infection. A large eosinophilia was very characteristic of *Filaria loa* infections.

Dr. P. MANSON-BAHR said he differed from the last speaker in the interpretation to be placed on the eosinophilia count in this case. The increased eosinophilia did not, he thought, indicate the presence of *Filaria loa* in addition to *Filaria perstans*.

**Researches on Blackwater Fever in Southern Rhodesia during the Years 1922 and 1923.**

By J. GORDON THOMSON, M.B.

(*Director of Protozoology, London School of Tropical Medicine, Blackwater Fever Commission, Southern Rhodesia*).

INTRODUCTION.

I have to apologize for attempting to deal with a subject of such magnitude in a comparatively short paper. I feel I cannot possibly discuss the condition and the research work in full detail and so do justice to a disease of such importance. During the two expeditions I made to Rhodesia I have accumulated an immense amount of statistical and pathological material and it will require a long period to study and correlate all the facts. This I intend to do in a forthcoming monograph. While, therefore, I cannot hope to deal with this subject in all its aspects, I propose to place before you some observations of interest.

I acknowledge with gratitude the great kindness shown to me by the Administration of Southern Rhodesia, by Sir Drummond Chaplin, Dr. Fleming, C.M.G., the Medical Director, and by all the medical men in the territory.

HISTORICAL.

The history of blackwater has been carefully investigated and described by various authors, and no research work on this subject could be complete without it, because a study of the earliest recognition of the condition throws a peculiar light on one of the factors necessary for its production. Although malaria, in its different clinical forms, was known and described by Hippocrates, no recognizable description of hæmoglobinuric fever exists, so far as we know, till the middle of the nineteenth century. This apparently late appearance of a disease exhibiting such serious symptoms has been a matter of wonder to medical men, and has led to the erroneous belief that it was a disease of recent origin and also a condition *sui generis*.

The late recognition of such a well marked condition in Europe was due to several factors, the most important being the exploration and development of the tropics by non-immune white races, and finally the settlement in these areas for the development of trade and agriculture. Attention, however, having been drawn to the clinical aspects, the disease was soon described in widely spread areas.

The French physicians Lebeau (1851) [1], Le Roy de Mérencourt (1853) [2], and Daullé (1857) [3] reported blackwater from the north-west of Madagascar. Varetas [4] and Konsola (1858) noted it in Greece as occurring after quinine administration. Early mention of the disease also occurs in the American literature, the first account being given by Cummings in 1859.

Although this disease was recognized and described by the French, Greeks and Americans in various parts of the world, it is a striking fact that no distinct mention of it is made in the English literature until comparatively recent times. Easmon (1884) [6], Prout (1891) [7], Manson (1892) [9, 10] and Crosse (1892) [8] all wrote on this condition and this, combined with the discovery of Ross, in 1898, of the transmission of malaria by mosquitoes, seems to have stimulated English workers throughout the world in the study of

tropical diseases. The earliest French and American observers seem to have connected the disease with tropical malaria, but Manson and Sambon supported the theory that it was a disease *sui generis*.

Since then, however, the great majority of observers agree that blackwater fever is the result of severe malignant infection especially associated with the tropics, and in those areas where the endemic index is high (Daniels (1900) [15], Stephens and Christophers (1900) [14], Christophers and Bentley (1908) [18], Deeks and James (1911) [22], &c.). The more recent literature on the subject is still more convincing, and every day new evidence accumulates to implicate malaria as the primary factor necessary before blackwater fever appears. Apart from this, in my experience it is the almost universal opinion of all medical men who have spent years in the endemic areas that blackwater is an end-symptom of chronic, neglected, and improperly treated tropical malaria and this is the unanimous medical opinion in Rhodesia. The history of this disease, therefore, emphasizes two factors necessary for the production of blackwater fever, viz.: malaria, and the introduction of non-immune races into those areas where the malarial endemic index is high.

#### THE CO-RELATION OF MALARIA AND BLACKWATER FEVER.

There are three factors which can be investigated in considering the close association of these two conditions; viz.: (1) The seasonal incidence, (2) the association of malarial parasites with the attack, and lastly (3) the clinical history of malaria. In considering these three points there are other important features to be considered, such as the species of parasite concerned, the infantile endemic index, the length of residence of non-immunes in the endemic area, the mode of living and the method employed in the treatment of malaria, and, finally, the relationship of all these factors in the determination of the geographical distribution.

##### (1) *The Co-relation in the Seasonal Incidence of Malaria and Blackwater Fever.*

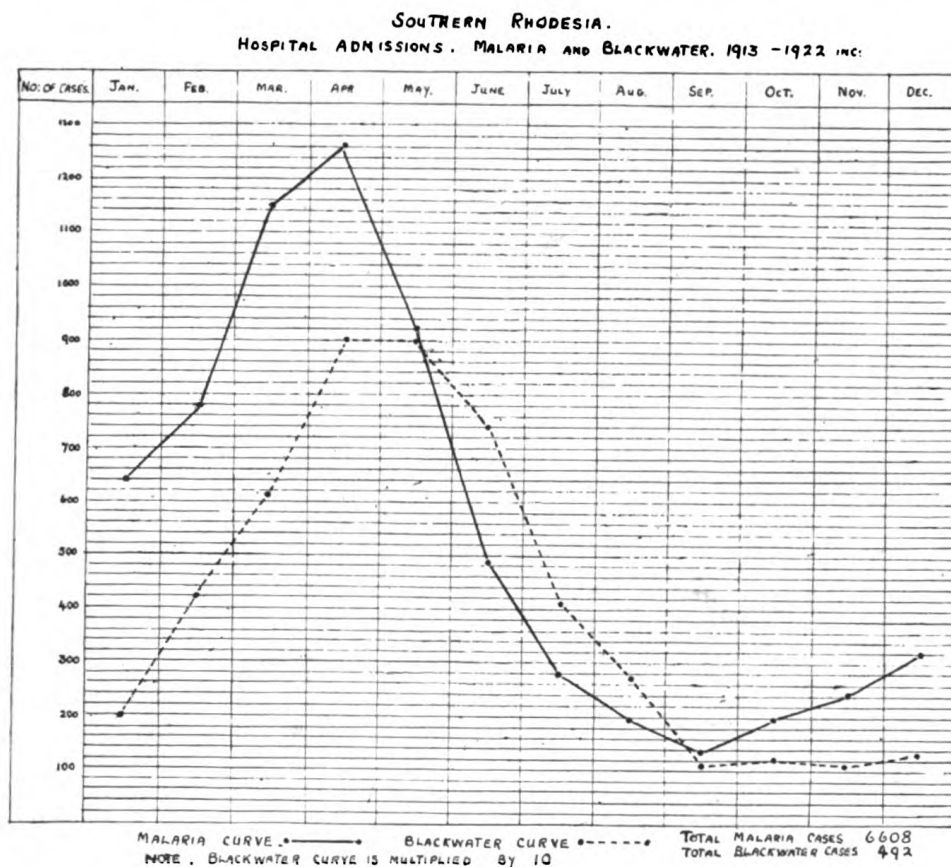
That the seasonal incidence differs is one of the arguments against the relationship. This can only be investigated thoroughly by the study of numerous cases over a period of years; and statistics of the number of cases of malaria and blackwater occurring each month must be kept if reliable results are to be obtained. The collection of such figures over the large areas in the tropics is extremely difficult. The most reliable figures are those supplied by hospitals and laboratories but these cannot be claimed as giving anything approaching the actual number of cases occurring in a given area unless notification is practised. The only known method by which more complete records of cases of blackwater fever could be accumulated would be by making a compulsory notification in various parts of the world. Such notification could be made in a form as suggested by Stephens (1914).

In Southern Rhodesia this year, Dr. Fleming sent out a form modified according to Stephens' suggestion and, further, every medical man in the territory was instructed to notify any case of blackwater fever by wire. On receipt of a telegram, if the case was at all accessible, I paid a personal visit to the district. If the distance was too great the medical man in charge was instructed to take blood films and send them direct to the Research Laboratory at Salisbury.

If the form sent out is too complicated the results obtained from filling it up are most unsatisfactory and in a great majority of cases absolutely unreliable. I came to the conclusion that the form as sent out by me in Rhodesia left much

to be desired, but on the other hand a considerable amount of information was acquired from the records supplied.

Deeks and James (1911) [22] showed a comparison between the epidemiology of malaria with that of blackwater fever in the Panama Canal zone. From July, 1904, to September, 1910, there were 40,928 cases admitted to Ancon Hospital diagnosed as malaria and in the same period 232 patients were admitted who were suffering either from hæmoglobinuric fever or its after effects, or who contracted the disease after admission. These observers used charts and histories of 230 cases of blackwater in their investigation and came to the following conclusions, viz., that the incidence of malaria in the



Panama Canal zone is lowest during the dry season and increases to the middle third of the wet season, when it remains stationary. The hæmoglobinuric fever rate is lowest at the beginning of the wet season and highest at the end. Both diseases have approximately the same period of greatest intensity towards the end of the wet season. Stephens (1915) [34] made a critical examination of these findings and also examined figures from the West Coast of Africa and concluded that the factors were in favour of a seasonal incidence.

In my researches in Southern Rhodesia the relationship between malaria and blackwater as regards seasons is very definite. This definite result is obtained because in Southern Rhodesia we have such sharply defined climatic



conditions. In order to illustrate this relationship I have prepared a chart (see p. 49) showing the seasonal relationship between malaria and blackwater fever over a period of ten years, and I might add that this relationship each year remains constant. I publish below a table giving the exact numbers of malaria and blackwater fever treated in hospitals each month during the years 1913 to 1923 inclusive.

TABLE I.—MALARIA.

## Admissions to Hospitals in Southern Rhodesia.

| Year  | Jan. | Feb. | Mar.  | Apr.  | May | June | July | Aug. | Sept. | Oct. | Nov. | Dec. | Total |
|-------|------|------|-------|-------|-----|------|------|------|-------|------|------|------|-------|
| 1913  | 78   | 113  | 124   | 120   | 136 | 74   | 45   | 17   | 18    | 19   | 15   | 20   | 779   |
| 1914  | 18   | 56   | 123   | 128   | 89  | 97   | 25   | 12   | 18    | 23   | 22   | 26   | 577   |
| 1915  | 83   | 127  | 176   | 191   | 75  | 39   | 19   | 21   | 18    | 11   | 15   | 28   | 803   |
| 1916  | 34   | 46   | 74    | 56    | 38  | 26   | 22   | 11   | 15    | 19   | 34   | 35   | 410   |
| 1917  | 68   | 72   | 108   | 98    | 83  | 57   | 30   | 28   | 10    | 20   | 20   | 24   | 613   |
| 1918  | 43   | 41   | 107   | 91    | 85  | 41   | 27   | 19   | 11    | 23   | 10   | 25   | 523   |
| 1919  | 56   | 60   | 123   | 216   | 120 | 53   | 25   | 21   | 12    | 23   | 30   | 44   | 783   |
| 1920  | 79   | 96   | 133   | 144   | 116 | 57   | 34   | 26   | 17    | 23   | 33   | 38   | 796   |
| 1921  | 96   | 111  | 116   | 148   | 129 | 77   | 29   | 20   | 10    | 19   | 36   | 33   | 824   |
| 1922  | 87   | 56   | 69    | 78    | 49  | 17   | 23   | 21   | 10    | 18   | 28   | 44   | 500   |
| Total | 642  | 778  | 1,153 | 1,265 | 920 | 478  | 279  | 196  | 139   | 198  | 243  | 317  | 6,608 |

## BLACKWATER.

## Admissions to Hospitals in Southern Rhodesia.

| Year  | Jan. | Feb. | Mar. | Apr. | May | June | July | Aug. | Sept. | Oct. | Nov. | Dec. | Total |
|-------|------|------|------|------|-----|------|------|------|-------|------|------|------|-------|
| 1913  | 1    | 4    | 3    | 5    | 9   | 17   | 7    | 4    | 1     | 3    | 1    | 2    | 57    |
| 1914  | 4    | 3    | 4    | 2    | 15  | 8    | 4    | 3    | 3     | 8    | 2    | 2    | 53    |
| 1915  | 1    | 2    | 5    | 16   | 14  | 7    | 4    | 6    | 0     | 1    | 1    | 3    | 60    |
| 1916  | 4    | 4    | 6    | 8    | 5   | 2    | 2    | 0    | 0     | 1    | 1    | 1    | 34    |
| 1917  | 1    | 4    | 14   | 10   | 4   | 3    | 7    | 2    | 1     | 0    | 2    | 0    | 48    |
| 1918  | 3    | 3    | 2    | 8    | 6   | 5    | 8    | 1    | 0     | 0    | 1    | 0    | 32    |
| 1919  | 0    | 2    | 5    | 10   | 7   | 5    | 1    | 2    | 1     | 1    | 1    | 1    | 36    |
| 1920  | 0    | 7    | 10   | 12   | 17  | 11   | 7    | 1    | 1     | 2    | 0    | 2    | 70    |
| 1921  | 1    | 6    | 7    | 8    | 8   | 13   | 3    | 3    | 2     | 1    | 1    | 0    | 53    |
| 1922  | 5    | 7    | 5    | 11   | 5   | 3    | 3    | 5    | 2     | 0    | 1    | 2    | 49    |
| Total | 20   | 42   | 61   | 90   | 90  | 74   | 41   | 27   | 11    | 12   | 11   | 13   | 492   |

In Southern Rhodesia, therefore, we have a definite seasonal incidence of the two diseases and from this we may conclude that either malaria and blackwater are one and the same disease or that they are transmitted by the same biting insects or insects with similar habits.

Dudgeon (1920) [44], in his investigations into blackwater fever amongst the troops in Macedonia, does not show any definite seasonal relationship to malaria as would be expected from the above findings. In Macedonia the disease occurred at the period in each year when malignant tertian was at its lowest limit, and, further, the period of lowest death-rate from malaria was the period of highest blackwater fever incidence. We must remember, however, that factors to be considered in war conditions are in no way comparable to those met with amongst a community permanently settled in the various parts of Rhodesia and elsewhere in the tropics.

(2) *The Association of Malarial Parasites with Blackwater Fever.*

The most important point to remember in reviewing this is that it is a well established fact that if no parasites are found in a blood film of a case clinically malaria it does not mean the patient is free from malarial parasites. This cannot have been realized by those who put forward the argument that blackwater fever cannot be accepted as malarial in origin owing to the large number of cases of the disease which show no evidence of malarial parasites in the blood. Stephens and Christophers drew attention to the absence of

parasites from the peripheral blood in cases afterwards definitely proved to be malaria, and I may state that this is particularly the case in infections with malignant tertian. In Rhodesia, although a first examination of a film of malaria often shows parasites, yet, on the other hand, I encountered several cases with raised temperatures and no quinine taken, in which I was able to demonstrate parasites only by examination of a film on the second or third day. To those who are acquainted with the asexual life cycle of the malignant tertian parasite this is easily explained. Further, it has often been stated by those opposed to the malarial causation of blackwater that the infection with parasites as shown by the examination of a blood film could not explain the severity of the symptoms. Here, again, I might simply point out that the examination of a blood film in pernicious malaria is no measure of the infection, for at the time such a film was taken practically all the parasites might be in the internal organs.

Deeks and James (1911) [22] state that in the Panama out of 40,928 cases of diagnosed malaria only 23,410 showed parasites at the time of examination. Many of these cases probably took a dose of quinine before the blood examinations, and so still further reduced the chances of a positive finding. Another factor not fully realized is that examination of a blood film does not exclude an infection with two or three species of parasites.

Acton, Rennie, Curjel and Dewey (1921) [46], in 102 cases of malaria in which malignant tertian had been found and in an area where re-infection could be excluded, found that sixty-four of these suffered at another date from a benign tertian infection. I have seen many cases of a similar character, and recently Dr. Newham showed me a film in which all three species of parasites were present at the same time. That mixed infections occur and are perhaps commoner, in heavily infected malarial areas, than is commonly realized, helps to explain the occurrence of benign tertian and quartan parasites associated with an attack of blackwater fever.

Deeks and James (1911) [22] in discussing the species of parasite found in 230 cases of hæmoglobinuric fever found that on sixty-eight occasions this was associated with *Plasmodium falciparum*. The percentages of parasite findings in cases were as follows: 76·4 per cent. were malignant tertian and 23·6 per cent. were benign tertian. Referring, however, to the past histories of these cases, the same observers found that 87·2 per cent. had suffered from malignant tertian.

Stephens (1913) [30] made a very valuable and exhaustive research into the literature on this subject, which incriminates the malignant tertian parasite, but the figures of Lovelace in 178 cases of blackwater fever give the following percentages for the various species of parasite found: malignant tertian, 48·9 per cent.; benign tertian, 41·6 per cent.; mixed subtertian and benign tertian, 9·6 per cent.

These parasitic findings of Lovelace are in sharp contrast to the findings of Deeks and James, and the findings of other observers, and are explainable by the assumption that he was dealing with double infections.

Quartan parasites have also been associated with an attack of hæmoglobinuric fever, e.g., by Stephens (1915) [34].

A study of the literature certainly points to the fact that the species of parasite most concerned with blackwater fever is the malignant tertian, and I would point out that possibly this parasite alone is responsible for the condition, and that in those cases where benign tertian and quartan parasites have been found, they acted only as the excitant of the attack without

being the primary causal factor in the production of the blackwater fever condition.

Stephens (1915) [34] made an examination of the literature showing the parasitic findings on the day before the onset, the day of the onset, and the day after. These figures clearly demonstrate that, as a rule, the parasites tend to disappear from the peripheral blood as the hæmoglobinuria proceeds. The percentage of positive findings in cases of blackwater as given by Stephens was as follows: before the onset 73 per cent. were positive, on the day of the onset 47·5 per cent. were positive, and on the day after only 23 per cent. showed parasites.

In 1915 Hatori, in twenty-six cases examined before the onset of hæmoglobinuria, found 96·3 per cent. were positive and Nakayawa found 85 per cent. positive before the onset. In Rhodesia I found in twenty-three cases examined before the onset *Plasmodium falciparum* in every case (i.e., 100 per cent.). In my experience in Rhodesia the parasites and also the pigment rapidly disappeared as a rule from the peripheral blood with the onset of hæmoglobinuria. In only one case in my series did the parasites remain very numerous throughout the onset, and after death they were found in the internal organs. Dudgeon, in Macedonia, reported similar cases.

In six post-mortem examinations I found crescents in the internal organs of three, and all showed pigment, although sometimes this was by no means abundant. In one case there was a remarkable absence of pigment, and only after a prolonged search was one crescent found. In five cases in which no parasites were found during the attack of hæmoglobinuric fever I found a parasitic relapse varying from five days as a minimum to fourteen days as a maximum after the cessation of hæmoglobinuric symptoms. In two of these cases there was certainly no chance of re-infection. An attack of blackwater fever therefore does not necessarily rid the system of parasites although they may be so diminished as to defy detection.

### (3) *The Clinical History of Malaria.*

In 150 cases of blackwater fever in Rhodesia I obtained a history of repeated attacks of malaria and irregular taking of quinine. The predominating type of malaria in Rhodesia is the malignant tertian, causing bilious remittent fever. The characteristics of this infection are a gradually progressing anæmia with a remarkable absence of acute symptoms. The patient often never realizes he is suffering from malaria in spite of the fact that an examination of the blood shows parasites. A history of biliousness is common, and often the patients declare they cannot take quinine because it upsets them. A characteristic feature is that the patient continues at his or her work feeling off colour, with an occasional attack ushered in by a feeling of chilliness, the so-called "touch of fever," for which they take a few doses of quinine, which is quickly abandoned when the acute symptoms disappear. Thus for months the patient goes on gradually getting more and more anæmic, and the skin and sclerotics develop a characteristic lemon tint, so well known to medical men in these areas as the forerunner of an attack of blackwater. All of these cases live at least for a certain time under conditions which certainly expose them to nightly infection from mosquitoes. They are infected and re-infected, and as I found that over 95 per cent. of the positive blood films are *Plasmodium falciparum* it is certain that these patients are living under such conditions as to favour repeated infections each season with this organism.

All the evidence obtained by me strengthened the deduction already made by many observers, namely, that a person must live in an area where tropical

malaria is endemic and expose himself to the chances of intense and repeated infections for several months before he develops blackwater fever. I have not time to discuss this further, but this means an incubation period which more or less rules out another causal parasite. In my cases the shortest period of residence was five months in Rhodesia in a patient who had come direct from England.

Béranger-Feraud (1874) in 185 cases of blackwater found only one case with a residence under three months. Stephens (1913) [30] gave the distribution of 1,050 cases of blackwater, and his percentages were as follows:—

|              |     | 0-6 mths. |     | 1st year  |     | 2nd year  |     | 3rd year  |     | 4th year  |     | 5th year  |     | Later     |
|--------------|-----|-----------|-----|-----------|-----|-----------|-----|-----------|-----|-----------|-----|-----------|-----|-----------|
|              |     | Per cent. |     | Per cent. |     | Per cent. |     | Per cent. |     | Per cent. |     | Per cent. |     | Per cent. |
| Percentage   | ... | 7.2       | ... | 21.3      | ... | 33.0      | ... | 24.9      | ... | 9.7       | ... | 4         | ... | 7.1       |
| No. of cases | ... | 76        | ... | 22        | ... | 346       | ... | 261       | ... | 102       | ... | 42        | ... | 75        |

Stephens concludes from these figures that blackwater fever shows itself in the majority of cases when the patient has been subjected to repeated malarial infection lasting over a certain time.

In my researches I found it necessary to consider several factors in residents and one of the most important of these was not to find out how long the patient had been in Rhodesia but how long in certain areas. In larger towns such as Salisbury and Bulawayo, &c., the chances of developing blackwater are practically *nil*.

#### THE GEOGRAPHICAL DISTRIBUTION OF BLACKWATER FEVER AND MALARIA.

I cannot discuss this as fully as I should wish owing to lack of time, but we require to consider several factors. Of these I consider the most important are (1) that malignant tertian must predominate, (2) that the endemic index must be high as evidenced by the figures in the native children, (3) the presence of a non-immune population living under conditions where anophelism is intense and where prophylactic measures are absent or imperfect. It is the absence of certain of these factors which has given rise to the idea that certain islands and areas, such as Ceylon and various parts of India, are intensely malarious and yet blackwater may be absent or rare. In Ceylon, for example, I found from the figures of S. P. James and Gunasekara (1913) [28], that quartan formed 71 per cent. of the infections, simple tertian 18 per cent., and malignant tertian 10 per cent., and P. H. Bahr (1913) [29], for another area, gives quartan as also predominating, and malignant tertian as extremely low.

The geographical distribution of blackwater fever is certainly associated with those areas where tropical malaria (i.e., *Plasmodium falciparum*) predominates, and where the endemic index is high. The tropics of Africa supply such conditions. In order to emphasize this fact I give the laboratory findings from the West Coast of Africa and from the East Coast of Africa, and my own findings in Rhodesia.

#### WEST COAST OF AFRICA.

Laboratory findings in positive blood films; table, after Macfie and Ingram (1917).

| Locality     | Observer              | No. of cases | Percentages of malarial parasite |        |         |
|--------------|-----------------------|--------------|----------------------------------|--------|---------|
|              |                       |              | Malignant                        | Benign | Quartan |
| Sierra Leone | Statham               | 676          | 98.5                             | ?      | ?       |
| Sierra Leone | Butler                | 201          | 95.0                             | 0.5    | 4.5     |
| Sierra Leone | Dalziel and Johnson   | 221          | 77.7                             | 0.4    | 21.9    |
| Sekondi      | Coghill and Hanschell | 259          | 56.8                             | 18.4   | 25.9    |
| Sekondi      | Coghill and Hanschell | 211          | 72.5                             | 11.1   | 16.4    |
| Accra        | Macfie and Ingram     | 250          | 87.6                             | 5.2    | 7.2     |

## LABORATORY REPORTS ON MALARIA.

Positive blood films. Kenya Colony 1911 to 1921 (inclusive).

| Year           | 1911        | 1912        | 1913        | 1914        | 1915        | 1916        | 1917        | 1918        | 1919        | 1920        | 1921   |
|----------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|--------|
| Sub-tertian    | ... 238 ... | ... 209 ... | ... 174 ... | ... 218 ... | ... 814 ... | ... 655 ... | ... 881 ... | ... 435 ... | ... 540 ... | ... 224 ... | ... 83 |
| Benign tertian | ... 25 ...  | ... 19 ...  | ... 12 ...  | ... 10 ...  | ... 61 ...  | ... 176 ... | ... 607 ... | ... 308 ... | ... 90 ...  | ... 44 ...  | ... 19 |
| Quartan        | ... 18 ...  | ... 13 ...  | ... 4 ...   | ... 10 ...  | ... 22 ...  | ... 27 ...  | ... 11 ...  | ... 7 ...   | ... 12 ...  | ... 10 ...  | ... 4  |

In both areas, in which *Plasmodium falciparum* represents the predominating species of parasite, blackwater fever is endemic. The findings of the various authors vary in the amount of quartan reported, and as pointed out by Macfie and Ingram (1917) [35], probably the personal factor has entered into this as it is well known that malignant tertian asexual forms often closely simulate quartan band forms. The figures given from East Africa are of great interest, as it will be noted that during the war, owing to the introduction of Indian troops, the benign tertian rose in numbers during the years 1916, 1917, and 1918; and with the cessation of these conditions the benign tertian incidence has fallen to its original pre-war position. These figures are interesting from two points of view, in that they show a temporary alteration in the types of malaria, and also that evidently in East Africa the factors are favourable for the transmission of *Plasmodium falciparum*. In Macedonia during the war Dudgeon's figures showed that from March to May, 98 per cent. of cases were *Plasmodium vivax* infections and that about the end of November there were about equal numbers of *Plasmodium vivax* and *Plasmodium falciparum*. The figures obtained during the war vary from those given by Cardamatis for Macedonia for a period of eighteen years during which *Plasmodium falciparum* in the autumn formed 73 per cent. of infections, and in the summer 55 per cent. of the infections. The figures from India, where obtainable, show that blackwater fever exists in those areas which closely approximate to tropical African conditions (*vide* Stephens and Christophers).

I have quoted the above figures to illustrate that when it is stated certain areas are highly malarious and yet practically free from blackwater several factors must be considered. In Rhodesia there is a remarkable predominance of the malignant tertian parasite, as seen from the laboratory figures obtained by me during 1922 and 1923.

## MALARIA IN RHODESIA.

| Year | Percentage of<br>malignant tertian |      | Percentage of<br>benign tertian |     | Percentage of<br>quartan |     |
|------|------------------------------------|------|---------------------------------|-----|--------------------------|-----|
| 1922 | ...                                | 98.3 | ...                             | 5.0 | ...                      | 1.7 |
| 1923 | ...                                | 96.0 | ...                             | 3.0 | ...                      | 1.0 |

Here, again, during the war benign tertian was introduced but failed to establish itself. Further, in the areas where blackwater exists the endemic index is enormously high both amongst the native children and the white children. I found amongst the native children between two and seven years of age at a single examination that 38.8 per cent. were infected with parasites and that 42.8 per cent. of these positive cases were carriers of crescents. These figures in native children form a sharp contrast to the findings amongst the adult natives where only 2.6 per cent. showed infection. As a further example of the intensity of malaria in certain areas I take four districts and give a table showing the endemic index as estimated by examination of the spleen in European children by Dr. Gatchell.

| SOUTHERN RHODESIA.                           |     |     |                 |     |                  |     |                    |                     |
|----------------------------------------------|-----|-----|-----------------|-----|------------------|-----|--------------------|---------------------|
| Splenic index amongst white school children. |     |     |                 |     |                  |     |                    |                     |
| Area                                         |     |     | No. of children |     | Malarial history |     | Blackwater attacks | Splenic enlargement |
| Shamva                                       | ... | ... | 70              | ... | 68               | ... | 7                  | 58                  |
| Bendura                                      | ... | ... | 25              | ... | 25               | ... | 1                  | 20                  |
| Sinoia                                       | ... | ... | 21              | ... | 20               | ... | 2                  | 11                  |
| Maryland                                     | ... | ... | 8               | ... | 8                | ... | 2                  | 8                   |

#### THE MODE OF LIVING OF THE NON-IMMUNE WHITES IN SOUTHERN RHODESIA AS ASSOCIATED WITH BLACKWATER FEVER.

During the year 1922 I visited every blackwater area in Southern Rhodesia and I paid personal visits to the so-called "blackwater fever houses" and their surroundings. In every instance the condition of living was absolutely favourable to the contraction of intense malaria. I will place upon the lantern examples of such houses. These were situated near breeding places of *Anopheles costalis* and were in practically every instance unprotected by screening. In many cases no attempts were made to use nets over the beds. Quinine was either used intermittently, despised, or feared as a poison, but curiously enough in spite of this it was usually taken as a last resource. Dr. Fleming and all the medical men in Southern Rhodesia have attempted to instruct the population in antimalarial measures and the proper method of using quinine, but there are still many who disregard this advice, and follow their own free will, which leads to final disaster. The efforts of the medical section in Southern Rhodesia, however, has resulted in great improvement in the housing and method of living of a large proportion of the population, and this has reduced blackwater amongst the more intelligent classes to a minimum. I was most impressed by the utter disregard for malaria shown by many settlers, and was most depressed at seeing the disrepute into which the most valuable remedy, viz., quinine, had fallen through its being misused and abused. Malignant tertian malaria in Rhodesia is most amenable to quinine, and regular administration of the drug in proper doses results in rapid improvement in the patients. The great error lies in the fact that patients stop using it on the cessation of acute symptoms, and in most cases after the cessation of treatment they are quickly re-infected. It has been abundantly proved that anti-malarial measures reduce blackwater. If, therefore, breeding places of anophelines were destroyed or avoided, houses well built and thoroughly screened, efficient mosquito nets used, and malaria properly treated in the first instance by quinine, I am convinced that blackwater fever cases would be reduced to a negligible quantity. Several houses were abandoned as dangerous to live in, and in 1923 I saw three cases in one house.

#### ATTEMPTS TO DISCOVER A SPECIFIC CAUSAL ORGANISM.

The earliest observers regarded hæmoglobinuric fever as a symptom occurring in severe malaria but numerous attempts have been made to find another causal organism. Sambon (1898) [12, 13] believed that blackwater was a disease apart from malaria, and Manson (1892 and 1893) [9,10] was inclined to support the same view. The similarity between blackwater fever in man and redwater in cattle led to the belief that an organism related to the genus *Babesia* might be responsible for the production of hæmoglobinuric fever. No evidence, however, has been produced to show that man is ever infected with *Babesia*. Recently, Wright (1920) illustrated babesia-like organisms from the blood of blackwater

fever cases, but these were simply forms of *Plasmodium falciparum*. This year, in Rhodesia, I continually encountered pear-shaped malignant tertian parasites which, by cultural methods, were easily proved to be simply morphological variations of the asexual forms of *Plasmodium falciparum*. Various bacteria have been suggested as possible causal organisms, e.g., *Bacillus coli communis* (Yersin 1895), *Bacillus megatherium* (Collet 1904) [17], and *Bacillus perfringens* (Fairley and Dew 1920) [42], but all of these may be simply considered as accidental organisms associated with the conditions.

In 1912 Leishman [25] drew attention to certain bodies in the larger endothelial cells which he thought might be parasites of blackwater fever, and brought forward the Chlamydozoal hypothesis, but these bodies were shown to be non-organismal in origin. Coles (1913) [26] described bodies few in number in the peripheral blood but according to Low and Wenyon these were artefacts.

Other organisms have been associated with blackwater fever and very probably have acted as exciting agents without being actually the causal organism. Ashburn, Vedder and Gentry (1912) described relapsing fever spirochætes in one case and Cook (1913) [31] found the same spirochætes associated with four cases of hæmoglobinuric fever.

Schüffner (1918) [36] found numerous *Leptospira* in a case and these were morphologically identical with *Leptospira icterohæmorrhagiæ*. These he named *Leptospira icterohæmoglobinuriæ*. *Plasmodium falciparum* was also found. In 1920 Noc [41] and Esquier described spirochætes in the liver of a fatal case, and also *Plasmodium falciparum*.

Napier (1913) suggested syphilis as the predisposing cause. Yorke (1909), Arkwright and Lepper (1918) [37], and Dudgeon (1920) [44] however, showed that this condition is not the causal factor of the disease. Spirochætes have also been suspected as the causal organism of blackwater fever and more attention has been devoted to this since the discovery of *Leptospira icterohæmorrhagiæ* and *Leptospira icteroides*. Blanchard and Lefrou (1922) [48], using special technique, announced the discovery of spirochætes associated with blackwater fever which they have named *Spirochæta biliohæmoglobinuriæ*. J. G. Thomson (1923) attempted to confirm their observations in fifteen cases in Rhodesia without success, using the technique described by Blanchard and Lefrou. Thomson found pseudo-spirochætes by dark-ground illumination or staining, and he emphasized the fact that these must not be mistaken for true spirochætes. Later, Low and Duncan (1923) in one case also failed to demonstrate any spirochætes by the same technique. Blanchard and Lefrou (1924) still uphold the view that the organisms seen by them were true spirochætes, while Knowles and Das Gupta (1924) [53], working in India, encountered pseudo-spirochætes in dengue and drew attention to the fact that they can be mistaken for true organisms. Dr. Connal, Director of the Laboratory at Lagos (1923), in a private communication, now informs me that he can only find pseudo-spirochætes in his cases. I am therefore justified in saying that so far no one has brought forward sufficient evidence that a spirochæte is the causal organism of blackwater fever, although it is possible that syphilis, relapsing fever, or acute infective jaundice might, in the presence of malaria, act as an exciting cause.

Dudgeon (1920) [44], in Macedonia, and I myself in Rhodesia, failed to demonstrate spirochætes in the serum, the urine, or the tissues. It might be asked, "Is the causal organism an ultra-microscopic one and a filter passer?" Here again no evidence has been collected. I inoculated fifteen guinea-pigs

from fifteen separate cases of blackwater in Rhodesia and no symptoms appeared, and further, sub-inoculations from these were also negative. This, of course, is only negative evidence. Dudgeon also obtained negative results from injecting the urine of hæmoglobinuric fever into animals. A more interesting experiment is that of Blacklock (1923) [51] who injected the blood of a severe case of blackwater into a healthy European with negative results.

#### A STUDY OF THE MALIGNANT TERTIAN PARASITES AS FOUND ASSOCIATED WITH BLACKWATER FEVER IN SOUTHERN RHODESIA.

While it has been usual to regard only one species of parasite in benign tertian and one in quartan, ever since the study of the parasite commenced, difficulties have arisen regarding the malignant tertian. Grassi and Feletti described three species of parasite associated with the latter condition, viz., *Hæmamoeba præcox* (or pigmented quotidian), *Hæmamoeba immaculatum* (non-pigmented quotidian) and *Laverania malarie* (the parasite associated with crescents). Since then, however, study of the morphology of the malignant tertian has shown that there is not enough evidence to form these three species, and it has generally become accepted that there is only one species, viz., *Plasmodium falciparum* (syn. *Laverania malarie*). Stephens (1914), however, described peculiar "tenue" forms in large numbers in a case from India, and more recently Sinton (1922) [47], from the same area as Stephens, described a similar case.

Sinton further showed in his case that when examined shortly after schizogony the parasites showed the typical rings usually associated with malignant tertian, but from the twenty-sixth to the thirty-second hour after schizogony the "tenue" forms appear in large numbers. Sinton, in his description of this parasite, shows that it varies enormously in size according to the age in the peripheral blood, and also shows great morphological variations. The youngest rings are  $1.5\ \mu$  to  $2.5\ \mu$  in diameter. In ten to twelve hours they measure  $2.5\ \mu$  to  $3\ \mu$ , and later  $3\ \mu$  to  $3.5\ \mu$ , and malignant stippling appears. The youngest rings show the typical two dots of chromatin, and „accolé" forms are also seen. Later, large oval and tailed forms are seen and also banded forms. Craig (1921) [45] described two species of malignant tertian, viz., *Plasmodium falciparum* with rings  $2$  to  $3\ \mu$  in diameter, and *Plasmodium falciparum quotidianum*, Craig, 1909, in which the youngest rings are only  $0.5\ \mu$  in diameter. We have thus various species of malignant tertian malaria described differing in size, morphology, and in the clinical symptoms produced, but it is doubtful if they are really true species or merely varieties of the same species. There is no doubt that malignant tertian varies enormously in its clinical manifestations, but these can be attributed to degrees of virulence and to variations in the resistance in the host infected. In Southern Rhodesia, therefore, I carefully studied the morphology of the malignant tertian parasites by taking films from cases shortly after schizogony until the asexual rings disappeared from the peripheral blood. I obtained cultures from eighteen cases, two of which afterwards developed blackwater fever. The merozoites were approximately  $0.75\ \mu$  long. The youngest rings in the peripheral blood are about  $1.5\ \mu$ , and these gradually increase in size to  $2.5\ \mu$ ,  $3\ \mu$ ,  $3.5\ \mu$  and even  $4\ \mu$  in diameter. It is extremely difficult to estimate for how long the rings remain as such in the peripheral blood, but from cultural observations it is about twenty to twenty-four hours. The time may be shortened or delayed. In the Rhodesian cases malignant stippling appears when the rings are  $3$  to  $3.5\ \mu$  in diameter, and at the same time the chemical reaction of the hæmoglobin or



stroma of the red cells changes, as can be seen by its staining reaction. This changed chemical characteristic of the red cell I consider of great importance in the production of blackwater fever. The infected cell stains a much darker pink colour in contrast to the surrounding non-infected corpuscles. In benign tertian the corpuscle enlarges and becomes paler owing to the loss of hæmoglobin, and in quartan malaria the red cell is practically filled with the schizont, no hæmoglobin being left. This peculiar alteration in the chemical composition of the red cell in malignant malaria was noted by Stephens and Christophers, Christophers and Bentley, Maurer, &c., and produces the so-called brassy colour of the corpuscle. In my cultural experiments in Rhodesia I again noted the massing of the parasites into clumps as soon as pigment commenced to collect. Maclay, in Macedonia (1922), observed that the clumping took place round mononuclears, and my cultures showed this in a remarkable manner, as illustrated in the figures shown on the epidiascope and in the preparations under the microscopes. The chief points of interest therefore distinguishing malignant tertian from benign and quartan lie in the fact that at a certain stage the parasites agglutinate and stick to the endothelial cells of the capillaries, causing them to be retained in the viscera, and producing the peculiar chemical alteration in the containing red cells.

I was quite unable to note any difference morphologically between the parasites in blackwater fever cases and those occurring commonly in malaria in Rhodesia. That the morphology of this parasite varies enormously can be seen by the figures shown on the lantern. In these we see the small typical rings, "accolé" forms, large rings, oval forms, tailed forms, "piroplasma" forms, band forms and occasional "tenue" forms. Pernicious stippling only appears as the rings enlarge and the corpuscles become brass-coloured.

#### EXPERIMENTS TO DEMONSTRATE THE PRESENCE OF AN AUTOLYSIN.

A considerable amount of discussion has taken place and opinions vary as to the true nature of the hæmolysis in hæmoglobinuric fever. In spite of the difficulties attending the demonstration of a true hæmoglobinæmia, owing to the fact that extravascular lysæmia may give rise to errors, it has been abundantly proved that hæmoglobinuria in blackwater fever is preceded by hæmoglobinæmia. Christophers and Bentley (1908) [18], Barratt and Yorke (1909) [19], Arkwright and Lepper (1918) [37], and Dudgeon (1920) [44], all agree that a true hæmoglobinæmia precedes the hæmoglobinuria. In order to demonstrate this condition it is necessary to examine the plasma at the right moment, as the hæmoglobin rapidly disappears from the serum during the onset of blackwater, and this would account for the negative results obtained by many observers. This year, in Rhodesia, I was able to satisfy myself that hæmoglobinæmia does occur, and I found evidence of this in nine cases, in one of which the plasma was a deep red (hæmoglobin = 3.2 per cent.). The hæmoglobin rapidly disappeared from the peripheral circulation.

Barratt and Yorke (1909) [19] experimentally established the fact that injections of hæmoglobin into a vein will produce hæmoglobinuria and even suppression, and further Yorke and Nauss (1911) [21] and Barratt and Yorke (1912) [23] showed that acute symptoms occurred comparable to those in blackwater fever, which are probably due to substances derived from the stromata of the red cells.

From the above experimental observations we may accept the following facts:—

- (1) That hæmoglobinuria is preceded by hæmoglobinæmia.
- (2) That the severe symptoms of blackwater fever are due to the presence of the hæmoglobin and the stromata of the damaged red cells.
- (3) That the suppression is mechanical, due to hæmoglobin in solution as well as to the débris of stromata of red cells.

The high temperature during an acute attack of blackwater fever is not entirely due to malaria. I had abundant evidence of this in Rhodesia. The temperature does not react to quinine and is undoubtedly due to the toxic action of the hæmoglobin and the products of the stromata of red cells. Probably also this severe toxic condition is assisted by the broken-up proteids of endothelial cells, &c. A. Plehn (1920) [43] supports the theory that the hæmolysis is due to the loss of epithelium and basement membrane of the kidney, thus bringing the red cells into contact with the urine, which provokes the hæmolysis. No evidence for this theory, however, exists, and it fails to explain the hæmoglobinæmia.

My own work in Rhodesia convinces me that the hæmolysis as seen in blackwater fever is due to the same phenomena as seen in a serum hæmolysis. The experimental work of the production of hæmoglobinuria by the injection of specific serum hæmolysins is well known (*vide* Christophers and Bentley, Dudgeon, &c.), but although a serum hæmolysin is injected which is capable *in vivo* of producing a lysæmia continuing over a period of several days, it has been found impossible to demonstrate the presence of such a hæmolysin *in vitro*. Browning (1913) [27] points out that the failure to obtain a positive hæmolytic effect *in vitro* does not afford conclusive evidence as to the absence of a hæmolytic mechanism. Further, Ameuille (1918) [38] and his colleagues have shown that an antihæmolysin may be present.

#### EXPERIMENTAL WORK ON HÆMOLYSIS IN RHODESIA.

With the help of Dr. Orpen I performed a number of experiments which confirmed the work of others in that we were unable to demonstrate autolysins in blackwater fever, using the Eason-Donath-Landsteiner technique. In blackwater fever, therefore, there is no autolysin detectable comparable to that which occurs in paroxysmal hæmoglobinuria.

We resorted to cultural methods of malignant tertian parasites. While doing this work I noted that if a culture was prepared from a patient during the rise of the temperature which corresponds to schizogony a distinct hæmolysis invariably takes place in the tube during incubation, and, further, that in such cases the parasites do not develop. Simpson (1912) [24], while working with benign tertian, also found evidence of a hæmolysis during schizogony. It is probable that this hæmolysis can only be detected at very definite periods and the power of demonstrating it in the blood during the progress of the disease quickly vanishes.

The fact that a hæmolysin may be detected at certain periods is encouraging, and more work on this subject is necessary, but it must be remembered that in such experiments fallacious results occur, and the technique must be conducted with great care and each experiment carefully controlled.

Dudgeon (1920) [44], by using alcohol and acetone, made tissue extracts from a few cases of blackwater fever, which he found to be powerfully hæmolytic. Here again, however, Browning points out that in using powerful extractive substances may be produced which have no existence as such in living tissues.

## SPECIFIC HÆMOLYSIS THEORY.

The evidence in favour of the production of a serum hæmolysis in blackwater fever is very strong, and the chief points in favour of this are the hæmoglobinæmia, the presence of shadow red cells, the sudden onset of symptoms. The inability to demonstrate the hæmolysis *in vitro* also shows a strong similarity to this condition. There is no evidence that the hæmolysis is of the nature of a cobra lecithid, in fact the evidence is to the contrary, as evidenced by the presence of shadow cells. Furthermore, in spite of the good results obtained by Boyé in using Calmette's antivenom in treatment of blackwater fever, I found the treatment of no use in Southern Rhodesia.

Before concluding, I should like to point out that blackwater fever is associated with malignant tertian malaria of several months' duration, and that while we are unable to conclude definitely that there are different species of this parasite, nevertheless it is evident that strains vary in their virulence. In Rhodesia repeated infections with this parasite produce a chronic condition of low fever, which is well named bilious remittent. The malignant tertian, as associated with blackwater fever in Rhodesia, after about twenty-four hours definitely alters the chemical composition of the corpuscle, and I suggest it is the continued breaking down of these corpuscles, so altered, which produces the antigen necessary to form a specific autolysin to them. We might regard these altered corpuscles as a foreign substance, and the gradual action of these over a period of months produces the autolysin. The same alteration in the corpuscles does not occur in benign tertian or quartan. This theory explains why the condition is associated with malignant tertian and, further, it would explain the action of quinine as a precipitating factor, owing to the fact that its administration liberates antigen, which, in the presence of a specific hæmolytic amboceptor and complement, produces a sudden onset of hæmolysis.

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## DISCUSSION.

Professor L. S. DUDGEON (President) said that he regretted Dr. Thomson had not referred to the pathology of the disease, more especially to the spirochætal theory, in which he had little belief. He did not consider that during the hæmoglobinæmia destruction of the parasites occurred from the same cause as the lysis of red cells, also he denied that they were difficult to find at this period in the peripheral blood. He had had no difficulty in finding parasites in the blood under such circumstances; and also if muscle was examined at the site of an intramuscular quinine injection, although tissue-necrosis was fully developed and lysis of red cells had occurred, yet the parasites could be seen apparently unaffected, or, at any rate, not destroyed. He thought that the term "immunity" in malaria was used much too freely, often without any intelligent meaning. Many patients referred to as "immune" were really suffering from the ill-effects of the disease and showed many of its worst manifestations.

Dr. G. C. Low said that Dr. Thomson's paper was of great interest, but most of the facts which he had brought forward were of course well known many years ago, and he (Dr. Low) was a little disappointed that no mention had been made of what actually

brought about the hæmolysis. More might also have been said about the geographical distribution of malaria and blackwater fever. There were areas in the tropics in which there was abundant malignant malaria of virulent type, and yet practically no blackwater fever. Why should this be so? It meant that there was some other factor behind the malaria which precipitated the actual hæmolysis. All were agreed that it was necessary that malaria must be undergone before one could succumb to blackwater fever, but the point for future investigators to elucidate was the unknown factor producing the hæmolysis. All, again, knew that chills and debilitating diseases were often responsible for the onset of blackwater fever, but yet these conditions did not produce blackwater fever in some of the intense malarial zones of the earth, such as Trinidad and British Guiana.

There were many other interesting problems yet to be solved in the study of this disease. The local indigenous native apparently escaped, but yet natives from India, Barbados, and other localities, when brought to Africa often developed blackwater fever and died of it.

Sir WILLIAM PROUT remarked that he also would have been glad to hear more of the hæmolytic properties in blackwater fever, as he felt that the association with intense malaria was now generally accepted. He, himself, had published a paper many years ago drawing attention to the relationship of malaria to the rainfall on the Gold Coast, but he thought that a seasonal relationship of blackwater fever could not be shown in the same way, as the number of cases were too small from which to draw correct statistical inferences.

He considered that there was a definite form of quinine hæmoglobinuria, and that the drug was one of the exciting causes of blackwater fever. He knew of several cases in which hæmoglobin could be produced by comparatively small doses of quinine, but he would emphasize the point that quinine only produced hæmoglobinuria where there had been a precedent and frequently a prolonged infection with malaria. In those cases a condition of the blood cells resulted which caused them to be readily hæmolysed. He had never known or heard of a case where quinine produced hæmoglobinuria in a healthy individual, even in large doses. He drew attention to the paradoxical fact that efficient quinine prophylaxis actually prevented the onset of blackwater fever by inhibiting the chronic infection which predisposed to that disease, and he thought that deductions about quinine prophylaxis could not be drawn from results based on war conditions—in Salonika, for example.

He suggested that investigations as to the causation of blackwater fever should be directed not to actual cases of that disease, as the condition had then been established and the mischief had been done—but to the cases of chronic malarial infection, especially with sub-tertian. Experiments with the blood in those cases might throw some light upon the causes of hæmolysis.

Dr. A. POWELL said that nearly all observers were agreed with Dr. Thomson that malaria, probably malignant malaria, was an essential factor in the causation of blackwater fever, but he had hoped for some light to be thrown on the other essential factors, for such there must be. Why was blackwater fever chiefly restricted to certain races? Why was it entirely confined to certain malarial districts while other regions with millions of cases of malignant malaria each year never had a single case of blackwater? In India, for instance, blackwater was confined to certain limited areas such as the Terai, the Duars, the jungles of Burma, and the foot of the Kasiya and Cachar Hills, where he had seen the first cases reported in India. In the Punjab, Scinde, the Deccan, and many other districts, there were millions of cases of malignant malaria, but none of blackwater.

In Bombay city alone he estimated there were at least a quarter of a million cases of malaria annually. During his stay in the city he had found malarial parasites in over 18,000 cases, about 40 per cent. of which were malignant. It might therefore be concluded that there were a hundred thousand cases of malignant malaria annually in the city, yet there was no indigenous blackwater in either European or Indian. During nineteen years in Bombay he saw six cases of blackwater, one from Burma, five from Africa, none indigenous.

Dr. A. O. P. REYNOLDS said he did not think Dr. Thomson had advanced any proof that blackwater fever was due to malaria. He suggested that the "hot season" in Rhodesia and South Congo was at the end of the dry weather, i.e., the end of July to the end of September, and not in the summer months, when clouds and rain allayed the heat of the sun. He agreed that the malaria incidence was chiefly from November to the end of March (in the rains), but blackwater fever was worst in the cold weather, i.e., during June. This, however, had no real bearing on blackwater fever. There was a very definite quinine hæmoglobinuria, but it did not much resemble blackwater fever. When quinine was suspended the hæmoglobinuria stopped.

He related a case of a man newly arrived in South Africa, who went to South Rhodesia, remained there three days, and returned to Cape Town. He came back to Rhodesia in three weeks and succumbed at once to blackwater fever. Though no proof, it was extremely suggestive that malaria was *not* the cause. A case might develop blackwater fever after very slight feverish attacks but show no signs of blackwater after very severe attacks of malaria subsequently, and yet have another attack of blackwater fever after only a slight fever two years later—a suggestive fact! He had never known a case of blackwater fever in which the patient took regular and sufficient quinine. Blackwater fever almost always cleared up better and abated more quickly when quinine was given during the attack.

Dr. J. G. THOMSON (in reply) said that he agreed with Professor Dudgeon that the term "immunity," as applied to malaria, was unsatisfactory. In answer to Sir William Prout, he stated that he had seen in Rhodesia severe cases in which hæmoglobinuria could be precipitated by quinine even in very small doses, but he thought the term "quinine hæmoglobinuria" should simply be called blackwater. Clinically the conditions were the same and both were associated with chronic malaria. In answer to Dr. Low, he pointed out that there was no evidence that blackwater fever was a syphilitic condition.

Communications were received from Dr. A. H. Skinner (Hankow) on the diagnosis of early and mild cases of Schistosomiasis japonicum, and Dr. M. L. Treston, Burma, on a "Case for Diagnosis, ? Anthrax Bacilli, found in the Spinal Fluid in a case of Paralysis of the Lower Limbs."

Specimens of *Blanfordia nosophora formosana*, the Intermediate Host of *Schistosoma japonicum* in Formosa (Dr. Carlos França), were shown also.

Specimen of Papillomatous Schistosomiasis of the Rectum (Dr. Mendoza-Guázon).

Specimen of Ascaris Infection of the Liver, showing Ova (Dr. Mendoza-Guázon).



## Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

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### Rice in Relation to Beri-beri in India.

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PRESENT-DAY views as to the association of true tropical beri-beri with the too exclusive use of a rice diet may be summarized as follows :—

(1) When decorticated rice is used as the main staple of diet for any length of time beri-beri appears.

(2) Outbreaks of the disease disappear when whole or under-milled rice is substituted for decorticated rice.

(3) Beri-beri never appears when under-milled rice is eaten.

(4) Beri-beri does not occur when parboiled rice is eaten.

(5) A rice which contains over 0·4 per cent. of phosphorus pentoxide ( $P_2O_5$ ) will not cause beri-beri, while one with less than that amount is unsafe if rice forms the principal article of diet.

(6) The incidence of beri-beri depends on the treatment to which rice is subjected before consumption.

(7) Beri-beri is directly caused by deficiency of a certain vitamin or vitamins in the diet.

I propose in this paper to consider how far these views are applicable to true tropical beri-beri as it occurs endemically in India. In that country, as elsewhere, beri-beri—or, more properly, beri-beri-like maladies—may occur not only in endemic but in sporadic and epidemic form, and may be associated with the use of other foods than rice, but to deal with all three would take more time than I have at my disposal. I shall confine myself, therefore, to the endemic form of the disease, and lay before you the results of my work on this subject during the past few years in India.

The first point to which I would direct attention is that although rice is widely cultivated in India, and is the staple diet of many millions of her peoples, beri-beri is not widely distributed over the peninsula. On the west coast of India, for instance, as much as 60 to 100 per cent. of the cropped area is under rice, while on the east coast as much as 40 to 60 per cent. is under rice. Now the extent to which rice is cultivated in any given locality is a very fair criterion of the extent to which it enters into the dietary of the people of that locality. This being so, and having regard to the profusion of modern rice mills in India at the present time, one would expect beri-beri, in the light of modern views, to be endemic throughout a wide extent of the peninsula. But it is not. The actual distribution of the disease in India and Burma is shown



in fig. 1; it will be noted that the distribution of beri-beri as an endemic is limited to a narrow strip of coastal area situated in the north-east coast division of the Madras Presidency, to a few circumscribed areas in Bengal and Assam, to the coast of Burma, and the valleys of the Irrawaddy and Salween rivers in that Province. Isolated cases, or even small outbreaks, occur from time to time in other parts of India—the sites of these are indicated by Arabic numerals—but these outbreaks are usually confined to troops (British or Indian), to the crews of ships touching at Indian ports, to prisoners in gaols, to certain Chinese communities, and to former residents of endemic areas who have migrated to other parts of India.

In this map I would direct attention to the narrow strip on the west coast which is indicated in black—the Konkan district of the Bombay Presidency. It is in the same latitude as the similar strip on the east coast of the Madras Presidency, where beri-beri has its endemic home. In the former beri-beri is wholly unknown among the civil population; in it rice cultivation is mainly dependent on rainfall, while in the similar area on the east coast it is dependent on irrigation; in the former the percentage of gross cultivated area under rice is 68, in the latter 38; in the former there are 173 rice mills, producing for the most part *raw, milled and polished rice*, in the latter 203 rice mills, producing for the most part *parboiled rice* (although they produce also raw, milled, and polished rice for local consumption). The density of the population to the square mile in the strip of coast on the west is 222, in that on the east 345; in proportion to the population there are as many rice mills in the one as in the other. It may safely be inferred that the consumption of raw, milled, and polished rice is as great per head of the population on the west coast as on the east, yet beri-beri is unknown in the former, while it prevails extensively in the other. In both decorticated rice is the main staple of diet of large sections of the community, and with the exception of pulses is the only grain eaten by the rice-eating classes. Clearly, then, if beri-beri be so closely related to the consumption of decorticated rice as to justify the assertion that it "appears when decorticated rice is used as the main staple of diet for any length of time," it should be as common on the west coast of Bombay as it is on the north-east coast of Madras. Since it is not, it would seem that other factors, apart from the staple diet of decorticated rice, are concerned in its absence in the one locality or in its presence in the other. The possibilities which cross the mind are that either the foods used to supplement the staple diet of rice are much more potent in their protective effect in the one district than in the other, or that there is some influence other than rice peculiar to the endemic area on the east coast which is absent from the non-endemic area on the west coast. There is considerable evidence in favour of the first of these possibilities, but personally I incline to the second; for however potent the supplementary foods used by the people of the west coast may be, there must be some among so large a population who, from poverty or other reasons, do not avail themselves of them, and who would, therefore, be expected to present examples of beri-beri, were its occurrence merely a matter of the too exclusive use of decorticated rice. Yet I have the assurance of unquestionable authority that beri-beri is unknown among the civil population of the Bombay Presidency. I might draw other similar contrasts relating to other parts of India, but the result would be the same, pointing in a conspicuous way to the probability of some peculiarity of endemic areas, apart from the diet of the people, on which the endemicity of beri-beri is in some way dependent. At the same time it is not to be forgotten that the occurrence of beri-beri may be

related as much to the nutritive and vitamin values of the articles used to supplement the staple diet of rice as to the rice itself; and it must be said of the endemic areas of the Madras Presidency that the food grains used for the purpose—usually millet and maize—are not of a quality calculated to afford protection against beri-beri, either when used as supplements to a staple diet of rice or as substitutes for rice.

It is generally believed that the occurrence of beri-beri amongst those who habitually make use of rice as their staple article of diet depends on the treat-



FIG. 1.<sup>1</sup>—I, Baluchistan; II, N.W. Frontier Provinces; III, Kashmir; IV, Punjab; V, Rajputana; VI, Sind; VII, United Provinces of Agra and Oudh; VIII, Nepal; IX, Eastern Bengal and Assam; X, Burma; XI, Bengal; XII, Central Provinces and Berar; XIII, Central India; XIV, Hyderabad; XV, Madras; XVI, Mysore; XVII, Bombay.

ment to which the rice grains are subjected before use by the consumers. The map of the Madras Presidency (fig. 2) shows the limitation of the endemic to the coastal areas of Ganjam, Vizagapatam, Godavari, Kistna, Guntur and Nellore, and its almost complete absence from other parts of the Presidency. Cases of the disease—chiefly amongst troops and emigrants from endemic zones—have been reported from five places in what one may call

<sup>1</sup> For the loan of the blocks illustrating this paper the author is indebted to the courtesy of the *British Medical Journal*.

"the non-endemic areas" of Madras, but the total number of such cases during the past twenty-one years was only 102, as compared with 12,500 cases reported from the endemic area during the past five years. In the endemic area most of the rice produced by the mills is parboiled rice; a less amount of raw, milled and polished rice is, however, produced for local consumption. There is little doubt that in general more raw, milled and polished rice than parboiled rice is eaten by the people residing in these endemic areas. In parts of the Presidency outside endemic areas parboiled rice is mainly used, so that in accordance with present-day views it would seem that the distribution of the malady in this Presidency is associated with the consumption of raw, milled and polished rice rather than with that of parboiled rice. Limiting our area, however, to the endemic localities themselves, we find a number of facts which make it impossible to affirm either that "beri-beri *never* appears when under-milled rice only is used," or that "beri-beri *does not* occur when parboiled rice is eaten."

The first of these is of great interest; it was recorded by Malcolmson so long ago as 1835, and has recently been confirmed by myself, although not until my own observations had been made did I have the opportunity of reading his remarkable book, entitled "A Practical Essay on the History and Treatment of Beri-beri." The facts relate to the peculiarly limited distribution of the disease. Malcolmson found that it "prevailed extensively" a century ago—and probably for long before that—in precisely the same areas in which I have found it to prevail to-day. He emphasized that it was rarely met with further inland than fifty miles from the coast, and then only amongst emigrants from the endemic area—facts equally true to-day. Now, it is only within comparatively recent years that the machine milling of rice has been practised in this district, and during these years the practice has extended rapidly; but despite the present profusion of rice mills beri-beri appears to have extended its endemic borders but little, if at all, during the past eighty-nine years, for I have found these borders to be practically the same as in Malcolmson's day.

Thus beri-beri "prevailed extensively" in its present endemic haunts long before the machine milling of rice was introduced, and at a time when the home pounding of rice and the use of unpolished rice were universal customs. But although the machine milling of rice is now so commonly practised in Madras, and especially in those parts of it where beri-beri is endemic, it is still not practised in places distant from the towns where rice mills are located. In country districts the home pounding of rice is still the common practice, and beri-beri still prevails amongst the users of home-pounded or unpolished rice in endemic areas, although it is less common amongst them than amongst the users of raw, milled and polished rice. Out of a total of 12,500 cases of the disease occurring in the six districts of the north-east coast of Madras during the past five years, over 2,000 were reported from country districts in which home-pounded (raw or parboiled) unpolished rice was in general use. In this respect different districts vary greatly; thus of 5,101 cases in the Kistna district 293, or 5·7 per cent., occurred among users of home-pounded unpolished rice, 4,808 among the users of milled and polished rice. The percentage was somewhat higher in other districts in this endemic area, but only in Ganjam did the number of cases reported as beri-beri among the users of under-milled rice exceed those among the users of polished rice, and in this case I have reason to believe that epidemic dropsy was included in the returns. It is inadvisable to place too much reliance on statistics, but there can be no reasonable doubt that in endemic areas of the Madras Presidency beri-beri can and does prevail,

and has so prevailed for a century at least, among the users of home-pounded or under-milled rice. Speaking generally, it may be said that in these areas the disease is approximately ten times less common among the users of such rice than among those who habitually use polished rice. To approximately this extent does the incidence of beri-beri depend on the treatment to which rice is subjected before consumption, but to this extent only. But while it is not true of endemic areas of the Madras Presidency that beri-beri *never* appears when under-milled rice only is used, it is true of non-endemic areas. In the former I have found the disease to be nine times more common in towns where the use of polished rice is fairly general than in country districts where the

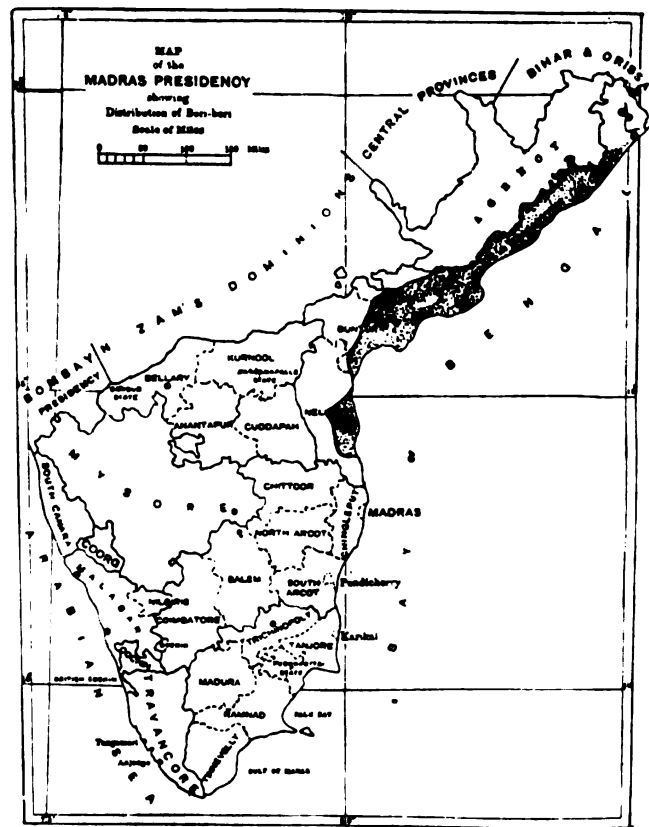


FIG. 2.

use of home-pounded rice is general. In the latter the disease is very rare or unknown.

Similarly, it is not true of the endemic areas of the Madras Presidency that beri-beri does not occur when parboiled rice is eaten, although it is true of non-endemic areas. It is certainly less common among the users of parboiled rice in the former areas, but it is far from unknown among them. Thus, the disease is rife among the users of this rice in such coastal towns in the endemic zone as Cocanada, Masulipatam, and Vizagapatam. The same is true of Rangoon. In this connexion it may be mentioned that the only rice with which I have succeeded in producing beri-beri in pigeons when used as their exclusive food

was of the status of parboiled rice, and was actually taken from the stock in use by a group of sufferers from the disease in Masulipatam.

Now, to my mind the chief point of interest in these observations is that while under-milled and parboiled rices may, and apparently do, afford protection against beri-beri, outside the endemic zone, they do not always afford protection against it in the endemic zone—a fact which again points to some peculiarity, apart from the rice diet, in the endemic zone on which the endemicity of the disease would appear to depend. In this connexion mention may be made of three facts of great interest.

(1) Contrary to the usually accepted belief, Mohammedans in endemic areas of the Madras Presidency suffer more from beri-beri than Hindus, despite that the food of the former is much more varied, and that the latter restrict themselves much more rigidly to a rice dietary. When I made this observation a few months ago it seemed to me incredible, and it was not until I found out that Malcolmson, Geddes, Macdonnell, Colhoun and others had also made it nearly a century ago that I was satisfied as to its truth. In regard to this matter of caste, Pariahs, who eat anything, suffer most from beri-beri in endemic areas of the Madras Presidency; native Christians, who have no food scruples, suffer next in order of incidence of the disease; Mohammedans, who eat most things, come next, and Hindus, the greatest rice-eaters of all, last in order of affliction by beri-beri. Here also it may be said that the orthodox Hindu does not use parboiled rice, while other castes do.

(2) The second fact is of equal interest—namely, that although at the present time the diet scale of Indian prisoners is the same in all central and sub-gaols in the Madras Presidency (consisting of millet or maize flour, rice, pulses, or vegetables, oil, and condiments), beri-beri *originates* only in those located within the endemic zone. Such cases as have been recorded during the past ten years in gaols located outside the zone are stated to have been imported from the endemic zone.

(3) The third fact is of still greater interest. Out of a total of 565 cases of beri-beri which occurred among Indian troops located in the various military stations throughout India during the years 1900 to 1914, 492, or 89 per cent., were in Madrassis, and the majority of these in men recruited from the north-east coast division of Madras, where beri-beri is endemic. Facts such as these almost force one to the conclusion that there are factors concerned in the causation of beri-beri in endemic areas—apart from the too exclusive use of a rice dietary—which are absent or of feeble potency in non-endemic areas. It would seem that residence in an endemic locality imparts to certain individuals a susceptibility to the disease which favours its development under conditions of life—faulty food, imperfect hygiene, over-exertion, hardship, cold, and damp—that do not suffice to cause it in others not rendered susceptible to it by such residence. It is always among recruits from endemic localities that the disease is most common in Madrassi regiments. Although their scale of rations is the same as that of men of longer service, they succumb more readily to beri-beri in consequence of their conditions of service (drilling and the like) in the army. Experimental evidence also supports the view that residence in endemic areas imparts to certain individuals a susceptibility to this malady which they take with them to non-endemic areas. My own observations in this regard support those of Malcolmson in 1835.

It is said that rice which contains over 0.4 per cent. of phosphorus pentoxide ( $P_2O_5$ ) will not cause beri-beri, while one with less than this amount is unsafe if rice forms the principal article of diet. I believe that

workers in the Dutch Indies have found this rule to have so many exceptions that it can no longer be regarded as a rule. The chemical investigations of Dr. Norris, who has been associated with me in the study of the rices in common use in India, confirm the results of workers in the Dutch Indies. They show that although rice of low phosphoric acid content is more likely to be associated with beri-beri in endemic areas, yet in these areas the malady may occur among the users of rice having a phosphoric acid content as high as 0.45 or even 0.5 per cent. On the other hand, in non-endemic areas rice of low phosphoric acid value may form the staple diet of many people, yet the users of it do not suffer from beri-beri. Thus, one rice in common use in the Tanjore district of the Madras Presidency, where beri-beri does not occur, contains only 0.36 per cent. of phosphorus pentoxide. A more detailed reference to these chemical investigations would take me too far afield; those who are interested will find a full account of them in the report by Dr. Norris and myself recently submitted to the Indian Research Fund Association and presently to appear in the official publications of that body.

Two further points may be mentioned before closing this section of my paper: I have found no evidence in Madras that the export of rice from endemic areas of that Presidency is associated with the spread of beri-beri. Thus rice is exported from these areas to Ceylon, where beri-beri does not originate (Castellani and Chalmers), such cases as there are being imported cases. Nor have I found any satisfactory evidence that the malady is associated with the storage and deterioration either of paddy or of rice. The limited evidence I was able to collect did not suggest that the generation of a poison in rice was concerned in the causation of the disease. There is one point in regard to deteriorated rice which calls for mention—namely, that such rice to be eaten at all must be thoroughly washed, thereby reducing its vitamin value to the status of a highly polished rice (*vide infra*). It is possible that the suspicions attached to deteriorated rice (which is commonly parboiled rice of comparatively high vitamin value) may be capable of partial explanation in this way.

So far, then, it would seem that the generally accepted views in regard to rice and beri-beri summarized at the beginning of this paper require considerable modification so far as India is concerned. There remains now to be considered the statement that "beri-beri is directly caused by deficiency of a certain vitamin or vitamins." The evidence I have to lay before you in this connexion is based on the experimental study of a large number of rices in common use in India, and of the paddy from which they were prepared. The general scheme of the experiments was this: Pigeons from the same locality, and as nearly as possible of the same age, weight, and strain, were divided into groups of six in such a way that the average weight of the birds in each group was the same. Each group then received as its exclusive food one of the rices or paddy to be tested. The effects of the various treatments to which paddy may be subjected before it reaches the consumers from the mills are shown in fig. 3 (p. 73). The paddy used was specially milled for me, both in the raw and in the parboiled state. Parboiling consists in the steeping of the raw paddy in tanks for twenty-four hours or longer. The water is then drained off, and the soaked rice conveyed to metal receptacles through which steam at high pressure is passed for fifteen minutes or so. The soaked and steamed rice is then removed and dried in the sun. Parboiling causes the pericarp to adhere to the grain, so that it is much more difficult to remove in milling. It thus preserves to the rice grain much of its vitamin B; but the

process involves a considerable loss of vitamin A, so that pigeons fed exclusively on certain parboiled rices sometimes develop ophthalmia.

In the experiment the same paddy in both the raw and the parboiled state was submitted to various degrees of milling and polishing. Adult birds were then fed on the various products; the average weight of the birds in each group was 310 grm. Their weight curves are shown, for the sake of clearness, as straight lines, and represent the average weight of the six birds in each group. It will be noted that the various products derived from paddy group themselves in order of nutritive value as follows: the raw, unpolished rice is the best; next comes the raw paddy; then follow in a diminishing order the parboiled unpolished rice; the parboiled paddy; the parboiled lightly polished rice; the parboiled highly polished rice; and finally, and worst of all, the raw polished rice. Amongst other things the chart demonstrates three of the utmost importance: the beneficial effect of parboiling, the effect of varying degrees of milling and polishing, and the enormous loss in nutritive and vitamin values entailed by the high milling and polishing of rice. These things have been known for long, but it is convenient as well as instructive to see them depicted relatively to one another on the same chart.

In fig. 4 (p. 74) the nutritive and vitamin values of thirty samples of paddy and rice in common use in India are compared one with another and with a normal diet of mixed grains. The samples were obtained from endemic areas of beri-beri as well as from localities where the disease does not prevail. Some of them were the actual rices eaten by sufferers from this disease. Each sample formed the exclusive food of a group of six young pigeons of approximately the same age and strain, all of which were obtained from the same locality—the last point is most important. The weight curve of each group (again shown for the sake of clearness as a straight line) starts at the outset of the experiment from a common point—250 grm.—which represents the average weight of the birds in each group. All weight lines are shown as extending, or as being prolonged, by interrupted lines, to the 100-day mark. At the point where the weight lines, or their prolongations, touch this mark the average daily gain or loss of body weight in grams, caused by the exclusive use of the particular paddy or rice, can be read off. For the comparative purposes of this investigation the figure thus arrived at may be spoken of as the “nutritive value” of the paddy or rice. It will be observed that the different products group themselves in a diminishing order of nutritive value: raw paddy, parboiled paddy, parboiled rice, washed parboiled rice, raw milled rice, and washed milled rice, with occasionally a raw milled rice interposing itself into a group of higher nutritive value. The line at which weight is, on the average, neither lost nor gained is indicated by the word “maintenance.”

For the purpose of comparison of the “vitamin values” of the various rices it was necessary to have some “unit of vitamin value.” This unit was designated by the figure 1; it is defined as that amount of vitamin B required daily to keep the average pigeon weighing 250 grm. at maintenance for 100 days without the development of polyneuritis. (It may be mentioned in passing that it is an amount which varies within fairly wide limits in different individuals.) The vitamin value of any particular rice is represented by the average number of days required by it to produce polyneuritis in a group of six young pigeons. This average is indicated in the weight lines by an arrow pointing downwards; by following the arrow downwards to the scale at the bottom of the chart the approximate vitamin value of any particular rice can be read off.

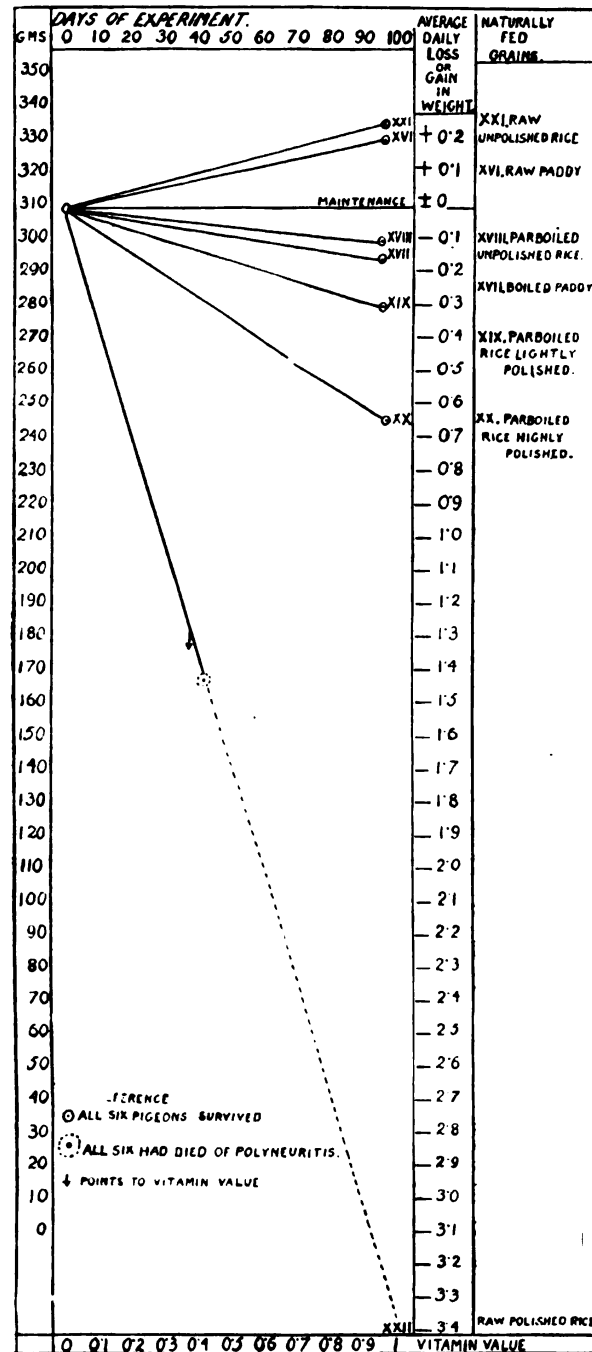


FIG. 3.—Diagram showing the effect of parboiling, milling, and polishing on the nutritive and vitamin values of "red Samba" paddy as determined by feeding groups of six pigeons on the various products.



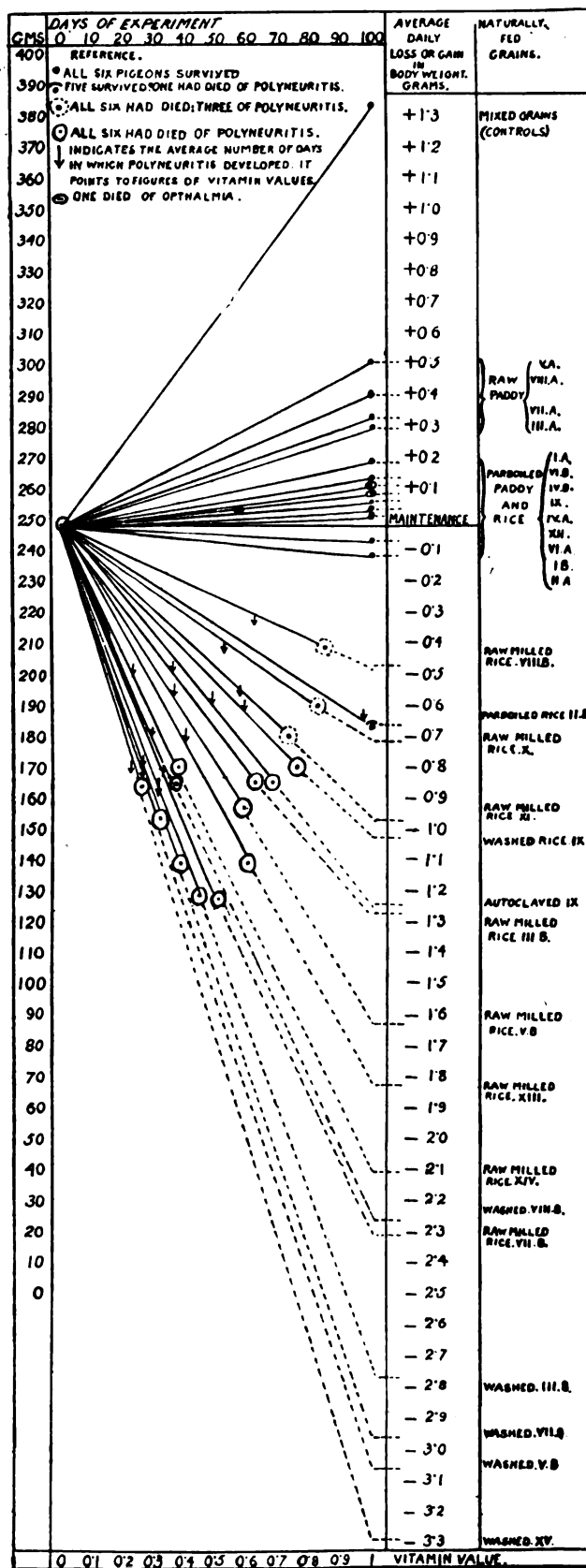


FIG. 4.—Diagram showing the comparative nutritive and vitamin values of paddy and rice as determined by feeding groups of six pigeons on the respective grains.

This chart shows a number of interesting points, which time does not permit me to discuss; among them are the wide variations in nutritive and vitamin values of the rices in common use in India; the fact that the one value does not always run parallel with the other; the superiority of parboiled over raw milled and polished rice; the effect of washing on paddy and rice in lowering their nutritive and vitamin, and, I may add, their phosphoric acid values; the fact that a parboiled rice may be reduced by washing to the status of a raw milled and polished rice; and that few rices in common use in India—and none in the present series—are wholly devoid of vitamin B, all of them containing some which may be extracted by washing. The most important point brought out by this chart, however, is that rices known to have been habitually used by sufferers from beri-beri are not by any means always those most deficient in vitamin B. Among these rices are two (viiiB and xi) obtained from the stock of rice actually used by sufferers from beri-beri. Both of these are of relatively high vitamin value, viiiB being in fact the best raw rice in the series so far as its content of vitamin B is concerned. It would seem, indeed, that there is no strict parallelism between the degree of the vitamin deficiency of these rices and their association with the human disease. Some of the worst rices in the series are from localities where beri-beri does not occur. A rice may be potent to cause polyneuritis columbarum, and yet not be associated with human beri-beri in places where it is known to be the staple article of diet of human beings. In this series it is rather those rices that are relatively less deficient in vitamin B than those that are relatively more deficient in this factor that are known to have been associated with the human disease. If beri-beri in man be indeed directly caused by deficiency of vitamins, there is matter for reflection in this observation that the malady is as likely to arise, and indeed so far as this series goes more likely to arise, when the deficiency is less than when it is greater.

Rice No. xi, which was obtained from Masulipatam, the most noted centre of beri-beri in India, is interesting; it was obtained from the stock actually in use by sufferers from the disease; its vitamin value is relatively high; of twenty-eight rices used it was the only one whose exclusive use was associated in pigeons with the occurrence of a malady having the complete pathological characters of true beri-beri—enlarged and degenerated heart, enlarged liver, enlarged adrenals, polyneuritis, and hydropericardium with or without other evidence of œdema.

Fig. 5 (p. 77) shows how variable are the effects of this rice in different birds—a point of the first importance. The chart gives the results of an experiment in which this rice was used as the exclusive food of three adult pigeons. It will be observed that one bird maintained an almost level weight—little less than the average of three adult controls on a normal diet of mixed grains—and survived for a hundred days; that another began to lose weight on the tenth day, and died of septicæmia on the thirty-eighth day of the experiment; and that the third died of beri-beri on the fiftieth day. Some may say that the second bird ought to be excluded because the additional factor of infection was in operation, but the operation of this factor was favoured by the rice diet; and why exclude it any more than the third bird, which died of beri-beri on the fiftieth day—for may not an additional factor have been operative in this also? May not this bird also have been the carrier of some unknown agent capable of causing beri-beri, as the second was the carrier of an agent capable of causing septicæmia? or may not it have been the producer of such an agent? If not, why should it die of beri-beri while the first survived for a

hundred days in apparently fair health on the same deficient food? It is constantly being impressed upon one in the course of such experimental work as this that under conditions of faulty food "the 'carrier' may become a patient," to use a happy phrase of Sir Humphry Rolleston's. I have from time to time recorded examples of disease arising in this way, the most notable of which is epithelioma contagiosum of birds. I desire here to emphasize this point of view in connexion with possible causative agents in true beri-beri. The variability of the effects of a notoriously "beri-beri-producing rice" in different individuals is illustrated by the fact that of eighteen birds fed upon this rice in 1923, two developed beri-beri, six polyneuritis (in from seventy-five to ninety-five days), four died from other causes, while the remainder survived for a hundred days. The incidence of beri-beri in this series was 11 per cent., while treble that percentage escaped obvious clinical signs of the disease other than some loss of weight.

The well known polyneuritis so readily produced in pigeons by an exclusive diet of polished rice is not the same thing as beri-beri, as the latter disease is seen either in man or in pigeons. In pigeons the great difference between them is that in polyneuritis the heart and liver are atrophied, while in beri-beri the heart and liver are enlarged and are the seat of pathological processes. These also are the main differences between human beri-beri and polyneuritis columbarum—differences to which I drew attention in 1919.

It was not until a few months ago that I ever saw a case of beri-beri in pigeons, although I had in previous years fed many hundreds on polished rice and a considerable number on rices credited with beri-beri-producing properties when used by human beings. Among the latter there was one that had been actually in use by sufferers from human beri-beri when I procured supplies from their stock in 1919 and again in 1923. Yet in 1923 I find 11 per cent. of cases among pigeons fed exclusively upon this rice which failed to cause the disease in pigeons in 1919. Why is this? It would not seem to be because this beri-beri-producing rice differed in 1923 from the same rice in 1919, for in both years it was associated with beri-beri in man. If this be so, it must be concluded that it was because of some difference in the birds used in the two years—a difference in susceptibility, constitution, infection, or what not, and a difference which involved only 11 per cent. of them. Now, in 1923 the pigeons used were obtained from Bangalore; and although beri-beri can hardly be said to be endemic in Bangalore, yet it is one of the places outside the endemic zone where cases of the disease crop up from time to time. In former years I had obtained my birds from places where beri-beri never occurred. It seems possible, even probable, that the place of origin of the birds may have been the determining factor in the occurrence of beri-beri columbarum under the conditions of my experiments. I have already emphasized, as Malcolmson did eighty-nine years ago, that residence in a locality where beri-beri occurs imparts to certain individuals a susceptibility to this malady, and I illustrated my point by the high incidence of the disease among Indian troops recruited from the endemic zone in Madras. It may be that the occurrence of beri-beri in my pigeons was as much a question of individual susceptibility or other variations in the birds themselves as of the vitamin insufficiency to which they were subjected while under experiment. Those individuals in which beri-beri appeared may possibly have been the carriers of an agent capable of imparting to polyneuritis columbarum the character of true beri-beri, or they may have been of such a constitution that under conditions of vitamin insufficiency a poison was evolved in them capable of so doing—a

poison exerting a specific action on the heart. Some may say that the rice itself contained such a poison. It may be so; but if it be, why in 1923 should two birds out of eighteen develop beri-beri and not the remaining sixteen? And why in 1919 should none develop it?

But I have further evidence to present in this connexion. It was not only amongst Bangalore birds fed on this beri-beri-producing rice from Masulipatam that beri-beri occurred; it occurred also amongst Bangalore birds fed on a mixture of millet and freshly milled and polished rice grown in the Tanjore district where beri-beri does not occur. In the latter diet there was no question of deterioration of the rice or of a contained poison, for it was freshly milled

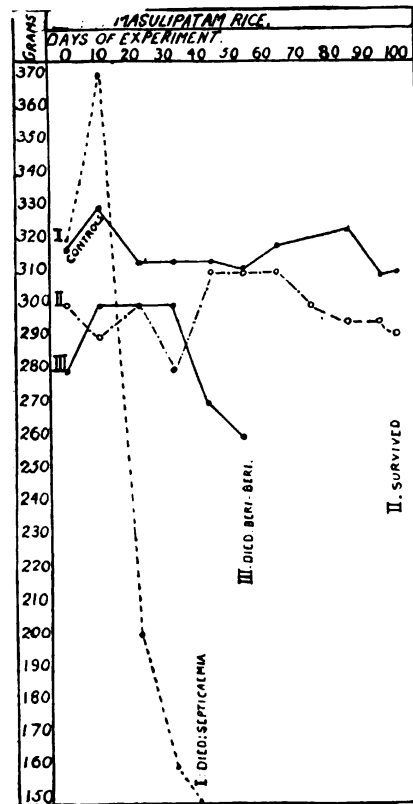


FIG. 5.—Showing the varying effects of "beri-beri-producing" rice in different pigeons.

for me, and my supplies were stored under conditions which did not cause deterioration. Indeed, neither the rice itself nor the millet which composed this diet, when used separately as the exclusive food of pigeons, caused beri-beri. The rice, being highly polished, caused polyneuritis in the usual short period of twenty-five days or so; the millet maintained the birds fed exclusively upon it in fairly good health; yet the two together led to the occurrence of beri-beri in a percentage of the birds fed on the mixture. The mixture was productive of beri-beri not because of complete want of vitamin B, but because of insufficiency of vitamin B, for the millet provided a sufficiency of this factor for its own metabolism, but not a sufficiency for the vitamin-poor polished rice with which

it was mixed. In the experiment the occurrence of beri-beri columbarum was a question not of complete want of vitamin B, but of insufficiency of this vitamin, and it illustrates further that this insufficiency operated in association with some unknown factor which the afflicted birds themselves provided—a factor capable of superimposing upon polyneuritis columbarum (due solely to vitamin want) the characters of true beri-beri.

The only difference between the beri-beri produced in pigeons by the beri-beri-producing rice from Masulipatam and that produced by the mixture of millet and Tanjore polished rice was in case incidence; on the former diet 11 per cent. of the birds developed beri-beri; on the latter 2 per cent. But this may well have been a matter of chance, due to the presence in the batch of birds fed on the former diet of a larger proportion of "susceptible" birds, although I would not wish to affirm that actually it was so. Nevertheless, since a non-deteriorated diet of millet and freshly milled rice gave rise to beri-beri columbarum, too great significance cannot be attached to deterioration of the "beri-beri-producing rice" as the dominant cause of the malady.

It is significant that none of my pigeons fed on highly milled and polished rice developed beri-beri, although they developed polyneuritis; the reason seems to be that when fed on a rice almost wholly devoid of vitamin B they did not survive long enough for beri-beri to develop. The latter malady took, in my cases, forty-nine days on the average to develop, while birds fed on highly polished rice usually contract polyneuritis within twenty-five days. Here I may be permitted to point out that, important as it is to know precisely the effect of vitamin want in an otherwise complete diet, it is still more important to know precisely the effect of vitamin insufficiency in association with the other food faults with which it is accompanied in the diets of man and animals. For there is no such thing in nature as a diet wholly wanting in vitamin B which is not at the same time imperfect in other respects. I cannot help feeling that we may neglect, in the profusion of our experiments wherein properly balanced synthetic diets are used to ascertain the effects of lack of a particular vitamin, to study the effects of those diets in common use, and associated with disease production, which are not only deficient in vitamins, but ill balanced as well. However this may be, a synthetic diet wholly devoid of vitamin B will not be as likely to cause true beri-beri either in animals or in man as one containing an insufficiency of this vitamin and an excess of starch. The fact that beri-beri did not arise in pigeons fed exclusively on highly polished rice is significant also, inasmuch as it shows that beri-beri, in so far as it can be said to be caused by vitamin want, is caused rather by insufficiency than by complete want of it. Paradoxical as it may sound, it needs a little vitamin B—always provided this little be an insufficiency—to enable beri-beri to arise; that is to say, to enable the birds to live long enough for this disease to develop. I doubt whether true beri-beri can with truth be said to be directly caused by deficiency of vitamin B, although I have no doubt that insufficiency of this substance is an essential factor in its causation. I expressed this doubt in 1919. I then stated that although vitamin B deficiency was the essential pathological factor in beri-beri, it was rarely so complete as to be the sole agent responsible for it. With my present experience I would word this conclusion a little differently, yet, worded as it was, it was found "to be in remarkable agreement with the clinical observations on beri-beri in Mesopotamia" during the late war.

I will now contrast the incidence of beri-beri in my pigeons with the incidence in man. Among 119 Bangalore pigeons which were fed until death or for 120 days either on the beri-beri-producing rice from Masulipatam, or on

raw milled and polished rice and millet, four showed at post-mortem examination pathological conditions which entitled the disease from which they had suffered to be regarded as beri-beri—that is to say, the heart was much enlarged and degenerated, and averaged in weight 5.2 grm.; the average in fifty-one control birds from the same locality being 3.1 grm., the maximum 4.1 grm. The liver was also much enlarged, as were the adrenal glands, while hydropericardium was present in all cases. These features, in addition to polyneuritis, are characteristic of the human disease. Of the 119 birds, fourteen were excluded either in consequence of death from accidental causes or from such maladies as epithelioma contagiosum. There were thus four cases of beri-beri columbarum among 105 birds fed in the way described—a case incidence of 3.8 per cent. Cases of intermediate type bridging the gulf between polyneuritis columbarum and beri-beri columbarum also occurred, but in these some one of the characteristics was wanting on which the diagnosis of beri-beri was based; of well defined cases there were four, or 3.8 per cent. Compare this case incidence with that in man; the military medical records of India provide at least one example of an outbreak of this disease in man, the circumstances of which are well authenticated. The outbreak occurred in a battalion of Gurkhas approximately 800 strong. The whole battalion had for several months been subsisting on a diet made up of decorticated rice, pulses, vegetables, oil and condiments, and for a fortnight before the appearance of the outbreak the rice was of bad quality. The diet was thus one that contained an insufficiency of vitamin B, but it was not wholly lacking in this vitamin. It may be that the outbreak was precipitated by the bad quality of the rice during the fortnight preceding its appearance, for, as I have pointed out, such deteriorated rice to be used at all must be well washed and its vitamin value thereby greatly reduced. Among the 800 men there occurred thirty cases of beri-beri, or a case incidence of 3.7 per cent. The outbreak, which was of a cardiac type, was promptly cut short when a proportion of the rice was replaced by atta (whole wheat flour).

The outbreak in the battalion and that in my pigeons resemble one another both in regard to the case incidence of the disease and in regard to the fact that the malady was associated with the use of a diet which contained an insufficiency of vitamin B, but was not wholly wanting in it. The question now arises: Why did 770 out of 800 men escape beri-beri when all had been subsisting on the same deficient food for months past, when, in fact, all were subjected to the same degree of vitamin insufficiency? And why did 101 out of 105 pigeons escape beri-beri when all were fed alike? Presumably because there was in operation in the thirty men who developed it some factor in addition to the vitamin insufficiency which was absent, or failed to affect in the same degree, the 770 who did not develop it. Similarly with the pigeons. If we look at the matter from the point of view of the heart condition alone we are at once impressed by the fact that some additional factor must have been in operation in beri-beri birds which exercised a specific effect on the heart, for vitamin deficiency alone does not cause cardiac enlargement in pigeons, but, on the contrary, cardiac atrophy. Surmise as to the nature of this factor is permissible, since if it is to be found we must have some line of thought along which to work. It is possible that the subjects of beri-beri may have been carriers of some organism whose products exercised this effect on the heart muscle, just as certain of my pigeons were carriers of an invisible virus which exercised a specific effect on epithelial tissue, and the fact that beri-beri arose only in birds obtained from a certain place is presumptive evidence in favour of such

a view; or the gastro-intestinal flora of the sufferers from beri-beri may have been of such a nature as caused the evolution of a poison which exercised this specific effect on the heart; or it may be that certain birds were of such a constitution that the disturbed metabolism resulting from the vitamin insufficiency caused a poison to be evolved in those who suffered from beri-beri which was not evolved, or not to the same extent, in those who escaped. Or was it merely a question of individual idiosyncrasy, and, if so, what do we mean by that? Is not individual idiosyncrasy likely to depend on such things as I have just mentioned? But even if this be so, individual idiosyncrasy to disease resulting from deficiency of vitamin B depends in great measure on the degree of the deficiency; the greater the deficiency the less the variation in individual idiosyncrasy: the less the deficiency—provided there be an insufficiency—the greater the variation in individual idiosyncrasy to disease resulting from it. So on a diet such as those used in the experiments I have detailed, and such as that used by the battalion above referred to, the vitamin values of which were in the neighbourhood of 0·6, one would expect many individuals to escape the grosser manifestations of disease (such as polyneuritis) consequent on their use. But whether the poison which imparts to polyneuritis columbarum the characteristics of true beri-beri be an extrinsic or an intrinsic one, I am of opinion that such a poison exists; that it is one peculiar to certain places or which is evolved in persons residing or who have resided for considerable periods in certain places; and that its operation is rendered possible by the insufficient intake of a certain vitamin or vitamins in an otherwise ill-balanced diet excessively rich in starch and deficient in suitable proteins. The conclusion which I reached in 1919, that vitamin deficiency is rarely the sole agent responsible for the causation of beri-beri, has been strengthened by my investigations during the past five years.

In the course of my work I have produced in pigeons, by means of the rices in common use in India, no less than four "beri-beri-like" diseases:—

- (1) Infective polyneuritis, due to infection, but favoured in its genesis by vitamin deficiency.
- (2) Polyneuritis due to vitamin deficiency alone.
- (3) Polyneuritis due to vitamin deficiency, but precipitated in its onset by various infections.
- (4) A malady presenting the characteristics of true beri-beri, and due to an unknown agent acting in association with insufficiency but not complete want of vitamins.

All these have many clinical and pathological features in common, although all have their own distinctive characteristics—easily enough recognizable at post-mortem examination if not always recognizable clinically; in all, vitamin deficiency, in association with the usual food faults which accompany it, is more or less concerned, directly or indirectly, in their causation. If there be this variety of beri-beri-like maladies in pigeons, whose staple diet is rice, there is probably a similar variety of beri-beri-like maladies in man—a surmise which proved to be correct in Mesopotamia during the late war. I am not at all convinced from the literature that when we argue as to the causation of "beri-beri" we are always arguing about the same condition. And so much has this been impressed upon me of late that I look for further progress along clinical and pathological lines which will separate these beri-beri-like states, into entities recognizable as such by the practising physician. Only then will the causal factors in their production be capable of accurate determination.

## DISCUSSION.

Dr. H. S. STANNUS said that he had seen very little beri-beri, though he had seen a good deal of the allied conditions. One thing about the theory of beri-beri had always struck him as peculiar—namely, that the absence of a certain factor should by itself be able to produce something—and he had been interested that evening to hear Colonel McCarrison say that in practically all the cases which figured in his experiments with pigeons the insufficiency of vitamin B was associated with some other unknown factor which the afflicted birds themselves provided. He believed that Colonel McCarrison had found this to be true in all the conditions he had noted, except polyneuritis columbarum, and he (the speaker) was of opinion that further investigation would show that it was true of this condition also. He had never been able to understand how the absence of a particular dietetic factor could produce a neuritis; it seemed rather a contradiction in terms.

Dr. ARTHUR POWELL said he had seen most of his cases of beri-beri in the days before vitamins were spoken of. From the resemblance of its symptoms to those of other forms of neuritis definitely caused by poisons such as alcohol, arsenic, emetine, &c., he had always suspected a toxic factor was at work in addition to the diet deficiency.

It was interesting to recall that in pre-war days some of his patients suffering from beri-beri or "epidemic dropsy," which he considered one disease, were in the habit of treating themselves with yeast. The results seemed to him so good, that in his capacity as consulting medical officer to the P. and O. Company he had ordered rations of marmite to be issued to the Indian crews of the ships on the Eastern station whenever any suspicion of beri-beri arose.

Dr. H. H. SCOTT said he had seen a certain number of cases of beri-beri in China and amongst the Chinese in the West Indies. The Spanish observer, de Melita, laid considerable stress on the factor of soil in the production of the disease, and stated that on an outbreak occurring, upon removing the cases away from the district to another district not remote the symptoms cleared up, although no change was made in the diet.

Colonel WILKINSON said that a point of interest was the occurrence of septicæmia amongst the experimental pigeons. Might not the presence of sepsis possibly be an explanation as to why some of the pigeons developed beri-beri while others did not? Continuing the same line of thought, it was well known that in the treatment of diabetes with insulin there were cases in which insulin did not produce any effect on the course of the disease, possibly owing to some obscure infection or constitutional difference. It might be that the presence of sepsis was a factor in the occurrence of beri-beri.

Colonel O'KINEALY said that one striking point about Colonel McCarrison's observations was the prevalence of beri-beri in particular areas of country. It seemed to him (the speaker) that perhaps the area might be at fault rather more than the actual diet or the form of rice used as food. He had no doubt that a thorough investigator like Colonel McCarrison had not lost sight of that point, but it was possible that more might be discovered about the causation of beri-beri if greater attention were paid to the environment of the individuals who suffered from it in endemic areas, and who apparently were carriers to areas not endemic.

Professor L. S. DUDGEON (President) asked whether Colonel McCarrison could say anything about the increased weight of the heart in beri-beri, and could affirm it to be due to the condition of oedema. Experimentally, if animals were treated with diphtheria toxin, an enlargement of the heart was produced, and the heart showed increased weight. This was an oedematous condition, and he believed that the same fatty change was recorded in fatal cases of beri-beri. He wished also to know whether Colonel McCarrison had any positive knowledge with regard to the infectivity of beri-beri. In this connexion he referred to the work of Hamilton Wright, who believed in an infective origin of beri-beri, and brought back from the Federated Malay States a bacillus which he had isolated from fatal cases, very largely from the duodenum. He (the President) remembered the organism quite well, and he showed it at the Society of



Tropical Medicine, as it was then called. He also carried out such tests as he could with the organism. He found it not pathogenic to the lower animals, and it was an organism such as one might meet with in the intestinal tract of normal human beings in this country. Hamilton Wright had wanted further experiments to be carried out, but nothing short of the introduction of the organisms into the human subject, would, to his (the speaker's) mind, have been of any further use.

Colonel MCCARRISON (in reply) said that his object had been to describe the results of his work up to date, and he really had no more information to impart than was contained within his paper. Unfortunately, at the most critical point of the work, the Inchcape axe fell, so that his researches were brought to an untimely end, and he was not able to follow up the investigation of true beri-beri in pigeons. But when he found his first case of beri-beri in a pigeon, he aspirated the blood from the heart of every pigeon which developed polyneuritis thereafter, in the hope that amongst them there would be some that would have true beri-beri; it happened that there were three which had true beri-beri, and in all the cases he found that the heart's blood was sterile, so that he had nothing to add to the President's remarks about the infectivity of the disease. He remembered Hamilton Wright's work very well, and, of course, it was possible that although an organism resident in the gastro-intestinal tract in man was not pathogenic to the lower animals, it might nevertheless be capable of assisting in the production of a toxin when the food was deficient in a certain quality.

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SESSION 1923-24

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SECTION OF UROLOGY



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## Section of Urology.

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## Section of Urology.

President—Dr. W. LANGDON BROWN.

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### The Influence of the Endocrines on the Work of the Kidney.

#### PRESIDENT'S ADDRESS.

By W. LANGDON BROWN, M.D., F.R.C.P.

I REGARD the honour of my election to the chair as a recognition by the Section that urology has a medical as well as a surgical aspect. One must admit that of recent years it is the surgical aspects that have shown the most advance, and that the surgeons have taken the most interest in the work of the Section. I therefore appeal to my fellow physicians not to lag behind but to emulate the good example of the surgeons.

I have chosen for my address the relation of the endocrines to the work of the kidney because it gives me the opportunity of bringing forward a view of the biological position of the endocrine system which has interested me of recent years; and of considering this more particularly in relation to the regulation of the excretion of water by the kidney.

The primitive excretory organs, as we find them in the invertebrates, are nephridia, typically segmental organs opening internally into the body cavity and externally on to the surface. At first they are all alike, as in the worms and that primitive arthropod *Peripatus*, but in the more specialized arthropods they have become differentiated according to the region of the body in which they lie. The most anterior group become condensed into the green gland of the Crustacea which still retains an excretory function. On Gaskell's theory of the origin of the vertebrates from arthropods, the old alimentary tract, of which the infundibulum represents the mouth, became the central canal of the nervous system. In the Crustacea the gills keep up a vigorous current of water from behind forwards, so that waste products discharged in this situation are speedily carried away from the animal, whereas in the fish, with the current going in the reverse direction this would lead to contamination of the water supplied to the gills. Circumstances alter function, and a new use was found for the gland in this position; it became the pituitary. Comparative morphology points to the conclusion that in the neighbourhood of the gill-slits, segmentally arranged structures give rise to the tonsils, thyroid, parathyroids and thymus, in the position which might be expected if they are homologous with nephridia, and on Gaskell's theory they represent the modifications of the second group of nephridia. It will be noted that two of these are now lymphatic in structure while the others are glandular. Now in both the invertebrates and the lower vertebrates part of a nephric tubule may become

## 2 Langdon Brown: *Influence of Endocrines on Work of Kidney*

converted into lymphoid tissue, which shows its association with excretory functions by taking up into its leucocytes alizarin blue and carmine which have been injected. A nephridium may thus differentiate in two directions. But it may become modified in yet a third direction. The position and arrangement of the uterus in *Limulus* closely resembles that of the thyroid with its thyroglossal duct in *Ammocoetes*, the larval lamprey, one of the most primitive of the vertebrates. Not only is the presence of this duct thus explained, but as Gaskell says, "the relationship which has been known from time immemorial to exist between the sexual organs and the thyroid in man and other animals, and has hitherto been a mystery without any explanation, may possibly be the last reminiscence of a time when the thyroid glands were the uterine glands of the paleostracan ancestor." We may also have an explanation here of the close association between tonsillar sepsis and thyroid enlargements if both structures originated from segmental nephridia. Coincident enlargement of the thymus with that of the tonsils or of the thyroid is capable of a similar explanation. These glands have become ductless because of the modification of the appendages to which they were originally attached. This modification became necessary because the formation of a new alimentary tract, ventral to the nervous system, closed their original outlet to the surface. Further down the body in the Elasmobranch fishes there is an unpaired series of interrenal glands in close connexion with the kidneys. These were considered by Weldon to be derived from mesonephric tubules (later observers suggest they are pronephric), and by Swale Vincent to represent the cortical part of the adrenals. The Elasmobranch fishes also show a paired series of chromaffin glands derived from sympathetic ganglia. Higher in the scale these separate elements coalesce, and the main mass of the chromaffin glands forms the adrenal medulla, becoming surrounded by the interrenal tissue as the cortex. This striking union of two structures of such widely different origin as the cortex and medulla of the adrenals must have an important advantage because once it was achieved in evolution it was never dropped. While if it fails to occur in foetal life, the brain remains in the fish stage, the neopallium does not develop, and an anencephalic monster results. I would suggest that the success of an adrenal graft into the testis in Hurst's case of Addison's disease might have been due to its insertion into a tissue of similar origin, the adrenal cortex and the interstitial cells both arising from the Wolffian body, which is of nephridial origin.

It may be asked why the excretory functions, originally so uniformly distributed in segmental nephridia, have become restricted to such condensed structures as the kidneys. It is doubtful whether the tubules of the vertebrate kidney are strictly homologous with the nephridia of invertebrates although so similar in structure and function. Nephridia arise as invaginations from the exterior, and are, therefore, epiblastic—while their lumen is intracellular, i.e., is tunnelled through the cells. In the lower vertebrates the kidney tubules grow out from the body cavity and are therefore mesoblastic—while their lumen is intercellular.

Of the embryonic nephros which arises from the Wolffian body only the hindmost part, the metanephros, becomes the functioning excretory organ. The duct of the pronephros becomes the oviduct, and that of the mesonephros the vas deferens. This was formerly regarded as an annexation of kidney ducts by the gonads, but recent work points to the opposite conclusion, that the functional kidney has been split off from primitively gonadal tissue. In the higher vertebrates it is questionable whether the functional kidney is

formed from the Wolffian body, as, like nephridia, it arises by epiblastic invagination, but unlike them, the lumen of the tubules is intercellular and never opens into the body cavity.

All this points to three interesting and distinct phases in the evolution of the excretory system. First, the epiblastic intracellular nephridia of the invertebrates which open into the body cavity; secondly, the mesoblastic intercellular kidney tubules of the lower vertebrates shut off from the body cavity; thirdly, a return in the higher vertebrates to the process of epiblastic invagination but not to an intracellular duct nor to a communication with the body cavity.

Although this is not yet completely proven, I understand that the opinion of biologists at the present time is tending to accept this view. It is an extraordinary example of the way in which nature can change her plans according to circumstances, and can construct organs *de novo* which resemble the old ones closely enough to have deceived biologists for more than a generation. It shows how plastic is the evolutionary process, and that it is not so dependent on recapitulation as we thought. Further, it throws fresh light on the alliance of gonadal and excretory functions in the genito-urinary system. It offers fresh fields for exploration as to the relationship between the gonads and the adrenal cortex.

I wish to suggest that the influence of the endocrine glands on the kidney is in part, at least, the expression of the interest they continue to take, as it were, in functions which were formerly their own. Macallum<sup>1</sup> maintains that the earliest function of the kidney was not so much the elimination of waste products as the regulation of the inorganic composition of the blood. While nephridia opened into the body cavity the external medium of the sea and the internal medium naturally tended to be closely similar. But with the shutting off of the nephridia from the body cavity by the intrusion of a vascular tuft the animal was set free from the tyranny of its external medium, could individualize its metabolism, as it were, and change its habitat without altering the conditions under which its tissues and organs worked. This independence of external media is far more characteristic of the vertebrates than of invertebrates, and the change from nephridia to kidneys has thus been of special importance in the evolution of vertebrates. This independence is maintained even when a mammal returns to marine life, so that whereas the oceanic forms of invertebrate life tend to have the same saline concentration as the sea, the whale retains very much the blood salinity of the pig. The development of a circulation which enabled excretion to be adequate even when confined within a small compass was an important factor in rendering this change possible. When nephridia were replaced by a more adaptable mechanism and some of their outlets were dammed up by the formation of new structures such as the vertebrate alimentary tract and the pleural folds, they either had to vanish altogether or to take on some new function. To a large extent the second course was adopted and some of them became endocrine glands. The internal secretions may be looked upon as specializations of the old chemio-tactic mechanisms and they reveal their antiquity in the way they cling to vestigial structures. Not infrequently, when in the course of evolution a structure has become useless for its original purpose the endocrine system supplies it with a new tenant. This change of function is illustrated by the adrenal cortex, thyroid, thymus, pituitary and pineal glands. They remind us of the hermit crab that seizes on an empty whelk shell. And just as the

<sup>1</sup> Macallum, *Proc. Roy. Soc.*, 1910, Ser. B. lxxxii., pp. 602-624.



#### 4 Langdon Brown: *Influence of Endocrines on Work of Kidney*

hermit crab existed before it found an empty house, so the endocrine functions were in existence, in a less specialized form, before they had a local habitation. It is one of the merits of Gaskell's theory that it explains many of these points. All of these structures, except the pineal, represent modified nephridia. And I doubt whether the pineal is an endocrine structure at all, but should agree with Llewellys Barker that in disease it acts by disturbing the basal ganglia rather than by a direct secretory effect.

As we see vestigial remains of excretory structures in the endocrine glands, so there are vestiges of excretory methods in internal secretions. The active principle of thyroid secretion is an iodine compound of indol, which is a decomposition product of tryptophane, a constituent of the protein molecule. Indol is ordinarily split off by the intestinal bacteria, so that it looks as if we were dependent on these parasites, particularly *Bacillus coli*, for the first step in the preparation of an important hormone; one, indeed, which is essential to adult development. During the nursing period the infant gets its thyroid secretion from its mother's milk, and during that time the *Bacillus coli* is hardly ever present in its bowel. The suckling child has not a very active pancreatic secretion, and therefore cannot split off tryptophane very easily from protein and thus does not form indol to any extent.

This would fit in very well with Herter's views as to a form of infantilism due to persistence of the intestinal flora of the nursing period. It would appear that the body can not only detoxicate indol by converting some of it into indican, but can actually change it into a defensive substance. This would explain McCarrison's observations on the influence of intestinal intoxication on thyroid enlargement. It is reminiscent of the primitive excretory functions of the thyroid to find indol conjugated with iodine in its secretion, while indol conjugated with potassium sulphate continues to be excreted as indican by the kidney.

It is also interesting to note that tyramine, which is produced from the putrefaction of tyrosin in the human alimentary canal, is closely similar in chemical structure and action to adrenalin. It tempts one to refer the formation of adrenalin to the tyrosin of food proteins, just as thyroxin arises from tryptophane, another aromatic group in food protein. But it should be noted that the body is capable of making its own tyrosin, and that in alkaptonuria, a congenital error of metabolism, although the body is unable to utilize the tyrosin of the food, symptoms of hypo-adrenalism do not occur. Nevertheless, I anticipate that further research may show that other endocrine secretions are modifications of excretory products, just as endocrine glands are modified excretory structures. Other hints are to be found in such things as the diuretic effect of thyroid extract by dehydration of the body fat; the destruction of a purin body such as guanidine and the regulation of the calcium ions in the blood by the parathyroids. To sum up this part of my argument, Macallum's theory may be restated, that the principal function of the primitive nephridia was to bring the internal working of the organism into harmony with its environment. Now surely this is exactly what the endocrine system continues to do, both through its secretory activity and through its close association with the emotional, i.e., the sympathetic nervous system. In this way a further division of labour is achieved, the kidney being set free from the labour of forming an internal secretion, and able to devote itself entirely to excretion. But at the same time we should expect to find that the other internal secretions would be able to influence the work of the kidney in order to ensure harmony between the internal economy and its environment.

This is particularly well seen in the persistent relationship between the pituitary and the kidney. This is of biological interest because the pituitary represents a gland which was definitely excretory in function in invertebrates as high as the crustacea, and is of clinical interest as bearing on diabetes insipidus and polyuria in general.

It would generally be agreed that polyuria is due to one of the following causes :—

- (1) Increase of the quantity of fluid imbibed.
- (2) Increase in the molecular concentration of the urine, as in diabetes mellitus or after saline diuretics. More water is thereby attracted into the blood-stream from the tissues by osmotic pressure.
- (3) Incapacity of the kidney to excrete a concentrated urine, as in chronic interstitial nephritis.
- (4) High blood-pressure, which tends to force more blood through the renal vessels.
- (5) Dilatation of the kidney vessels, as produced by stimulating diuretics of the caffeine group.

In addition to these recognized causes, there is the condition of diabetes insipidus the cause of which to some extent is still a matter of dispute. I wish to discuss its pathology to see what light it throws on the work of the kidney in relation to polyuria in general. It is clear that several different conditions have been thus described. Erich Meyer, in 1905, described a type dependent on a primary defect in the kidneys, which are incapable of secreting urine of normal concentration, so that a much larger amount of water is needed to remove the ordinary products of metabolism. This would place the disease in the third category of my classification, but, personally, I should not include it in the category of diabetes insipidus at all. Meyer found that in this type 20 grm. of sodium chloride would cause a marked diuresis, whereas the normal kidney can respond by secreting a more concentrated urine. Yet in spite of diuresis the excretion of the salt would be delayed, even for days. This condition resembles interstitial nephritis in that particular, for the minute trace of albumin would be hardly recognizable in such a large amount of fluid.

Indeed the post-mortem records of some cases diagnosed as diabetes insipidus strongly suggest that the patient really suffered from chronic interstitial nephritis. When Saundby stated that in diabetes insipidus the cause of death might be a gradual destruction of kidney substance, producing uræmia, he was evidently describing this type of renal insufficiency.

Meyer introduced an interesting test for recognizing this condition. Theocin sodium acetate increases the permeability of the kidney, and when this permeability is diminished for solids, this drug, by facilitating their excretion, does away with the necessity for further dilution of the urine. Hence, though it ordinarily acts as a diuretic, here it merely raises the concentration of the urine without increasing the output of fluid. Now in the cases of ordinary diabetes insipidus in which I have tried this test, theocin sodium acetate did act as a diuretic; in one case the daily output rose from 5,000 c.c. to 8,000 c.c. as the result of giving 2 gr. of the drug twice a day. On the other hand, in a case of polyuria, which I concluded was one of Erich Meyer's type, the drug did not have a diuretic effect. The patient was a boy of seven and I investigated him five weeks after symptoms began. His output of salt was delayed though that of iodide was normal. His urea concentration was very poor, being 0.5 per cent. before the test and never rising above 0.6 per cent. afterwards. Yet the blood urea was normal, showing that with an output of 3,175 c.c. he was

## 6 Langdon Brown: *Influence of Endocrines on Work of Kidney*

able to prevent urea retention. There was generally a trace of albumin present in the urine. I re-investigated him three years later, and I shall have occasion to refer to some of the results later on. He was clearly suffering from renal insufficiency and as he had developed very little in the three years that elapsed between my two observations he recalled the type of renal infantilism described by Morley Fletcher, except that he had no rise of blood-pressure. Rabinowitch has shown that in ordinary diabetes insipidus, unlike chronic interstitial nephritis, the power of concentration is quite good for nitrogen. Which confirms the distinction I am drawing.

It will be realized that restriction of the ingested fluids is a futile and cruel procedure in the renal type of case. The condition of the kidney necessitates polyuria and if fluid is not given, then the patient must obtain it from his own tissues. He loses weight and the output of nitrogen rises, showing that the deprivation of water is producing a breakdown of tissues. This increased excretion of nitrogen in turn demands greater excretion of water. The appetite and general health will soon suffer, while the distress from thirst becomes extreme. An attempt to restrict fluids in this case, which was made before he came under my care, resulted in the boy drinking any dirty water he could find, and even his own urine.

It is well known that a syphilitic meningitis at the base of the brain can produce the symptoms of diabetes insipidus, especially in children. The Wassermann test is therefore imperative in every case, even when there are no stigmata of syphilis, congenital or acquired. An example of the latter cause in an adult came under my observation. A woman at the age of 40 married for the second time; two years later she suddenly began to pass 25 pints of urine a day. The Wassermann reaction was strongly positive. She improved on antisyphilitic treatment and valerian, but two years later the reaction was still strongly positive, although the quantity of urine had fallen to 9 pints. The thirst and polyuria were certainly better when she had valerian as well as mercury and iodide. This was before the days of treatment by pituitrin injections. Her tissues remained fat and flabby throughout, as is the case in some cases despite the high grade of polyuria. This is in itself suggestive of hypopituitarism. The nearer the meningitis is to the interpeduncular space, the more apt is it to excite diabetes insipidus, though this in itself does not enable us to decide whether the pituitary or the nervous tissue of the hypothalamus is really responsible.

Frank disease of the pituitary body may certainly be associated with persistent polyuria. The association of pituitary tumours with diabetes insipidus has been recognized since 1882. Primary optic atrophy, bitemporal hemianopsia and some form of ophthalmoplegia may occur in diabetes insipidus, pointing to the pressure in the region of the pituitary. In 1898 Bousfield reported three consecutive cases of diabetes insipidus with primary optic atrophy. In 1912 Frank recorded the case of a bullet-wound involving the posterior fossa which produced diabetes insipidus; he collected eighty-five cases of bitemporal hemianopsia, in eighteen of which this disease was present. Fractures at the base of the skull often excite a transient glycosuria and may induce a more prolonged polyuria. In a case of pituitary tumour operated on by Cushing, incomplete decompression was followed by unquenchable thirst, with polyuria reaching twelve litres a day. Several observers, among whom I may mention Bailey and Bremer, maintain that here as in the other group of cases, it is not the posterior lobe of the pituitary which is itself at fault, but that it acts by irritating the nervous tissues of the hypothalamus and tuber cinereum. Indeed

Bailey and Bremer regard the hypothalamus as an important head ganglion of the visceral nervous system, thus making the condition depend directly on the sympathetic.

It will therefore be convenient to consider next the cases of diabetes insipidus in which there is no evidence of syphilitic meningitis, pituitary disease or renal incapacity. The polyuria is then sometimes regarded as secondary to polydipsia. But in that case it ought by gradual and systematic reduction of the intake to be possible to reduce the output to normal, even though at the cost of much discomfort to the patient. This is not what occurs, for a point will be reached at which further restriction of the intake will not be followed by a fall in the output, which still remains abnormally high. Buttersack believes that when the unrestricted intake exceeds the output the condition is due to a primary polydipsia, but even here I have not found it possible to reduce the output to normal. In such cases I have found a negative Wassermann and a normal skiagram of the pituitary fossa. Theocin-sodium acetate did not reduce the output of urine, and salt was readily eliminated, though a saltless diet diminished thirst. Glycosuria excited by the injection of phloridzin appeared and disappeared in the normal time. Valerian diminished both thirst and polyuria. This is a point which can be determined by direct observation and we should not be deterred from the use of this drug because it has sometimes been used irrationally or because absurd explanations were formerly given of its mode of action.

The three types of diabetes insipidus due to syphilitic meningitis, frank pituitary disease and the so-called primary form, show remarkable resemblances to one another, and a sharp contrast with the renal incapacity type. It is therefore reasonable to suspect a common cause and to look for that cause in the pituitary. It is tempting to assume that even in the "primary" type there is some nervous or toxic disturbance of the pituitary when we know that drugs such as valerian, codein and pituitrin can help all three forms. Nervous ties between the medulla and the pituitary through the curiously indirect course provided by the sympathetic have been described by Cushing, Weed and Jacobson, but Dixon failed to find that any form of nerve stimulation caused any change in the secretion of the pituitary. Yet some structural or secretory change in the posterior lobe affords the most plausible explanation of diabetes insipidus, of the temporary polyuria after some head injuries and perhaps even of hysterical polyuria.

But are we to postulate an increased or diminished action of this lobe? The diuretic effect of pituitrin described by Schäfer and Magnus in 1901 has been proved by later observers to be quite transitory. Indeed, most of the earlier experiments must be discarded because allowance was not made for the complications introduced by anæsthetics. Farini in 1913 was the first to show that injections of pituitrin diminished polyuria and this effect has subsequently been repeatedly demonstrated in the normal man and in patients with diabetes insipidus.

That a toxic influence on the pituitary may produce diabetes insipidus is shown by the following case under the care of C. H. Miller. A young officer was admitted for diabetes insipidus following some pyrexial attack at Salonika. He passed about 590 oz. of urine a day and drank a corresponding amount of fluid. The Wassermann reaction was negative, and there were no signs of pituitary tumour. Miller noted a thrombotic condition of the saphenous vein over the internal malleolus which he had found more commonly in typhoid and paratyphoid fevers than in other conditions. The

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vein was hard, rigid and solid like a tendon, the same condition extending back along the branch communicating with the deep veins among the calf muscles. He therefore had a stool examined, with the result that paratyphoid B organisms were found in pure culture. He concluded that diabetes insipidus had been started by paratyphoid fever, the patient now being in the carrier stage of the disease. He then tried the effect of pituitary extract, and found that great improvement followed immediately. The urinary output fell to 200 oz. on each occasion. Miller's suggestion is that there was a partial interference with pituitary function as the result of paratyphoid. In some cases of diabetes insipidus the associated symptoms also point to hypopituitarism. Motzfeldt (*Endocrinology*, 1918, ii, p. 112) has reported three instances of this association, and the persistent obesity which I have several times seen in spite of profound diuresis in the disease seems to me to support this contention.

We may next inquire at what point pituitrin normally acts in its control of diuresis. It does not interfere with the absorption from the bowel, for as Priestley (*Journal of Physiology*, vol. lv, 1921, p. 305) points out, diarrhoea is not provoked as one would expect if large quantities of water remained in the bowel, particularly as pituitrin stimulates the intestinal musculature.

It does not act through the vasomotor system, since it is equally effective on the denervated kidney,<sup>1</sup> and no obvious effect on the renal circulation was noted by Priestley. Indeed, Priestley's experiments in which an injection of pituitrin delayed the onset of diuresis by four to six hours, during which the ingested water was stored up in the tissues while dyes such as phenol red could still be excreted in a concentrated form, point to some direct inhibitory action on the renal tissues. This view is also taken by Addis. Oehme (quoted by Priestley) expresses the action of pituitrin as inhibiting the sensitiveness of the kidney to hydræmic stimulus. I should agree with Rabinowitch in regarding diabetes insipidus as due to the lack of some internal secretion which normally regulates and moderates diuresis by acting on the cells of the kidney, and I look upon pituitrin as that secretion. The output of the pituitary may be diminished either by structural or toxic damage and possibly by nervous causes. In connexion with nervous factors I should like to refer again to the view of Bailey and Bremer that it is not the posterior lobe of the gland which is at fault but the nervous tissues of the hypothalamus. Surely it is extraordinary in that case if there is an anti-diuretic hormone produced in close anatomical relation to the hypothalamus yet having no physiological relation with it. Is it not more probable that any influence of the hypothalamus exerts itself through the pituitary? This would explain the apparent contradiction that while nervous factors obviously influence diuresis, the effects I have been discussing can be demonstrated in the denervated kidney. It would be but one more example of the influence of the involuntary nervous system upon the endocrine system.

It seems to me that we can express the situation most concisely by saying that *pituitrin regulates the threshold of the kidney for water*. This would account for the apparently aberrant instances of a diuretic effect of pituitrin. It would also explain its selective action on water while not hindering the output, for instance, of dyes.

The materials excreted by the kidney fall into two categories, according to Ambard: (1) Those which are purely waste products and useless to cellular life, such as urea, ammonia, and uric acid. There is no threshold for their

<sup>1</sup> Bailey and Bremer, *Arch. Int. Med.*, 1921, xxviii, p. 773.

excretion. Most drugs are excreted in this way. (2) Those which may play a useful part in cellular life, such as sugar, sodium chloride, hæmoglobin and water. The kidney interposes a threshold in the way of these, so that they only pass into the urine when their level in the blood exceeds this barrier. But the barrier is not kept at a constant level. The variations in the height of the threshold is one of the ways in which the kidney adapts itself to contingencies. Water is clearly a substance of value to cellular life. So is sugar, and all recent work on glycosuria has served to emphasize the importance of the variable threshold in this condition. Thus we have in hyperthyroidism a clear instance of an endocrine disturbance, which leads to a marked rise of the threshold for sugar. I should regard a high blood sugar curve without glycosuria as typical of hyperthyroidism. Indeed, the word "threshold" conveys a very inadequate idea of the situation. A sluice, which can be raised or lowered as required, is a far more exact simile. And this sluice can apparently be controlled through certain endocrines. It can be made to vary for one urinary constituent without involving another. And thus the endocrines can influence the work of the kidneys in a subtle way. It has been suggested also that the calcium drainage in the ketosis of diabetes is due to failure of the parathyroids, which coöperate in some respects with the pancreas, but I do not know of any evidence that this is due to an alteration of the kidney threshold for calcium.

But the matter is not without its difficulties even on this view. Thus the experimental evidence as to the relation between the pituitary gland and the cerebro-spinal fluid is somewhat contradictory. Why should lumbar puncture produce such a striking effect for a time in diabetes insipidus? We have all seen it, and Herrick<sup>1</sup> in one instance reduced the urinary output from 11,000 c.c. to 600 c.c. by merely withdrawing 5 c.c. of cerebro-spinal fluid in this way. Is it possibly due to relief of pressure on the hypothalamus? One can hardly suppose such a slight relief of pressure on the pituitary itself could produce such an effect. Dixon<sup>2</sup> has shown that pituitrin normally passes into the cerebro-spinal fluid, yet Marenon and Gutierrez,<sup>3</sup> while agreeing that pituitrin is antidiuretic, failed to find any evidence of an oliguric hormone passing into the cerebro-spinal fluid. In this connexion it is interesting to recall that Douglas Cow found the diuretic effect of water absorbed from the bowel to be greater than if it were injected subcutaneously. On the theory here adopted this would suggest that something absorbed from the bowel with the water diminished the output of pituitrin—a sort of antiseoretin. But Dixon in repeating Cow's experiment found that a boiled and filtered extract of intestinal mucosa actually caused a secretion of pituitrin into the cerebro-spinal fluid after a latent period of one to two hours. It is really very difficult to reconcile all these discrepant results. Pituitrin is certainly antidiuretic under clinical conditions; it appears to pass into the cerebro-spinal fluid, yet no oliguric hormone is found there. Water absorbed from the bowel is more diuretic than water injected subcutaneously, yet a substance can be extracted from the intestinal mucosa which actually increases pituitary secretion in something over an hour, whereas by hypothesis the secretion of the gland should be diminished. Evidently the experimental evidence requires repetition. Kennaway and Mottram have postulated the presence of both

<sup>1</sup> *Arch. Int. Med.*, 1912, x, p. 1.

<sup>2</sup> *Journ. Physiol.*, 1923, lvii, p. 129.

<sup>3</sup> Quoted by Hall, *Amer. Journ. Med. Sci.*, 1923, cxlv, p. 560.

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diuretic and anti-diuretic principles in the pituitary. I think it safer merely to say that the pituitary regulates the output of water by some direct effect on the kidney cells, and that at present we do not know exactly how it strikes a balance between the water of the blood and urine. When present in considerable excess its oliguric effect is certainly the predominant one. On the current theory of urinary excretion, this action of pituitrin would promote re-absorption of water by the renal tubules, thus concentrating the urine.

It is quite conceivable that when the output of water tends to fall a small quantity of pituitrin may stimulate the glomeruli of the kidney, causing mild diuresis, but that when the output is greater a larger dose of pituitrin may stimulate the tubules, thus causing them to concentrate by re-absorption. Such reversible actions are familiar in the case of catalysts. But this is merely speculation in the present state of our knowledge.

Finally, I should like to point out that a large dose of pituitrin has an anti-diuretic effect not only on the normal subject and the patient with diabetes insipidus but also on the Erich Meyer type of renal insufficiency. I have already said that in this type the concentrating power of the kidney is impaired for solids, which is not the case in ordinary diabetes insipidus. A dilute urine is necessary, yet here, too, pituitrin can hold up the output of water. Thus, in the example of this type I have already referred to, Graham and I found that although the injection of  $\frac{1}{2}$  c.c. of pituitrin had no effect on diuresis 1 c.c. would check it for sixteen hours. Three years later we repeated the observation with a similar result. It was recently stated by Blumgart that intranasal spraying of pituitrin would produce a similar effect on diuresis to that produced by intramuscular injection. I have certainly found this an effective way of relieving the headaches of hypopituitarism. We therefore tried the spraying method in this boy's case. An antidiuretic effect was obtained, though not so marked as that obtained by injection. On a fixed intake of 2,000 c.c. there was an output of 4,300 c.c. without any pituitrin. By spraying this was reduced to 2,850 c.c., while after injection it fell to 2,250 c.c. The conditions were not perhaps strictly comparable. For the nasal method one uses a solution of pituitrin only half the ordinary strength, and it is not practicable to administer 2 c.c. of this, intranasally, at a single sitting. Half a c.c. was therefore given three times a day. Now it had previously been shown in this case that 1 c.c. in a single injection produced an effect, whereas  $\frac{1}{2}$  c.c. would not. It comes to this: the equivalent of  $\frac{3}{4}$  c.c. of ordinary pituitrin given in three doses intranasally reduced the output by 33 per cent., (i.e., at the rate of 44 per cent. per c.c.), while 1 c.c. given intramuscularly reduced it by 48 per cent. This supports the efficacy of the intranasal method. But it also shows that an adequate dose of pituitrin controls excessive diuresis whether the pituitary is or is not at fault, and therefore does not really lend support to the theory of pituitary deficiency as the cause of diabetes insipidus.

Nevertheless, on a review of the whole evidence, I would suggest that so-called idiopathic diabetes insipidus may be due to some affection of the hypothalamus which interferes with the normal control of the threshold of the kidney for water, not through the vasomotor system but through the pituitary gland.

I would summarize my conclusions thus: Claude Bernard urged that the object of all vital mechanisms was to keep the internal environment constant. We have seen that nephridia originally were the principal means of maintain-

ing a constant relationship between the internal and external environments. In evolution nephridia were replaced by analogous but probably not homologous tubules which form the kidney, the function of which may be defined as that of keeping the chemical composition of the blood constant. Some nephridia were retained and modified to house the hormones, which, previously less specialized and more widely distributed, now play an important part in maintaining the internal environment. Haldane will not allow that they are to be regarded as something *sui generis*. He says "The truth is that every substance which enters into the life processes of any part of an organism is as much a hormone as any other such substance. Water, for instance, is the most abundant constituent of the body, and a very minute excess in the diffusion pressure of water in the blood excites very striking reaction in the kidney."<sup>1</sup> But while it is, of course, true that to a certain extent every product of cell activity has an effect on every other tissue, I think it is, nevertheless, convenient to place in a separate category substances having such specialized actions as the recognized hormones.

Not only morphologically but physiologically these modified nephridia recall their origin. The last of them to retain excretory functions, the pituitary, still shows its association with such functions by the profound modification it is able to exert over the threshold of the kidney for water. It is probable that diabetes insipidus is due to a loss of this control, and it is possible that the overlying nervous structures influence the matter through the pituitary; they certainly can do so without the intervention of the vasomotor system.

<sup>1</sup> "Respiration," 1922, p. 387.





## Section of Urology.

President—Dr. W. LANGDON BROWN.

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### The Mechanism of Prostatic Obstruction.

By J. SWIFT JOLY, F.R.C.S.

I HAVE recently expended a good deal of time over the problem of how and why an enlarged prostate prevents a patient from emptying his bladder, and I must confess that the more I consider the subject, the more complicated it appears. I am therefore bringing forward a theory, first in the hope of explaining my own conception of the mechanism of this type of retention, and secondly with the idea of inviting criticism, and learning the opinions of other workers in urology.

#### OLD THEORIES.

It would be easy to spend the whole of my time discussing the numerous theories that have been put forward to explain the phenomenon of prostatic retention, but they may briefly be grouped under four headings: (1) A primary lesion in the musculature of the bladder, in which case the enlargement of the gland only plays a secondary part. (2) Changes in the bladder sphincter due either to irritation or to anatomical disturbances of the parts produced by enlargement of the prostate. According to this theory spasm of the sphincter is supposed to be the prime cause of the retention. (3) Mechanical obstruction due to the enlarged prostate. The enlarged middle lobe was supposed to play a predominant part in obstructing the urethra, and I believe its rôle is comparatively insignificant. (4) Two or more of the above causes acting in conjunction with each other.

The first theory was propounded by Guyon to explain the fact that the amount of obstruction bears no relationship to the size of the prostate. As in some cases a very small prostate is accompanied by complete retention, while in others, where the gland is enormous, there is practically no residual urine, he considered that the retention was not due to the prostate, but to an arteriosclerosis of the bladder wall itself. Keyes [1] considers that the true cause of chronic retention lies in the bladder muscle, while that of acute retention is due either to congestion or spasm. Marion explains residual urine in a similar manner. He considers that the bladder muscle becomes fatigued in its struggle against the mechanical obstruction, and is therefore unable to force out all the urine. The good results of prostatectomy, and the fact that manometric observations show that the intravesical pressure is not reduced, both tend to disprove this hypothesis.

Li Virghi [2] is the principal exponent of the second theory. He believes that the growth of the prostate irritates the sphincter, and throws it into a state of spasm. In the earlier stages of the disease, the spasm relaxes

sufficiently to allow the urine to escape completely, though in a diminished stream: later on the relaxation only occurs during part of the time necessary for micturition, and the patient retains a portion of the contents of the bladder; in the last stage, the spasm is permanent, and the patient is in a state of complete retention. He goes so far as to say that if the sphincter of the bladder could alone be destroyed by a special operation, in cases of prostatic hypertrophy, one could, in a number of cases, dispense with a total prostatectomy in order to obtain a disappearance of the retention. He also considers that the good results obtained by prostatectomy are not due so much to the removal of the gland as to destruction of the sphincter. However, such a long continued spasm as Li Virghi invokes must inevitably give rise to secondary changes in the sphincter, and they, as far as I know, have never been described.

I believe that the theory of mechanical obstruction is the true explanation of prostatic retention, but I feel that it is a mistake to attribute the blame to the middle lobe, which has been supposed to act as a ball valve. This does not explain retention in cases where no middle lobe is present, nor does the type of retention found in prostatic cases resemble what one would expect if the middle lobe acted as a ball valve. If it acted so, we must assume that it is swept into the urethral orifice by the force of the urinary stream. If this was the case, the first part of the act of micturition would be perfectly normal, but sooner or later the stream would be suddenly interrupted, and the interruptions would vary in number and intensity in a marked degree.

The best example of a "ball-valve" type of retention I have observed, occurred in a young man of 32. He had a firm compact papilloma, about the size of a cherry, which was attached by means of a long, thin, and very flexible stalk to the bladder wall, about a centimetre from the internal meatus. This growth was freely movable, and was often swept by the flow of urine against the internal meatus, and blocked the urethra completely. This patient had a most unusual type of micturition, and perhaps the most characteristic feature about it was its extreme variability. No two acts were exactly similar. For instance, a perfectly normal act of micturition might be followed by one in which the stream was interrupted two or three times. These interruptions were sudden and painful, and often the growth remained impacted in the urethra, and gave rise either to partial or complete retention. The amount of retained urine varied enormously. Occasionally there was none at all, but as a rule it varied from  $\frac{1}{4}$  oz. to 12 oz. The attacks of complete retention were also variable. He had as many as two in one day, perhaps separated by one or more acts of spontaneous micturition, but usually they occurred about twice a week. In every case the retention necessitated the passage of a catheter. The urinary stream was good and forcible. There was no diminution of its calibre, and no hesitation; in fact this patient could pass urine freely and easily, or not at all. All these symptoms were relieved by removal of the growth.

Thus we see that in almost every respect, the type of micturition in cases of "ball valve" retention differs from that observed in prostatic obstruction.

Both Legueu and Marion believe that prostatic retention is due to a combination of causes. Marion [3] considers that in the case of minute prostates the cause of the retention lies in the bladder sphincter, and he makes a point of removing it with the prostate. In cases of larger adenomata, he admits mechanical obstruction, and explains partial retention on the supposition that the detrusor becomes fatigued, and is no longer able to overcome the obstruction completely. He considers that complete retention is due to mechanical obstruction in cases where it is chronic, and to congestion if it is transitory. Legueu [4] invokes spasm or inextensibility of the bladder neck, which he considers acts in conjunction with the mechanical obstruction of

the enlarged gland, but is unable to explain the phenomenon of partial retention.

From this very brief *résumé*, it will be seen that there is no settled consensus of opinion as to the cause of prostatic obstruction.

#### AUTHOR'S THEORY.

This theory depends upon two conditions being present. The first, that there is an intravesical projection of the prostate, or that the growth of the gland has raised the internal orifice of the urethra above the level of the surrounding parts of the bladder base. The second, that the tissues of the neck of the bladder, and of the surrounding parts, are soft and free from infiltration, and therefore their relative position can be modified to a slight degree by pressure. If these postulates are granted, I think that the mechanism of prostatic obstruction can be explained by the ordinary laws of hydrostatics. It is obvious that the second postulate excludes all cases of prostatic carcinoma, and many of fibrous prostatitis. In both these conditions the internal meatus and the upper part of the prostatic urethra are narrow and inextensible, and the retention is due in a great measure to this stenosis. In fact it resembles the type of retention occurring in cases of urethral stricture, more than that produced by the ordinary adenomatous enlargement of the prostate. The latter is the only type of retention I propose to discuss in this paper.

The bladder, like all other hollow muscular organs, exerts a definite pressure on its contents. This is due to the tone of its muscle fibres. The reaction to this pressure, if we exclude the force of gravity, is transmitted equally in all directions. Thus, each unit of area of the bladder wall sustains an equal amount of pressure, and this pressure is exerted at right angles to the plane of the wall at that particular spot.

Now, supposing there is an intravesical projection of the prostate, and for sake of simplicity let us first of all assume that this projection has the shape of a regular cone, with the internal meatus at its apex. (I should mention that in this paper I am using the term "internal meatus" to mean the highest point at which the walls of the urethra lie in apposition. This is not necessarily the level of the sphincter vesicæ. As the prostate enlarges, it may press the whole floor of the bladder upwards, without dilating the sphincter, but as a rule the sphincter is dilated, and it then only surrounds the base of the intravesical projection. In the former case the sphincter lies at the level of the internal meatus, in the latter it may lie far below it.) However, to return to our cone, the intravesical pressure acts at right angles to its surface, and is therefore directed downwards and inwards towards its axis, that is towards the urethra. This pressure can be resolved into two components, a horizontal and a vertical. The former acts at right angles to the course of the urethra, and the latter parallel to it. These two components must be considered separately.

The horizontal component is directed from all sides towards the upper part of the urethra, and has the tendency to close this part of the tube. In fact it has exactly the same effect as if an elastic band was stretched round the intravesical projection of the prostate, and the amount of compression that the urethra sustains is directly proportional to the tension and the thickness of the band, and inversely proportional to the resistance of the prostatic tissue to distortion. In this simile, the tension of the india-rubber band corresponds to the amount of the horizontal component per unit of effective area, and its thickness to the size of that area. Thus, the constricting force depends on two factors: (1) The intensity of the intravesical pressure as measured by a mano-

meter, and (2) the size of the intravesical projection of the prostate. From this it will be seen that straining, or any other cause which increases the intravesical pressure, tends to occlude the urethra, and it is only when the pressure is diminished by relaxing the muscles that the urine flows freely.

If, as we have assumed above, the intravesical projection is cone-shaped, the total constricting force can be easily calculated. It is  $P \pi R H$ , where  $P$  is the intravesical pressure as measured by means of a manometer,  $H$  is the height of the cone, and  $R$  the radius of its base. From this it will be seen that for a given intravesical pressure, the amount of the force constricting the urethra varies directly with the height of the prostatic projection, and also with the diameter of its base, but I hope to show that the former of these two factors (i.e., the height) is the more important. The constricting force is opposed by the resistance to compression and distortion of the prostatic tissue itself, and I think it legitimate to assume that the resistance increases as the thickness of the tissues is augmented. Now if we compare two cones with bases of equal diameter, but one of which is twice as high as the other, the constricting force in the case of the higher cone is double that of the lower; and as the mean thickness of the tissues is the same in both cases, the loss due to resistance is the same for both. Again, supposing both cones are of the same height, but the base of one has twice the diameter of the other, the constricting force is double in the case of the broad based cone, but as the mean thickness of the tissues is also double, a greater proportion of the force is lost in overcoming the resistance. From this I argue that in considering the effectiveness of the horizontal component of the pressure on the intravesical projection of any prostate, the height of the projection is more important than its diameter. As the prostate increases in size, and as its intravesical portion projects higher and higher into the bladder, the constricting force also increases, until, sooner or later, the walls of the upper portion of the prostatic urethra are so tightly pressed together that no urine can pass through it. When this happens the patient has complete retention, and the prostate acts as a "non-return" valve, which cannot be opened by any effort on his part.

It may, however, be argued that I am only dealing with a special type of intravesical projection, a type that is comparatively uncommon, and that what is true concerning this particular type is not necessarily true about others. I shall therefore endeavour to show that there is a tendency for the urethra to be constricted in all types, but that the amount of the constricting force varies according to the form of the intravesical projection of the prostate.

First let us take the case where the intravesical projection is still cone-shaped, but the urethra instead of opening at its apex, opens at the bottom of a crater-like hollow. I shall make use of the simile of a volcano to make my meaning clear. In a volcano, the depth of the crater may be equal to the height of the cone, but usually it is much less. In the latter case we must consider the portion of the cone below the level of the floor of the crater apart from the portion above this level. In the lower portion of the cone, the pressure of the fluid is directed from all sides inwards towards the urethra, and is uncompensated save for the resistance of the tissues. In the upper portion, that is, above the floor of the crater, the constricting force is partially balanced by the pressure of the fluid within the crater itself. This acts in an outward direction, and tends to dilate the urethra. However, as the surface area of the crater is always less than that of the corresponding portion of the cone, the constricting force is the greater of the two. Even in the extreme case

where the floor of the crater descends to the base of the cone, this is true, and if we assume that the wall of the crater slopes at the same angle as that of the outer wall of the cone, it is possible to show mathematically that the constricting force is three times that of the dilating force.

The most common form of the intravesical projection of an enlarged prostate consists of rounded elevations of both lateral lobes, with or without a middle lobe lying between and behind them. These lobes are separated by deep clefts. In the case where the enlargement is confined to the lateral lobes, these clefts are two in number, and occupy the middle line in front of, and behind the internal meatus. If all three lobes are hypertrophied, there are three clefts, one anterior, and two postero-lateral. The internal meatus is not round, as I have hitherto assumed it to be, but is an antero-posterior slit in cases where the lateral lobes alone are hypertrophied, or an elongated triangle, with its base placed posteriorly, when a middle lobe is also present. The prostatic urethra is flattened from side to side, and forms a scabbard-like slit between the lateral lobes. Both these types of prostatic enlargement can give rise to complete retention, but I shall first discuss the type in which all three lobes are hypertrophied. The middle lobe is usually the highest point of the projection, while the lateral lobes form ridges extending forwards from it. These ridges end abruptly in front, and are separated from each other by a deep cleft, which usually extends right down to the floor of the bladder. The middle is separated from each of the lateral lobes by a cleft, which, however, is rarely as deep as that between the lateral lobes.

In every case where there is marked retention, the lateral lobes will be found to lie in contact with each other up to a point considerably above the floor of the bladder. This can only be appreciated when there is no instrument in the urethra, and observations on the shape or form of an intravesical projection should be made, not through the cystoscope, but by direct inspection or palpation after the bladder has been opened. Now the effect of the hydrostatic pressure is to compress the whole of the intravesical projection. The lateral lobes are pressed up against each other, and the base of the middle lobe is wedged in between their posterior extremities. This means that the sides of the various clefts are pressed together, and fluid is effectually prevented from trickling through them. In fact, the greater the pressure, the more effectually they are closed. In this respect they resemble the gates of a canal lock. These gates point up stream, and can only be opened when the water pressure on both sides is the same. As long as the pressure on the upper side is greater than that on the lower they remain shut, and the greater the difference of pressure the more tightly they are closed. Thus any cleft the sides of which naturally lie together may be considered as non-existent. However, no cleft is completely "closed;" there is always a distinct V-shaped fissure at its upper extremity. This I call the "open" portion of the cleft. Now the effectiveness of an intravesical projection in causing retention depends on the height of the closed portions of the clefts. The portion of the prostate above this level may be ignored, as in it the constricting and dilating forces are equal and opposite. This is most easily seen if we consider a simple bilobed gland. If the lobes lie together, that is, if the clefts are closed, there is a tendency towards retention. The most effective pressure in this case is directed from either side inwards towards the middle line, and the urethra is squeezed as if it was caught in a vice. The reason being that the intravesical projection is oval, with its long axis directed from before backwards. If, however, the clefts between the lobes remain open right down to the floor of the bladder, then, no matter how large

the prostate may be, there will be no retention and no residual urine. In this case the lateral lobes resemble a roof with a double span, and the outward pressure acting on the sides of the central valley exactly counterbalances the inward pressure on the outer side of each span. The lobes of the prostate are compressed but the urethra is not constricted. This is one of the reasons why a very large prostate may give rise to little or no retention, and as a matter of fact, I find there is a greater tendency for the clefts to be open in large rather than in small prostates. It also follows from this argument that if the middle lobe was cut off at the level of the upper limit of the closed portions of the clefts, there would be no alteration in the amount of retention present, but if on the other hand, it was removed by a V-shaped incision, in such a manner as to leave an open cleft between the lateral lobes, the amount of retention would be materially reduced. In fact, it would be reduced to zero if the open cleft extended down to the floor of the bladder. The same idea underlies both Bottini's operation and "forage" of the prostate. If by either of these methods an open cleft is established which extends down to the floor of the bladder the retention will be relieved, but no matter how this cleft is made, it soon becomes closed again because the continued growth of the lateral lobes brings them once more into contact. One cannot guarantee a permanent result from any of these operations so long as portions of the prostate, which can grow and obliterate the gap, are left behind.

#### RESIDUAL URINE.

No theory of the mechanism of prostatic retention is complete or convincing unless it also explains incomplete retention. When considering the phenomenon of residual urine, we must take into account the effect of the vertical component of the intravesical pressure on the enlarged prostate. Fortunately the action of this component is not so complicated as that of the horizontal. It simply acts as if a weight was placed on the top of the intravesical projection, which tends to compress the gland and makes its upward projection less prominent than it would be if the bladder were empty. In other words, the greater the intravesical pressure the less the prostate projects into the bladder, and as the pressure is relieved by draining away the fluid, the intravesical projection rises higher and higher. I have noticed this phenomenon time and again and it can be demonstrated during almost any prostatectomy. The method I adopt is as follows: A small incision is made into the bladder, which has previously been distended with lotion, and into this opening the finger is immediately inserted. If this is done rapidly, not more than a few drachms of fluid will escape. The intravesical projection of the prostate can then be palpated. If a catheter is introduced, or if the fluid is allowed to escape alongside the finger, the projection will be found to grow steadily larger as the bladder becomes emptied. This is most noticeable in cases where the gland is moderately enlarged. In many cases the lateral lobes do not appear to project at all when the bladder is distended, but form definite rounded elevations when it is empty. This point can also be demonstrated by means of cystograms. Three exposures should be made, the first with the bladder completely filled with opaque solution, the second when it is half empty, and the third with just sufficient fluid to outline the prostate. In taking these cystograms, care must be taken that the position of the patient relative to that of the X-ray tube is not changed between the exposures. On comparing the plates it will be found that there is a definite elevation of the internal meatus as the bladder is emptied, and also that the prostatic lobes at the same time become more prominent.

The effect of this increase in the height of the intravesical projection is to impose a greater obstruction to the outflow of urine as the bladder empties. It may be argued that as the contents of the bladder are evacuated the pressure in it is reduced, and that this reduction must also be taken into account. However, this is not the case. The truth of this statement will be realized once the total effect of the two components are compared. I have already shown that the horizontal component, the constricting force, is  $P \pi H R$ , where  $P$  is the intravesical pressure,  $H$  the height of the intravesical projection, and  $R$  the radius of its base. The vertical component, on the other hand, is independent of the height of the prostatic projection, but is proportional to the area of its base. If, as we have assumed, the base is circular, this component is equal to  $P \pi R^2$ . These two components are to a certain extent antagonistic. The horizontal constricts the urethra and is the prime cause of the obstruction, while the vertical, in that it diminishes the height of the prostatic elevation, diminishes the value of the horizontal component. The amount of obstruction in any case, therefore, depends on the ratio of these two components to each other, or in other words on the ratio  $H/R$ . Now during micturition there is no change in the value of  $R$ , as the area of the base of the intravesical projection does not alter, but  $H$  steadily increases, therefore the ratio  $H/R$  becomes greater and greater as the bladder empties. This means that the resistance to the flow of urine increases during the act, and if it becomes so great that the stream is completely stopped before the bladder has been emptied, the patient will then have residual urine. It is well known that the amount of residual urine may remain unchanged for considerable periods of time. I have watched cases where it has not varied more than a few cubic centimetres in eighteen months. This simply means that the ratio  $H/R$  has not altered during the time. The increase in the amount of residual urine caused by congestion, &c., merely indicates that the ratio has been altered by the temporary increase in the height of the intravesical projection.

#### THE DISPARITY BETWEEN THE SIZE OF THE PROSTATE AND THE AMOUNT OF RETENTION CAUSED BY IT.

It is well known that the amount of retention is not proportional to the size of the prostate. A tiny gland may give rise to complete retention, while some enormous prostates are only accompanied by a slight amount of residual urine. The discrepancy is at once explained by this theory. The amount of retention is not proportional to the size of the gland, nor even to the size of its intravesical projection, but to the ratio  $H/R$ . The greater this ratio, the more marked the retention becomes. Now if we compare once more the intravesical projection to a cone, the ratio  $H/R$  is simply the tangent of its basal angle and the measure of the steepness of its sides. Therefore the amount of retention is proportional to the steepness of the prostatic elevation. The more abruptly it rises into the bladder the greater the obstruction, and the more gradual its slope the less the amount of retention becomes.

In cases where a small prostate gives rise to marked obstruction, I have invariably found that its intravesical projection rises almost vertically from the base of the bladder and forms a thin-walled cuff surrounding the urethra. It may only be a few millimetres high, and as its base is also small these tiny projections have usually been considered insufficient to account for the symptoms. However, this type of projection forms a remarkably efficient valve and effectually prevents the passage of urine. In fact it resembles the



ileo-cæcal valve, which is formed by a partial invagination of the small intestine into the cæcum. The invaginated portion forms a cuff surrounding the orifice. I have been so much impressed by the efficiency of a valve of this nature that I have endeavoured to copy it when implanting a divided ureter into the bladder. The ureteric stump is sutured into the wound in the bladder wall at a point about a centimetre from its lower end. The terminal portion projects freely into the bladder and is not anchored in any way.

If the prostate is large, it is often found that the intravesical projection is comparatively small as compared with the size of the whole gland, and that it rises from the bladder base in a gentle slope. In such cases the amount of retention is slight, as would be predicted by this theory. Again, the greater part of a large intravesical projection may be ineffective. In these large glands there is a tendency for the clefts between the lobes to remain "open" for the greater part of their length. This applies especially to the anterior cleft between the lateral lobes. Now it is only the lower portion of such a projection that has any influence in causing obstruction, and in this type of gland also, the amount of retention would be slight. I therefore feel that this theory explains the disparity between the size of the prostate and the amount of retention it causes.

#### CONCLUSIONS.

- (1) That the urinary pressure acting on the intravesical projection of an enlarged prostate tends to constrict the urethra.
- (2) That the height of the projection is more important in this respect than the size of its base.
- (3) That when the bladder is full, and the urinary pressure at its maximum, the intravesical projection is not so prominent as when it is empty.
- (4) That residual urine is caused by the ascent of the intravesical projection during micturition, and when this reaches a certain height, the pressure constricts the urethra, and prevents the bladder from being completely emptied.
- (5) That the amount of obstruction does not depend on the size of the prostate, but on the proportion that the height of the intravesical projection bears to the radius of its base.

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## Section of Urology.

President—Dr. W. LANGDON BROWN.

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### Case of Cyst of the Prostate.

By Sir JOHN THOMSON-WALKER, F.R.C.S.

A. J. L., AGED 46, complained of difficulty in micturition of two years' duration. The onset was gradual, but the symptoms had been very severe for the past fourteen days. Micturition commenced freely and then suddenly stopped "with a jerk," and this was followed by intense cramping pain and great straining, during which a small quantity of urine was passed in drops. The urine he passed was clear, acid and contained no shreds, pus, albumin nor blood.

There was no history of venereal disease. He was a healthy, well-built man, without signs of disease of the nervous system or elsewhere.

The prostate, on rectal examination was found to be normal in size, elastic and movable, and the seminal vesicles could not be felt.

A No. 8 gum elastic prostatic catheter was passed into the bladder but some obstruction was detected at the internal meatus. There were ten ounces of residual urine.

*Cystoscopy* (under local anæsthesia).—On the anterior lip of the internal meatus, there was a rounded projection overlying the orifice. It was about the size of a large cherry, sessile, having an attachment equal to about one-third of its circumference, semi-translucent, smooth on the surface and with numerous vessels coursing over it. The remaining part of the circumference of the internal meatus showed nothing abnormal. The trigone was much hypertrophied. The bladder-wall showed slight trabeculation and the ureteric orifices were normal.

The cyst was incised with the high frequency cautery and rapidly collapsed, the cystoscopic field being clouded with its contents.

The patient was at once able to pass with complete freedom the fluid left in the bladder.

On cystoscopic examination four days later, the anterior lip of the internal meatus was slightly prominent and there was a grey slough on the mucous membrane.

The relief he obtained from the incision of the cyst was immediate, and there has been no further difficulty.

I described a case of cyst of the prostate, and showed the specimen of the prostate containing the cyst after enucleation by suprapubic cystotomy, at this Section in 1923 (*Proc. Roy. Soc. Med.*, 1923, xvi, Section of Urology, p. 31).

## 22 Thomson-Walker : *Cyst of Prostate; Eccles: Perirenal Lipoma*

At that time I did not know of any cases diagnosed with the cystoscope. I have since found an excellent description of cysts of the prostate by Young, of Baltimore, in "Keen's Surgery," vol. viii, 1921. Ten cases were collected, four of which were observed by Young himself. In six cases the cyst was removed through a suprapubic cystotomy, in one through a perineal incision. In three cases the cyst was destroyed by cystoscopic operation, two being removed by Young's cystoscopic rongeur and one opened by the high frequency cautery. The second case I have recorded is therefore the second case dealt with in this way.

Mr. Nitch recorded, at a subsequent meeting of this Section of Urology, a case of cyst of the prostate arising from the posterior lip of the internal meatus, which he removed by suprapubic cystotomy. In this case obstruction recurred and a portion of the prostate was removed at a subsequent operation.

### A Perirenal Lipoma, with (?) Myxo-sarcomatous Changes in one portion.

By W. McADAM ECCLES, M.S.

THE specimen was removed from a woman, aged 57, who had noticed three months before operation that the right side of her abdomen was beginning to increase in size. The enlargement had been more rapid in the last fortnight.

She had never had any increased frequency of micturition, and there had never been any blood in her urine.

She was stated to have lost several pounds in weight since the appearance of the tumour.

The swelling occupied the whole of the right loin, and was smooth on the surface and elastic. It could be moved freely from side to side, but not much from above downwards.

A long incision in the right linea semilunaris exposed a retroperitoneal tumour, apparently wholly fatty. After incision of the parietal peritoneum and the pushing over of the ascending colon, the whole mass was fairly easily enucleated. The kidney could not be seen at first, but when the lower pole was exposed it was decided that it would not be possible to leave it behind, consequently it was removed with the main mass. The patient made a good recovery.

The tumour weighed  $6\frac{1}{2}$  lb. It will be seen that the bulk of the mass is clearly lipomatous in nature, but a part at the lower end is different and resembles myxomatous tissue, with considerable hæmorrhagic areas. On microscopic section the upper part is seen to be a typical lipoma, but this lower portion has the characteristics of a myxo-sarcoma. There is no evidence of any disease of the kidney, and there is no interference with the functions of the ureter.

## Cholesterin Stones, (?) Gall-stones, Removed from the Urinary Bladder.

By S. G. MACDONALD, F.R.C.S.

THE exhibit consists of: (1) Complete faceted stones passed by patient; (2) fragmented stones removed by litholapaxy. The patient is a spinster, aged 76. In 1914 she had an attack of sudden abdominal pain diagnosed by several doctors as appendicitis. It subsided, and no operation was done. In 1918 the patient began to have attacks of painful retention of urine, followed by and relieved by the passage of a stone. The three stones shown were passed in this way. These attacks continued for two years, during which over 100 stones were passed; they then ceased entirely. I saw the patient with Dr. Melandri in October, 1923, at which time she was suffering from great frequency of micturition and dysuria. The urine was purulent, but there was no hæmaturia. Cystoscopy revealed a nest of stones, which I crushed and evacuated. The cystitis has cleared up entirely, and the patient has been quite well since. Cystoscopy subsequent to the litholapaxy did not reveal any abnormal opening in the bladder, and both ureteric orifices were normal in size. The patient has never had renal colic, biliary colic or jaundice, and no stones have been passed *per rectum*, so far as is known. No gas or faecal material has ever been passed *per urethram*, and as far as can be ascertained the urine has not been bile-stained. The patient is very definite in her statement that, apart from the attack of "appendicitis" and the attacks of painful retention, she has never had any abdominal pain at any time. On abdominal examination there is found to be tenderness on pressure anteriorly in the region of the gall bladder, but neither this organ nor the kidney can be felt. Apart from this nothing abnormal can be detected in the abdomen.

The X-ray report by Dr. Lynham is as follows: "The gall-bladder outline shows faintly (pathological gall-bladder). One stone is seen high up, with three others close to the spine, and possibly in the duct; these appear to be too dense for pure cholesterin; 2 others in lower part of gall-bladder. There is a curve of lumbar spine with scoliosis and arthritic changes. Organs are slightly displaced. The right kidney shadow can be seen. No renal calculus is detected. There is no evidence of gall-bladder pressure on the duodenum. Rib cartilages show some calcification and small calcified glands are seen. A pyelogram might give further information."

The stones were examined independently by Dr. Fletcher and Dr. Knyvett Gordon, who both agree that they consist of cholesterin, but whereas Dr. Fletcher reports that he can find no evidence of bile salts or bile pigments, Dr. Knyvett Gordon says: "The calculi have a nucleus consisting of protein matter and definite bile pigment." Dr. Fletcher, however, comments that, "The tests for both bile salts and bile pigments are by no means exact or convincing."

I think everyone who has seen these stones will agree with me that they are typical gall-stones. I have had the literature searched, and can find no record of gall-stones being passed through the urinary tract. Cholesterin stones in the kidney and ureter have been described; in Dixon Mann's "Physiology and Pathology of the Urine" (second edition, 1908, p. 276), it is stated that: "Isolated instances of concretions of cholesterin have been recorded.

Horbaczewski (*Zeitschr. f. phys. Chemie*, 1893) analysed a calculus which weighed 360 gr. and found it consisted of 95·84 per cent. cholesterin. At the necropsy on the body of a woman, Glinski (*Wratsch.* 1893) found five irregularly formed stones in a calyx of one of the kidneys, along with one in the ureter, all of cholesterin. During the last month of her life the patient passed a large quantity of cholesterin crystals in the urine. Fatty concretions of doubtful origin have also been met with."

### Retro-peritoneal Tumour (Suprarenal Adenoma).

Shown by W. GIRLING BALL, F.R.C.S.

THIS tumour was removed from a man, aged 55, who stated that he was quite well until June, 1923, when he had an attack of pleurisy on the left side. In September, 1923, he began to lose appetite, and suffered from occasional nausea with subsequent loss of weight. He had no other symptoms. When first seen he was looking very ill, and a swelling was discovered in the left hypochondrium; this swelling extended from the sixth rib above, in the mid-axillary line, down to 1 in. below the umbilicus in the same line: it passed into the left loin and across the middle line in front. It was obviously a solid swelling which moved on respiration. There was resonance over the tumour on percussion. Examination of the urine did not reveal the presence of any abnormal constituents, and the X-ray pictures failed to demonstrate the presence of a stone.

Cystoscopy was carried out on November 30, and both ureteric orifices were seen to be working quite naturally, and there was a normal efflux from each. An attempt was made to pass a ureteric bougie, which only reached to 4½ in. above the vesical orifice, where there seemed to be a blockage.

On December 7, 1923, this tumour was removed, and was found to lie above the kidney, which had been pushed down into the left iliac fossa, the ureter itself being kinked apparently at the site at which the catheter had stopped; above, the tumour reached to the diaphragm, and was lying behind the tail of the pancreas and quite separate from it. During removal of the tumour the duodenum was seen, also the vena cava and aorta. The tumour itself presented very little difficulty in its removal except for its size and the presence of a large number of greatly distended blood-vessels on its surface; some of these were torn, and as a result there was considerable bleeding during the operation, which necessitated blood transfusion being carried out at the end of the procedure.

The patient made an uninterrupted recovery.

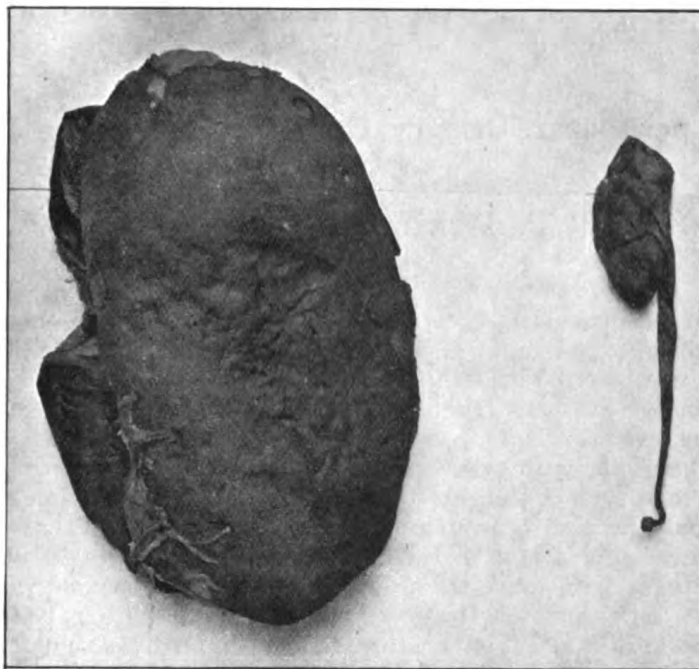
*Microscopically*, the structure is that of suprarenal cortex with a tendency to the occurrence of alveolar structure in some parts and to the presence of distinctly vacuolated cells in others. Sir Bernard Spilsbury, who examined the sections, believes this to be a suprarenal adenoma, probably innocent in character, but he would not be surprised if it gave rise to metastases.

**Congenital Atrophy of Left Kidney.**

By H. P. WINSBURY WHITE, F.R.C.S.

THE specimen was removed from a male patient, aged 56, who died at the Royal Free Hospital, of influenzal pneumonia.

The kidney measured  $1\frac{1}{2}$  in. by  $\frac{1}{2}$  in. by  $\frac{7}{8}$  in., and weighed 1 drachm. It was buried in a large mass of peri-renal fibro-fatty tissue, to which it was very adherent. The main blood-vessels entering the hilum, were seen to be quite patent with no evidence of thrombosis. By rolling the pelvis between the fingers, the patency of this structure could be made out. A bristle passed into the pelvis through a slit demonstrated that the ureter was patent for its upper  $2\frac{1}{2}$  in. only. On section no differentiation between cortex and medulla could be appreciated.



*Microscopic Examination.*—Widespread marked fibrosis. Many small retention cysts containing inspissated colloid material surrounded by a thin fibrous capsule, devoid of any epithelial lining. In other areas there was no inspissated material but a ragged degenerated epithelium. The calyces were patent and lined with a degenerate type of cells. All the arteries showed a considerable degree of endarteritis obliterans, the fibrosis of the vessel walls being within the elastic laminae. There was no sign of thrombosis of vessels. A healthy looking nerve fibre could be seen entering from the hilum.

Microscopic section of the lower part of the ureter cut transversely showed the lumen to be completely blocked by colloid material and the entire absence of any lining epithelium was noted.

The opposite kidney showed a degree of hypertrophy consistent with long standing abnormal functional activity. It weighed  $7\frac{1}{2}$  oz. which is

nearly twice the weight of a healthy adult kidney. Microscopically, the hypertrophy was very evident in the appearance of the convoluted tubules. Here the lining cells were so large and swollen as to leave the lumina extremely narrow. Corresponding changes were noted in the glomeruli, which almost completely filled the Bowman's capsules. Evidence of the hypertrophy even extended to the blood-vessels, the musculature of the arteries being increased and the veins dilated.

It is interesting to consider whether this state of atrophy is not acquired rather than congenital. Such a concentric and widespread absence of functioning elements might be expected to arise from thrombosis of the main blood-vessels. But the patency of these structures can be made out both microscopically and macroscopically. The atrophy of chronic Bright's disease is never so extreme, and is in some degree bilateral. The non-patency of the greater portion of the ureter is against the idea of the condition being an acquired one. Congenital atrophy of one kidney is quite rare and there are not many cases recorded. Morris found only three examples in 15,000 post-mortem examinations.

### **Retro-peritoneal Urinary Cysts resulting from Injury to Ureter.**

By H. P. WINSBURY WHITE, F.R.C.S.

I HAVE three examples of this condition produced experimentally in rabbits. In each case 10 per cent. iodine was injected into the wall of one ureter after its exposure by laparotomy. A fine hypodermic needle was used. This was inserted into the ureteric wall, which could be seen through the peritoneum. It was, therefore, not necessary to incise the peritoneum overlying the ureter. The strong iodine produced ulceration and eventually rupture of the ureter with extravasation of urine.

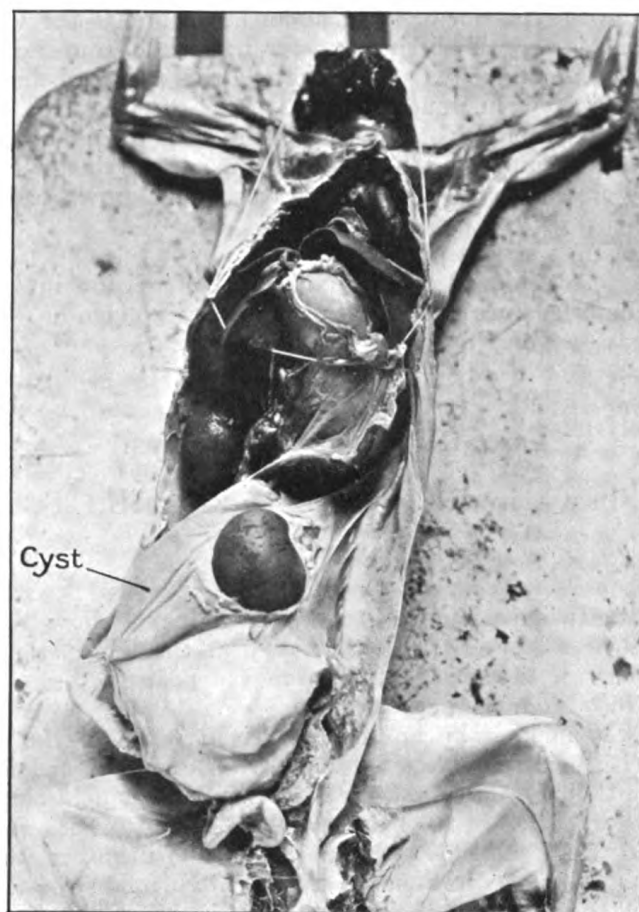
The course of each of the three cases was very much the same. A rounded swelling about the size of a tennis ball was palpable on that side of the abdomen where the ureter had been injured, and within a week of the operation. In each instance the animal at this stage appeared well. Within the next ten days, however, the swellings were difficult to palpate, and from this time onwards it was noticed that there was progressive deterioration in health, until the animals were killed, at periods varying from a fortnight to a month from the time of operation.

The difficulty in palpating the tumours was explained in each case by the presence in the peritoneal cavity of a large quantity of fluid. This, on examination, was found to contain urinary crystals.

In the case of the last animal, killed fourteen days after operation, a perforation in the cyst wall allowing escape of urine into the abdominal cavity, was clearly seen. In all the cases a close investigation after hardening showed the proximal portion of the ureter opening into the sac. In none of the animals did the sac communicate with the distal portion of the ureter, which was found to be obliterated by scar tissue. The cyst walls were well formed and about  $\frac{1}{8}$  in. thick, the inner surface for the most part being covered with granulation tissue; but within an inch or so of the opening of the ureter into the sac, in the first case, a well defined layer of transitional

epithelium could be made out on microscopic examination—its inward spread from the ureter being indicated.

The photograph of the cyst of the first case *in situ* shows its extent and its relationship to the kidneys. It originated in connexion with the right ureter. The left kidney and ureter however lie on its anterior surface. The posterior



Retro-peritoneal Urinary Cysts.

surface of the left kidney was much flattened by the pressure of the cyst. The right kidney showed a considerable degree of chronic interstitial nephritis, and a slight degree of hydronephrosis. It is instructive to observe that a well-formed cyst wall of such a size as that shown in the illustration can develop in so short a time.



**Sarcoma of Urinary Bladder, removed from Male aged 37.**

By E. T. C. MILLIGAN, F.R.C.S.

THE specimen shows the histological characteristics of a sarcoma. The growth arose from the interior of the bladder below and to the left of the left ureteric orifice and is about  $2\frac{1}{4}$  in. in diameter, irregularly rounded with nodular surface. In the bladder it was pedunculated, the pedicle being about  $\frac{3}{4}$  in. in diameter and about  $\frac{1}{4}$  in. long.

There had been symptoms of bleeding and cystitis for five months. Later, incontinence and difficulty of micturition had occurred, this being due to the proximity of the growth to the internal meatus. The growth as seen through the cystoscope appeared to fill the bladder.

The Wassermann test was positive.

The growth was removed through suprapubic incision into the bladder. It is now eight months since the operation; there is no sign of recurrence, and the symptoms have entirely disappeared.

**Two Tubercular Kidneys.**

Shown by J. B. MACALPINE, F.R.C.S.

I.

The first of the two specimens of tubercular kidney I show by the courtesy of Dr. Langley, who had charge of the case at the Grangethorpe Pensions Hospital, Manchester. It is a specimen removed post mortem, and with it is mounted the kidney from the opposite side.

The patient was a gunner in the Royal Garrison Artillery, who was invalided home in 1917 with heart trouble. The kidney was first noticed to be enlarged in 1915, but in the notes taken from Medical Boards, held as late as 1920, there is no further notice of its being palpable. In 1921 its size was again detected, and in 1922 the only examination of the urine, in which pus was found, occurred. This was in September, and in November of the same year he was admitted to the Grangethorpe Pensions Hospital suffering from shortness of breath, pain in the chest, cough and vertigo. The mass in the right side of the abdomen was then noticed, and was considered to be the kidney. Repeated examinations revealed an absence of pus in the urine, and a marked trace of albuminuria. The Wassermann reaction was positive and 1 in 40 dilution. There was no eosinophilia. The night temperature ran up to  $100^{\circ}$  F. A surgeon was asked to see the patient, but in view of the general condition he considered that operative interference was contra-indicated. He eventually died as the result of his cardiac condition, in February, 1923.

The specimen of the kidney weighed 7 lb. and consists of large caseous cysts with little intervening renal tissue; the right ureter was as thick as a thumb, and in some places much larger. At its opening into the bladder it was strictured; the bladder itself contained one healed ulcer, and one superficial erosion. There was a tuberculous nodule in the prostate, and granulation tissue in both seminal vesicles. There were a few small foci of

tubercle in the lung, none being bigger than a filbert, and also one calcareous focus. There was a nutmeg liver, which was slightly cirrhotic and a typical sago spleen.

The left kidney weighed 13 oz. There was no tubercular focus, but on section there was evidence of chronic parenchymatous nephritis and amyloid disease.

Five points of interest are presented by this specimen:—

- (1) Its unusual development: it weighing 7 lb. when first removed.
- (2) A ureter strictured at the vesical inlet with which is associated an absence of pus from the urine, save on one occasion when a small quantity presumably leaked through.
- (3) The small healed vesical lesion associated with absence of urinary symptoms.
- (4) A positive Wassermann reaction.
- (5) An evidence of a primary focus of pulmonary tuberculosis.

## II.

This second specimen of tuberculous kidney I bring before you because it represents a type of which I have not seen another example, and so far as I know it is not generally described. It presents a massive infiltration of the kidney with tuberculosis. Unfortunately it is mounted without its capsule and the pelvis of the organ is also missing. This is due to the fact that nephrectomy was performed by the subcapsular method, the kidney being very adherent.

An examination of both the external and the cut surface shows that the whole of the organ, with the exception of a small portion at the upper pole, is permeated with the disease. In spite of this no area of suppuration or advanced caseation is observable. The cut surface shows numerous lines of tubercular infiltration, which radiate from the pelvis to the capsule, like the spokes of a cart wheel. These are almost ubiquitous, except for the before-mentioned upper pole. They were even more conspicuous when the section was fresh. Examination of the outer surface shows that practically the whole of the parenchyma is replaced by tuberculous material.

I make the following suggestion with regard to the pathology. In the ordinary types of tuberculous kidney, for instance the ulcero-cavernous type, any section taken in an area which, to the naked eye, is healthy, will show radiating lines of round-celled infiltration and tubercles. These extend from the pelvis to the capsule and can be found at any time after the infection of the pelvic mucosa has become well established. Generally speaking, they remain microscopic and abortive; their importance consists in the fact that they form a connecting link between pelvis and capsule, and ultimately with the perirenal fat. The patient from whom this specimen was taken, came to hospital with a perirenal abscess, and was without any urinary symptoms. The abscess was shown to be tuberculous by aspiration, and the kidney was removed after it had been incriminated by cystoscopic methods.

The site of the original focus of the disease is a matter for conjecture, and I suggest that it may have been at the apex of a papilla, from which it rapidly infected the pelvis with secondary spread to the parenchyma.

This specimen and the one just shown make an interesting pair in that the ureter in both was clinically strictured. There was here, in this specimen, also evidence of slight change at the ureteric orifice, this having evidently taken

place before the ureter had become occluded, and not having healed when the supply of infective material was cut off. No excretion of dye could, however, be obtained, and it was impassable to the catheter.

Ureteric obstruction, which is supposed to occur in 10 per cent. of cases, is in my own short series of cases, of greater frequency, and is of much importance, as the first of these specimens shows, for it removes pus and other evidences from the urine, and also saves the bladder from involvement. The bladder is the usual tell-tale in urinary tuberculosis, and under these circumstances it is silent, with the result that symptoms are liable to be late, and when they do occur, to be anomalous. This I have seen well marked in several other cases. The significance of the tumour in the first case was not appreciated, a fact which probably made little difference to the patient, in view of the severity of his cardiac condition.

In each of these patients there was evidence that the bladder had been infected before the stricture was formed, and this is precisely what one would expect, for occlusion can only be a fairly late event, and one would presume that some bladder ulceration must occur before the supply of infective material was cut off.

### Subcapsular Hæmorrhage (Renal).

By H. A. LEDIARD, F.R.C.S.

THE few references made to this rare condition justify the record of an example that was met with in 1911. A woman, aged 49, was sent into hospital for emergency operation with suspected appendicitis. In usual health up to a fortnight before, when pain in the right lumbar and iliac region began, followed by vomiting.

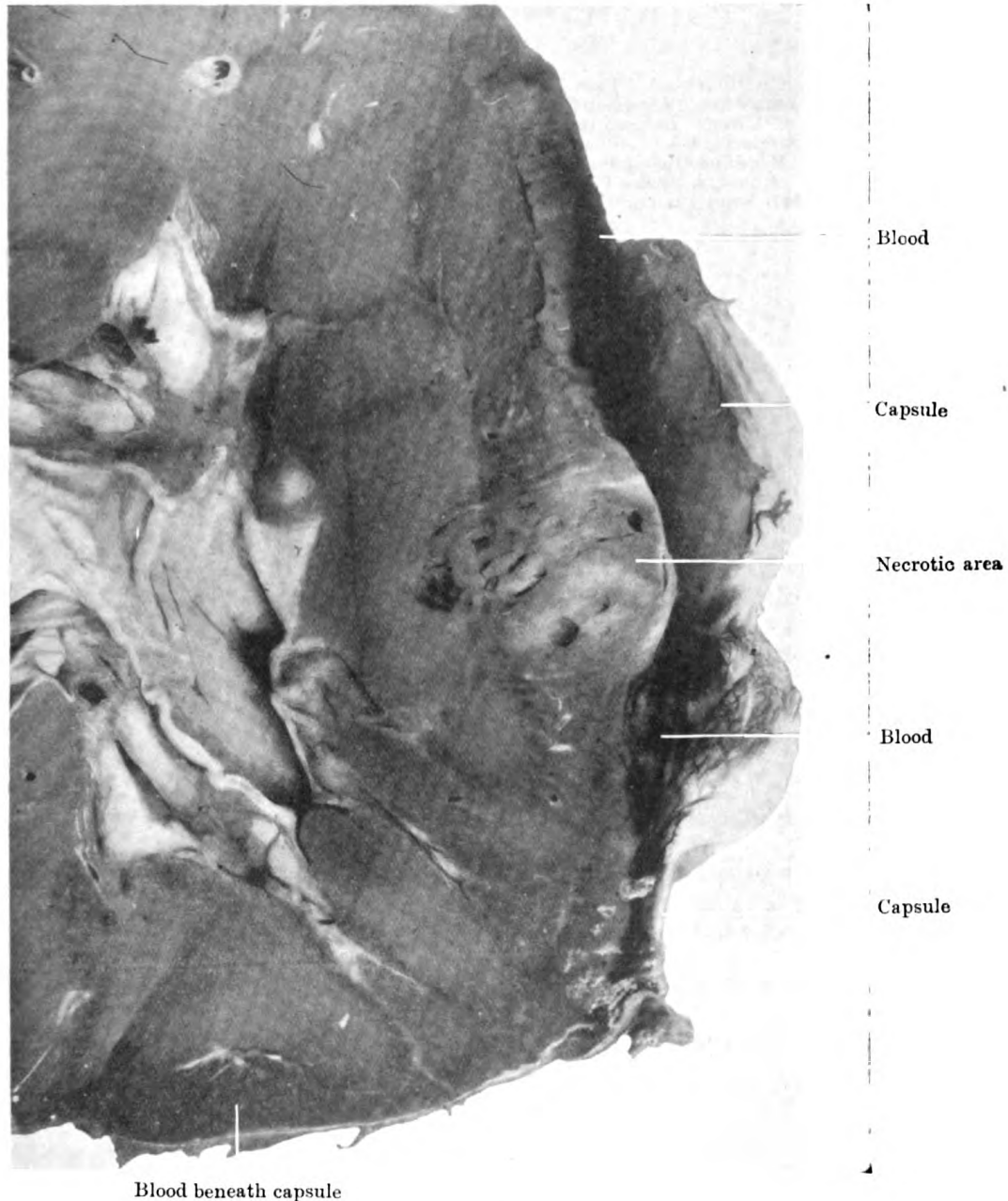
When admitted there was a temperature of 103° and a pulse of 112, an elastic swelling, somewhat mobile, in the region of the right kidney. Upon the swelling being exposed, it was seen to be connected with the kidney and on incision through the unruptured capsule, a large quantity—estimated at half a tumblerful—of recent blood clot was turned out from the back part of the kidney surface. The next thing seen was a projection from the outer border of the kidney, suggesting a neoplasm, and the kidney was removed.

The urine examined previous to operation contained hyaline and granular casts, and after nephrectomy, there was practically suppression, the patient dying on the fourth day from uræmia.

The kidneys were examined and prepared for me by Mr. Richard Muir, of Edinburgh, who reported on the kidney removed by operation: "The renal tissue shows advanced chronic interstitial nephritis followed by more acute changes and is extensively necrosed, due to some toxic substance. There are both staphylococci and bacilli present in the tissues, some of the vessels being plugged. The hæmorrhage in the capsule extends into the surrounding fat."

The left kidney, removed at the post mortem, was examined at a much later date and showed interstitial change but not of the suppurative or hæmorrhagic character. The case interests in diagnosis, treatment and pathology. There being no hæmaturia the nature of the renal tumour was in doubt, and the blood beneath the unruptured capsule came unexpectedly. The treatment adopted is open to question, and the necrotic area revealed was not established until the microscope was used on the excised kidney. The pathology is quite clear,

for necrotic areas, with minute abscesses unbroken down, are familiar in women after childbirth, as described by Sir Humphry Rolleston, but the occurrence of hæmorrhage beneath the capsule is looked upon as a surgical curiosity. The patient had had nine children as well as two miscarriages.



With regard to the necrotic area, there was no thrombosis of vessels or signs of infarct, neither was there any clinical history of trauma or strain.

That the interstitial nephritis was of old standing there can be little doubt, equally so, that a hæmatogenous infection was superadded during the fortnight preceding admission to hospital. I suggest that the extravasated blood oozed slowly and caused the pain, vomiting and fever which accompanied. The capsule of the kidney, being thick, resisted any tendency to rupture. Lastly, the kidney with subcapsular hæmorrhage showed marked arterial sclerotic changes.

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## Section of Urology.

President—Dr. W. LANGDON BROWN.

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### Sub-parietal Injury of the Kidney : with Notes on Forty-two Cases.

By H. P. WINSBURY WHITE, F.R.C.S.

#### INTRODUCTORY.

As the title of my paper indicates, I am dealing only with non-penetrating injuries of the kidney.

One is fortunate in being able to refer to such an extensive review of this subject as that of Keen [1], of Philadelphia. He collected details of 108 cases published between 1878 and 1896.

In 1902, Davis [2] followed on from where Keen left off, and gave us another thirty-four cases, while in 1916, Gregory Connell [3] dealt with the whole of the literature of the subject to that time, which included 861 cases. Since then there have been recorded many more examples in smaller numbers.

The cases that I have had an opportunity of reviewing are the last forty-two consecutive cases dealt with in the wards of the Royal Free Hospital up to the end of 1923, and I wish to take this opportunity of thanking the staff of the hospital for permission to publish the notes of these cases.

I will, as briefly as possible, relate their outstanding features :—

Forty (95 per cent.) were males ; thirty (71 per cent.) were under 30 years of age.

In all cases except six, there was definite evidence of a direct injury, and in five of the latter it was not known whether there was direct injury or not, while in the sixth, the injury was apparently indirect, the patient having fallen out of a van on to his back into the road.

The right and the left kidney were each found to be the injured organ in the same number of instances, namely, twenty-one.

In six cases (14 per cent.) the renal injury was complicated by serious damage to other structures, as follows :—

(a) Fracture of several overlying ribs in three cases ; (b) fracture of ribs and skull in one case ; (c) fracture of ribs, rupture of liver and diaphragm, with right kidney in pleural cavity in one case ; (d) rupture of liver and spleen in one case.

Of these all except two cases were beyond surgical aid on admission, and died shortly afterwards. One only of this group ultimately recovered.

Connell noted about 5 per cent. of complicated cases out of 861.

## CLINICAL FEATURES.

With regard to the signs of kidney injury, they may be stated as follows:—

(1) Shock; (2) pain in the loin, with tenderness and rigidity; (3) swelling in the kidney region; (4) hæmaturia.

Disregarding the four complicated cases which were moribund on admission, it was noticed that in nineteen cases (50 per cent.) there was no report that the patient was suffering from shock. The absence of this sign in many instances has been remarked upon by other observers. Undoubtedly its presence or absence depends largely upon how soon after the accident the patient is seen. The immediate shock associated with a blow over the kidney is well recognized in boxing circles.

The sudden or gradual onset of pain was observed in almost all cases. Apart from that suffered at the time of violence, there is to be noted a definite increase of this symptom some time afterwards. It appears to be due to the tension caused by the accumulation of blood or urine, either beneath the true capsule, in the pelvis of the kidney, or in the perinephric tissue, or more rarely by an invasion of the peritoneum. And according to the amount, situation, and rapidity of accumulation of blood or urine, so will the pain vary in its intensity and time of onset. In one case the pain did not occur till fourteen days after the injury, when it was accompanied by the first manifestation of hæmaturia. In another, not till a week after the patient was struck in the abdomen was there any pain, when, however, it came on with great severity following strenuous muscular exercise and was accompanied by a tumour in the loin. Operation revealed a distended pelvis with a ureter blocked by clot.

In another case the occurrence of severe pain three weeks after the injury revealed a large urinary cyst in the loin.

In all the other cases the maximum pain was experienced soon after the injury. It was sufficiently frequent to be worthy of special notice that the pain in most instances was described as being very severe. Tenderness and rigidity of the overlying muscles were noted in all cases, the degree corresponding fairly closely with the amount of pain.

Swelling in the kidney region may be difficult to make out on account of abdominal rigidity, or it may be entirely absent, as revealed by a thorough examination under an anæsthetic. The absence of this physical sign, however, cannot be regarded as an indication that the bleeding is slight, as it must not be forgotten that the blood may be escaping into the peritoneal cavity.

Rupture of the peritoneum is much more common in children than in adults, for the reason that the perinephric fat is not present till the age of 8 or 10, and preceding the development of this the peritoneum is in direct contact with the anterior surface of the kidney and closely adherent to it. It might be expected, therefore, that in children under 10 years of age, extravasation into the peritoneum would be a common complication. This is not so, as can be seen from the figures in this series, in which renal injury occurred in six children under 10, without any intraperitoneal complication. In one case it was doubtful, but an exploratory laparotomy proved negative. Rupture of the peritoneum undoubtedly makes the prognosis grave when present; in support of this Keen [1] quotes figures from the statistics of Maas, showing that out of seven cases of ruptured kidney under 10 years of age, six of the patients died. In the cases I am presenting of four laparotomies performed, blood from a ruptured kidney was found in the peritoneal cavity in a small amount in one case only, a male of 30 years.

If the four severely complicated cases, admitted in a moribund condition, be disregarded, in seventeen cases only (44 per cent.) was a palpable swelling in the loin or dullness in the flank made out with certainty. Of the remainder, the renal injury was indicated by hæmaturia, except in two cases. The fact of severe injury to the kidney was not certain in either of these till many days after the receipt of violence. In one instance a large urinary cyst was discovered and evacuated thirty days after admission. In the other, a similar swelling was located and drained twenty-five days after receipt of injury.

The importance of hæmaturia in cases in which the violence has been confined to the kidney region lies in the fact that it is direct evidence of renal injury. Where, however, the violence has been sustained in the kidney region and in the lower part of the body as well, the possibility of bleeding from some part of the lower urinary tract should be borne in mind and investigated. There was an average of eleven days for the duration of the hæmaturia in twenty-eight cases in which this occurrence was noted. With regard to the time of onset of hæmaturia, the cases fall into three groups, according as the hæmaturia (1) occurred soon after injury; (2) was delayed for several days; (3) was completely absent. Eighty-four per cent. of the cases fall into the first group—hæmaturia soon after injury. In one case there was marked delay in the appearance of blood in the urine. It was noticed for the first time fourteen days after the accident, and accompanied by the onset of pain in the region of the injured kidney.

In five cases (13 per cent.) there was no noticeable hæmaturia at all throughout the course of the illness. As the account of these cases will show, they form the most difficult group to deal with. In four of them, the kidney injury was confirmed by operation, and in the fifth one on the post-mortem table. In each of the four cases there were obvious signs of some serious lesion beneath the abdominal wall. In three, the abdomen was opened, without any intraperitoneal damage being found, and the kidney lesion was dealt with either at this time or later. In two of the five cases the patients died. In one of these, not only was there absence of hæmaturia, but the presence of an otherwise obvious swelling or of dullness in the loin was masked by a paralytic distension of the bowel. It was not till seven days later that a loin tumour was located, when a perinephric abscess was drained. The patient died four days after this. The difficulties in dealing with such a case are striking. In addition to the absence of hæmaturia, the kidney injury in one case was hidden by serious damage to the overlying chest wall, death resulting in forty-eight hours after the accident, and only at the autopsy was it discovered that the lower part of the left kidney was torn away and the ureter blocked with the clot.

From these facts it is obvious that the complete absence of hæmaturia may accompany the more severe degrees of renal injury, at the same time leaving the site of the lesion a matter of doubt.

In all the cases treated conservatively there was some degree of pyrexia within forty-eight hours of the receipt of violence. In those with the lesser degrees of injury this had disappeared within another two or three days. In the majority the temperatures were settled by the end of the first week. In several it took nine or ten days, and in those on which operation, though delayed, was ultimately necessary, the unsettled state of the temperature was a prominent feature.

Among the clinical features of less importance may be mentioned retention



of urine, which occurred in three cases. Two cases were associated with copious hæmaturia. Maas quotes three cases of clot retention out of a total of seventy-one. The other case was apparently a reflex phenomenon, occurring in the case just mentioned, with paralytic distension of the bowel and absence of hæmaturia.

#### PATHOLOGY.

Before dealing with the treatment, it will be helpful to consider some points with regard to pathology.

In the large majority of cases there is evidence that the injury to the organ is direct. It is certain that indirect violence is not often the cause of a serious lesion, although spontaneous rupture following severe muscular action has been reported in a number of diseased conditions of the kidney.



FIG. 1.—Front view of a right kidney injured by a blow in the side. It shows two grooves indicating the lines of junction of the renal lobules. A large transverse tear, broadest towards the hilum, has occurred along such a groove in the upper half.

An examination of a considerable number of specimens representing traumatism of the kidney shows the common type of injury in the form of one or more transverse fissures extending from the hilum towards the outer border. Thus, the split occurs along the grain formed by the medullary rays, and in conformity with the mode of development (figs. 1 and 2).

Working on the cadaver, Küster demonstrated that rupture took place from hydraulic force through the fluid in the pelvis of the kidney and in the blood-vessels. He showed that quite a different type of rupture occurred when produced in a kidney the vessels and pelvis of which were empty, while force applied to the outer border produced a longitudinal fissure.

Figs. 1 and 2 represent the front and back views respectively of an injured kidney illustrating the common characteristics of fissures. The persistence of the foetal type is seen in this particular specimen, the lines of junction of the lobules being indicated by the transverse grooves. The fissures follow the lines of junction of the lobules rather than the courses of the larger blood-vessels, which run more obliquely in the kidney substance (fig. 3). It is to be noted that the fissures are widest at, and apparently radiate from, the hilum. This indicates the likelihood of injury to the pelvis.

In a number of examples of experimental rupture which I produced in the post-mortem room, these features were repeated with unvarying regularity, and in every instance in which there was fluid present in the pelvis rupture of this structure was produced as well.

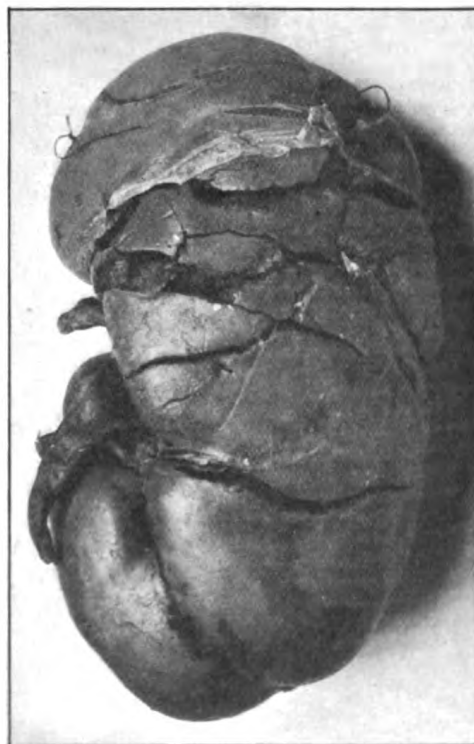


FIG. 2.—Posterior view of same kidney as shown in fig. 1. In addition to the transverse direction of the main fissures, note the shattered appearance resulting from hydraulic force.

The bearing of the character of the lesion in the kidney on the clinical features of the case is of the greatest importance. First, with regard to whether there will be little or much bleeding. Secondly, as to whether the amount of bleeding will be indicated by its accumulation in the bladder or loin, or whether it will pass into the peritoneum. Thirdly, with regard to the extravasation of urine, which may eventually give rise to infection.

Rupture of the renal substance, while the true capsule remains intact, is unlikely to give rise to serious hæmorrhage, and there will be no tumour in the loin. Pain, however, may be very severe. A rupture of the renal substance and true capsule will allow a considerable escape of blood or urine

into the perinephric tissue, giving rise to a palpable tumour. The extravasation may be further increased if the perinephric capsule itself is torn, and in children under ten years all the blood may escape into the abdominal cavity, and the prognosis of the case at once becomes more grave.

Injury of the renal vessels without involvement of the organ itself, with escape of blood into the peritoneal cavity, is a serious and rare form that has been recorded.

The danger of primary hæmorrhage would seem to be in proportion to the different degrees of extravasation previously mentioned.

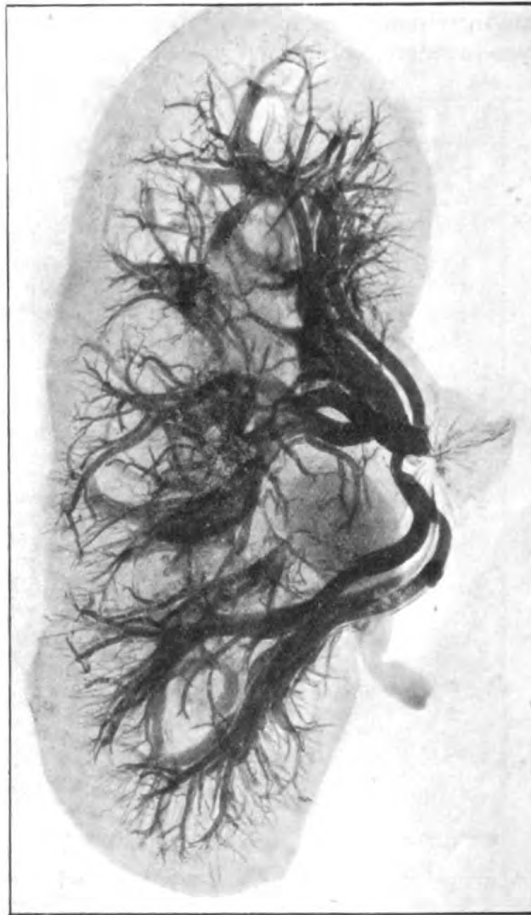


FIG. 3.—A radiogram of the injected blood-vessels of a kidney. The oblique direction of the larger channels in the vicinity of the hilum is well illustrated. They do not correspond with the directions of the fissures in the two preceding figures.

A complete division of the kidney does not necessarily mean that a very severe primary hæmorrhage will follow. A number of cases are on record in which not till many days after the receipt of injury had it been discovered by operation that the organ had been completely divided. On the other hand, some of the more serious cases have been due to relatively small tears.

It is of sufficient importance to repeat that the escape into the bladder of only small quantities of blood, or the absence of blood in the bladder, is no indication as to the amount of bleeding taking place.

In many cases some extravasation of urine must result. Fortunately, normal acid urine is not infective, so the danger of sepsis is not immediate. But experience shows that any considerable degree of leakage of urine eventually gives rise to infection, although the results of conservative treatment show that serious extravasation and infection do not occur in the majority. From a consideration of the hydraulic theory of rupture, it seems clear that extravasation must take place whenever the rupture is due partly or wholly to the presence in the pelvis of urine which is forced into the calyces and through the parenchyma. Moreover, in addition to the outflow of urine through the kidney substance, it is probable that in many cases the pelvis or one of the main calyces ruptures as well. For Guterbock reports a torn pelvis in 40 per cent. of thirty-two examples of injured kidneys found at autopsy.

The same writer also states that in 326 autopsies on cases of death from accidents, injury to the kidney was found in 10 per cent.

#### TREATMENT.

In the forty-two cases that I am presenting, thirty-six (85 per cent.) were treated without operation. Four, however, were dying when admitted, and operation was not attempted. Another patient died from primary hæmorrhage forty-eight hours after admission. The actual site of hæmorrhage was obscured by damage to the overlying chest wall. Six cases were operated upon, with two deaths, bringing the total mortality to 16 per cent. Four were dealt with as emergency operations soon after the receipt of injury. One of these patients was too ill to stand nephrectomy, which was abandoned after the abdomen had been opened, and the patient died soon after operation. In another, nephrectomy was performed successfully by the lumbar route. The kidney was found in two pieces.

The remaining two emergencies were both submitted to laparotomy, as intra-peritoneal injury was suspected. In one of these no peritoneal injury was found. A left perinephric abscess subsequently developed, and it was drained seven days after injury. Death followed two days later. The finding at the laparotomy of the second case was similarly negative, but in thirty-three days' time a urinary cyst in the right loin was drained, with recovery as a result.

Of the two remaining cases not dealt with by immediate operation one was that of a patient admitted fourteen days after injury with a large swelling in one loin. Exploration revealed a distended kidney, the ureter of which was blocked by clot. The kidney was drained, with complete recovery. In the other, three weeks after receipt of injury, a swelling developed in one kidney region, accompanied by signs of infection. An incision was made and a large retroperitoneal accumulation of urine was drained away. Nephrectomy was done fourteen days later, when a hydronephrotic kidney with a stricture at the uretero-pelvic junction was found. Recovery resulted.

Perirenal extravasation of urine should be suspected when the patient's general condition remains poor, especially when there is the accompaniment of an oscillating temperature. The loin should be opened and drained.

Of the thirty-one cases (73 per cent.) treated successfully without operation, although in all there was hæmaturia and tenderness with rigidity over one kidney region, after shock had passed there was no reason to suppose that any serious hæmorrhage was going on. In two cases was there a definite palpable swelling round the injured kidneys. In neither case did it give any trouble,

and in each case it had diminished considerably in size before the patients' discharge from hospital fourteen and seventeen days respectively after receipt of their injuries.

Judging from the literature on the subject, it appears that considerable diversity of opinion has existed as to which line of treatment should be followed. Keen [1], in reviewing the 108 cases that he collected, criticized the treatment of the cases unoperated upon that died. There were thirteen early deaths from shock and primary hæmorrhage, and ten late deaths from septic causes and hæmorrhage. He suggested that probably all of these cases should have been operated upon, with a likelihood of a considerable lowering of mortality. This, of course, would depend largely upon the degree of exsanguination reached before operation is undertaken, as, for example, in the four cases of this series in which the patients were already past the stage when surgical aid could benefit them at the time they first came under observation. Keen took it for granted that when operations had to be carried out, nephrectomy was the proper course.

Davis [2] was in favour of packing in most cases submitted to operation. He did not consider suture safe. Gregory Connell collected sixteen cases treated by suture of the damaged kidney, with no death. In every case the sutures caused satisfactory hæmostasis. In one case, however, secondary nephrectomy was necessary, and a fistula occurred in another. He quotes also 107 cases with a mortality of 8.5 per cent. treated by conservative operation in which the damaged kidney was sutured, packed or drained. He concludes that nephrectomy should be reserved for very extensive injury, but that conservative treatment, preferably by suture, is indicated in the majority of early cases.

Ponomareff [4], following conservative treatment as much as possible in fifty-seven cases, had a mortality of less than 6 per cent. In the three fatal cases there were injuries of other organs. Operation was performed in eight cases.

Frank and Michelson, who also treated conservatively, record about the same figures for mortality and necessity for operation. It is clear that opinion has steadily inclined towards conservative treatment, whether it be operative or expectant.

In considering the line of treatment in an early case, the immediate concern is as to whether there is any serious hæmorrhage or involvement of the peritoneum. In the absence of indications of either we may institute expectant treatment.

Between the two extremes, where operation is clearly indicated on the one hand and expectant treatment on the other, there will always be a certain proportion of cases which will leave the surgeon in a state of indecision as to which course he should follow. When the doubt is serious, it is perhaps wiser to operate, for although experience shows that so many cases do well on non-operative treatment, it is quite impossible to foretell that no severe complication will arise. If expectant treatment is undertaken in a doubtful case, the closest watch must be kept, and on the first suggestion of increase in size of any perinephric swelling which may be present, or of other signs indicating infection or continued hæmorrhage, there should be no delay in opening the loin. The results of delaying operation until perinephric infection is advanced, are bad, especially where nephrectomy is performed. In this type of case drainage of the loin should precede nephrectomy, unless the surgeon's hand is forced by having to deal with a secondary hæmorrhage which is not controlled by more conservative measures. In some cases in which preliminary drainage is instituted, the condition clears up without nephrectomy.

The actual way the kidney is dealt with will depend upon its condition when exposed. A pulped, or divided kidney, or detached fragments of renal tissue, should be removed. When, however, the patient's condition is so poor that he is not likely to stand nephrectomy, hæmostasis may be obtained by ligaturing the vascular pedicle, and draining the loin. Blood transfusion should proceed the moment the source of bleeding has been controlled. It should be carried out in all cases except where the loss of blood has been slight. If the presence of the opposite renal organ has not previously been established, the strongest efforts should be made to avoid the performance of nephrectomy. Bearing in mind the good results from suture, and the likelihood of secondary hæmorrhage from packing, the former procedure should be followed where possible, after dealing with any bleeding points.

In considering the question of nephrectomy the possibility of the injured kidney being the sole functioning renal organ has always to be kept in mind. When the case is too urgent to allow of investigation by cystoscopy, provided there is no perinephric sepsis, the point may be settled by extending the lumbar incision to open the peritoneum, so that the hand may be admitted to the abdominal cavity. In non-urgent cases a cystoscopic examination can, as a rule, be carried out. The subsequent exploration may then be kept entirely extraperitoneal.

A practical point in connexion with the loin incision is to make an extensive one, in order to obviate any further damage to the kidney from manipulations in bringing it to the surface, otherwise a nephrectomy which might have been avoided may become a necessity.

Although the lumbar approach is to be preferred, the possibility of intra-peritoneal complications makes abdominal section necessary in some cases. A difficulty in deciding as to whether the abdomen should be opened arises from the knowledge that notwithstanding the presence of widespread rigidity and tenderness on more than one occasion laparotomy has failed to reveal any serious intra-abdominal lesion. If, however, these signs progress and become very marked, there should be no delay in performing abdominal section.

In dealing with retroperitoneal extravasation, and infection in later cases, incision and drainage should precede nephrectomy, as the results of the one-stage operation in the presence of extensive perinephric sepsis are not as good as when preliminary drainage of the loin is carried out before the removal of the kidney.

While considering the treatment of the perinephric infection, the most earnest attention should be given to the question as to whether the peritoneal cavity is not infected as well. Three of Keen's [1] patients died of peritonitis following perinephric infection at intervals of fourteen days, twenty-two days and eleven weeks respectively, from the times of the accidents. In the experimental production of retroperitoneal urinary extravasation which I have carried out in a number of rabbits, it was found in each case during the first week or ten days, that although the retroperitoneal extravasation was readily detected on palpation as a localized swelling, there was no sign of any peritoneal infection. Subsequently, however, evidence of a low form of peritonitis became manifest, and after killing the animals at different intervals an abundance of free turbid fluid was found in each peritoneal cavity.

#### SUMMARY.

(1) In the great majority of cases of renal injury, there is hæmaturia soon after the receipt of violence. This was noted in 84 per cent.

(2) Complete absence of hæmaturia may accompany the most severe degrees of rupture of kidney. Hæmaturia was not present in 13 per cent. Death occurred in 40 per cent. of these cases. The appearance of blood in the urine may be delayed for some days.

(3) The amount of blood present must not be taken as an indication of the full extent of the bleeding.

(4) Cases complicated by injury of other organs, show a high mortality, 83 per cent. in this series.

(5) The majority of cases of renal injury can be successfully treated without operation. A good result was obtained in this way in 73 per cent.

(6) In a few cases in which expectant treatment is commenced, operation may eventually be necessary on account of secondary hæmorrhage or sepsis. Operation should not be delayed till infection is advanced.

(7) The indications for immediate operation are: (a) rapid tumour formation in the loin: (b) signs of intraperitoneal involvement.

(8) Blood transfusion should be carried out in the case of severe hæmorrhage, as soon as the bleeding is controlled. When operation is performed and the case is desperate, a ligature may be passed round the vascular pedicle, and the loin drained.

(9) When the diagnosis of renal injury is certain, the incision should be made in the loin. The abdomen may be explored by this route when intraperitoneal complications are suspected as well.

(10) An anterior laparotomy is reserved for cases in which renal injury is a matter of doubt, owing to the absence of hæmaturia.

(11) Operation should be undertaken with a view to preserving the kidney if possible. This object will be furthered by making an extensive parietal incision.

(12) Nephrectomy is necessary when the kidney is badly mutilated, or the vascular pedicle is torn, and, generally, for secondary hæmorrhage.

(13) Before nephrectomy is carried out, the fact of the presence of the opposite kidney should be ascertained whenever possible.

(14) Preliminary drainage of the loin for septic complications should precede nephrectomy, which, however, may be found later not to be necessary.

(15) With signs of delayed onset of perinephric sepsis, the possibility of peritoneal infection as well, should be kept in mind.

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## Section of Urology.

President—Dr. W. LANGDON BROWN.

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### The Treatment of Carcinoma of the Prostate.

By A. CLIFFORD MORSON, O.B.E., F.R.C.S.

#### PART. I.

REVOLUTIONARY change in the surgical treatment of malignant disease commenced with Listerian methods, which enabled clinicians to remove tumours without fear of sepsis. Then the advancement in our pathological knowledge of cancer, especially with regard to its mode of spread, resulted in more extensive operations being undertaken.

We now know that the malignant cell will live and multiply in the blood-stream as well as in the lymphatics. I recall with interest the simple experiment of injecting isolated sarcoma cells into the tail vein of a rat. Half a dozen of these cells in physiological saline introduced direct into the blood-stream gave rise to metastases in many of the organs within a few weeks.

The work of Handley upon lymphatic permeation has effected a remarkable change in our surgical technique. Whereas in breast cancer twenty-five years ago the breast only was removed, now together with this organ, muscles, fascia and lymphatics are taken away in one large mass. Other examples of operations based on this principle are the abdomino-perineal, the Wertheim operation and the block dissection of the neck for carcinoma of the tongue.

In those situations in which cancer will demonstrate its presence in the early stage of the disease, eradication by means of the knife may give relief from recurrence for as long as twenty years. I refer to intrinsic carcinoma of the larynx, cancer of the breast and melanotic sarcoma of the eye. It has been known for many years that the scirrhus type of cancer is the most chronic. Handley has shown that the malignant cell lying in a lymphatic channel may become surrounded by fibroblasts and disappear.

Lastly, Drew, of the Imperial Cancer Research Fund, experimenting upon the growth of cells outside the body, has demonstrated that a malignant tumour will take on disorderly growth in the absence of fibroblasts, but that in their presence the converse occurs. Thus, in the present state of our knowledge the greatest enemy of the malignant cell is the fibroblast.

With regard to the treatment of malignant disease of the prostate the advantages or disadvantages of a particular remedy must be reviewed in the light of pathological knowledge.

There are two distinct types of malignant disease which occur in the prostate gland. Albarran first drew our attention to one of these, the adenocarcinoma, a complication of the simple enlargement of the prostate. Its signs



and symptoms in the early stage are those commonly associated with the simple enlarged prostate, but hæmaturia may often be severe. In the late stage pain is an additional sign. The other manifestation of carcinoma is the scirrhus variety. In this type there is no intravesical projection of the growth and no hæmaturia, either in the first stage or the last. Pain is only present when the disease has spread beyond the anatomical limits of the gland, the chief signs and symptoms being those connected with a gradual contraction of the prostatic urethra.

To the expert clinician the diagnosis of the latter is straightforward, but in the condition where carcinoma is superimposed upon a simple enlargement, the surgeon removes the gland in the belief that he is dealing with an innocent growth, and his serenity of mind is not disturbed until he reads the pathological report. A frozen section made at the time of operation prevents such mistakes.

For the moment I would remind you of the nature of the structures which surround the prostate gland. The gland is encased in a fibrous bag. In the upright position the bottom of the bag is formed by the triangular ligament which is perforated by the urethra. The top or open end is formed by the mucous membrane of the base of the bladder and beneath that, cellular tissue, in which are embedded the seminal vesicles; immediately posterior to these structures is the rectum. The sides of the bag consist of a portion of the sheath of the levator ani, dense fibrous tissue which lies between the prostate and the anal canal behind, and the pubic arch laterally. If, following the enucleation of an enlarged prostate, the prostatic cavity be illuminated by the bladder-lamp, it will at once be seen how smooth is the lining of this fibrous bag and how avascular it is; and it will be noted that all the bleeding is coming from the top or open end, namely immediately beneath the mucous membrane.

Now I have already drawn your attention to the fact that the successful barrier to the spread of malignant disease is fibrous tissue, and the more dense and avascular it is the less chance has the malignant cell of penetrating it.

Thus I explain the reason why it is possible with the finger alone to enucleate a malignant prostate, however extensive, out of its fibrous bag, with the exception of the region at the open end of the bag where cellular tissue and seminal vesicles are situated. The extension of malignant disease beyond the glandular tissue is always by the latter route. It never invades the rectal mucous membrane so as to cause ulceration in the bowel, and it never involves the triangular ligament, but true to its name "cancer" it will send, claw-like, a chain of cells along the membranous urethra.

It appears to be the custom of the majority of surgeons to decline to perform any operation upon a malignant prostate, other than suprapubic cystotomy, which is a purely palliative measure for the relief of urethral obstruction, in all cases where by rectal examination it can be demonstrated that the prostate is fixed or immobile bimanually.

A correct interpretation of the size of a growth of the prostate by digital palpation through the rectal wall is almost impossible. I have entirely discarded this method of investigation for assisting me to decide whether a case shall be treated by surgical removal of the malignant mass.

Science has placed in our hands three methods by which we are enabled to determine the extent of the spread of the disease. Changes in the mucous membrane of the base of the bladder can be demonstrated by cystoscopy, which will reveal gross infiltration by cancer cells, that is, the presence of

nodules, congestion and œdema. Next, an estimation of the renal function will determine whether the growth has reached the inter-ureteric bar and is occluding the ureteric orifices. Lastly, before any method of treatment is decided upon it is necessary that a skiagram of the lumbar sacral region should be taken. No further X-ray examination is needed, for, to quote Bumpus, of the Mayo Clinic, "it is unnecessary to search the chest for evidence of metastases since it probably never occurs there unless it is also in the pelvis or lumbar spine."

The methods of surgical removal of the malignant prostate are two: (1) Either the gland only can be taken away, a procedure comparable to amputation of the breast tissue without the removal of muscle, fascia, and lymphatics,

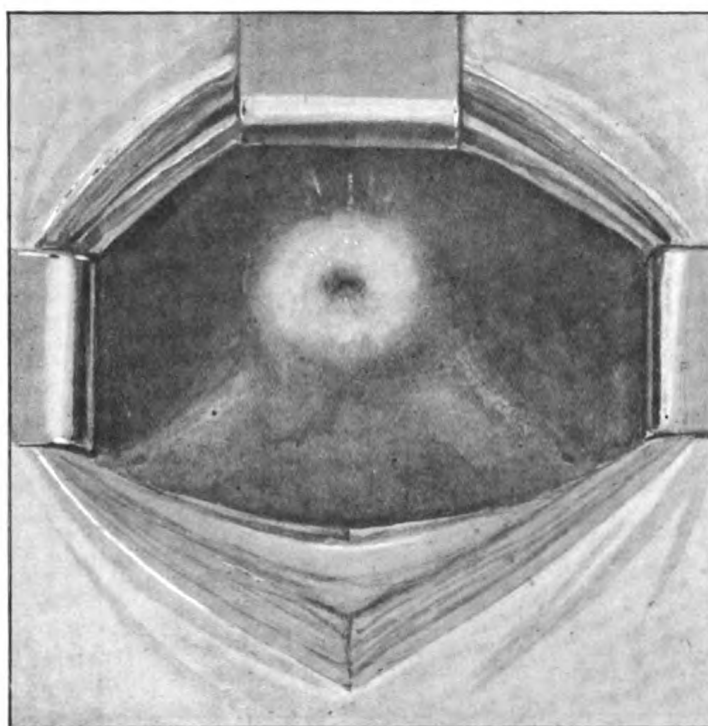


FIG. 1.—The appearance of the internal meatus of the bladder in a case of early carcinoma of the prostate. Note the commencing œdema of the mucous membrane.

which all surgeons to-day condemn as unsound; or (2) a block dissection of those tissues in the pelvis which pathology teaches us may become invaded by extension from the primary lesion, may be attempted.

With regard to the removal of the prostate *per se*, there is little to be said in its favour, though, just as in local removal of a malignant growth in any other organ of the body, relief of symptoms may be obtained.

I will now discuss what may be termed the radical operation for carcinoma of the prostate. This must consist in the removal not only of the prostate and seminal vesicles, but of all the tissues which form the base of the bladder, and which lie between it and the anterior wall of the rectum. For such a dissection, two routes are open—the perineal and the suprapubic. Young, of Baltimore,

is the advocate of the perineal route; he claims a considerable measure of success under operation done in this way. The details of this operation may be studied in the numerous papers which Young has published, and therefore I do not intend to discuss his technique beyond allusion to what may be necessary for purposes of comparison between the two methods of approach.

The operation which I have elaborated and practised is by the suprapubic route. This operation consists of two stages, first, the partial enucleation of the prostate, and, secondly, the removal by dissection of the tissues between it and the inter-ureteric bar. On one occasion the ureter was transplanted, but to this I will refer later.

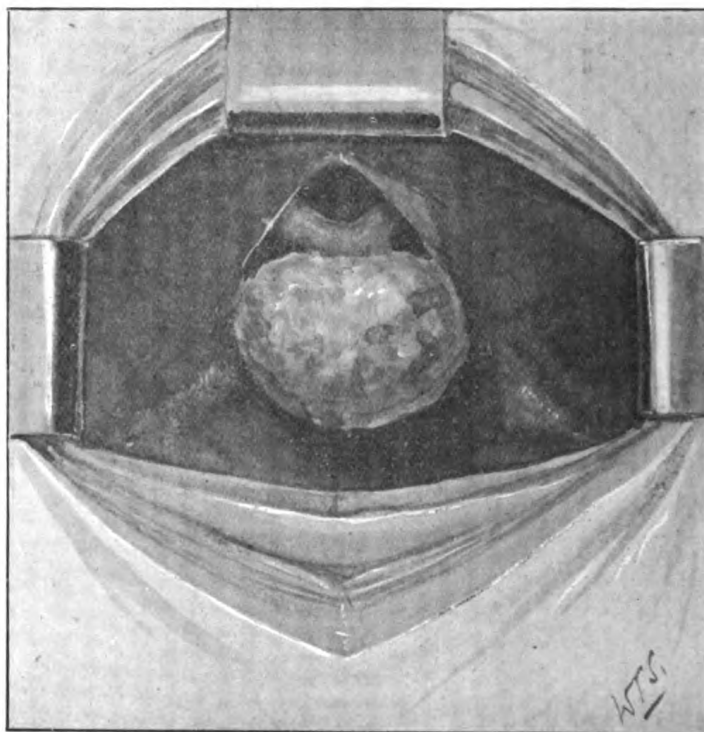


FIG. 2.—The same case as in fig. 1, showing the first stage of the radical operation for removal of prostatic cancer completed.

My technique is the following: With the patient in the Trendelenburg position, the bladder is opened and retractors inserted so as to give a good exposure of the internal meatus and the bladder base (fig. 1). An incision is then made round the internal meatus, and with a blunt dissector the line of cleavage between the growth and the fibrous capsule is determined. Partial enucleation with the finger follows, it being unnecessary to put a finger into the rectum to assist in the enucleation. When the prostate has been mobilized it is delivered into the bladder, with the aid of Duval's forceps, still with its attachments to the base of the bladder and the seminal vesicles intact. The latter act like hinges, and are exposed to view on their posterior surface (fig. 2).

The most difficult part of the operation now begins, for dissection is necessary in order to remove all the tissue anterior to the front of the rectum. Until recently, I have been hampered by the blood welling up at the bottom of the wound and obscuring my view, but this obstruction has been overcome by the use of Lake's suction pump. This instrument not only evacuates the blood as it collects, but by a slight modification can be used as a retractor.

A good deal of hæmorrhage follows the dissection of the seminal vesicles on their rectal surface, and it is necessary at this stage to tie the vessels in this region. At the same time it should be possible to find the vasa deferentia, and

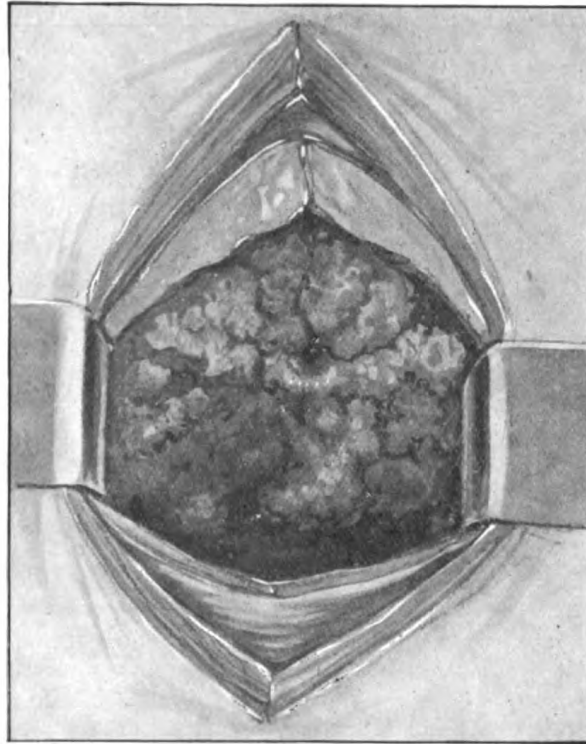


FIG. 3.—The appearance of the internal meatus of the bladder in a case of advanced carcinoma of the prostate.

to ligate and divide them. The latter manœuvre assists appreciably in freeing the seminal vesicles together with the base of the bladder. I then make an incision close to, and parallel to, the inter-ureteric bar and sweep it round towards the seminal vesicles. By blunt dissection the whole mass is separated from the rectum. At this point there is danger of buttonholing the anterior rectal wall, a mistake which may at a later date cost the patient's life.

With the removal of the malignant growth, a large raw surface is exposed, the base of which is formed by the rectum and the sides by the torn edges of the bladder walls. I try as far as possible to cover over the raw surface with flaps of mucous membrane, for there is always a fear of secondary hæmorrhage if

urine is leaking over an extensive granulating area. The operation is completed by tying a catheter in the urethra and inserting a large tube into the bladder suprapubically. I have now performed this operation in fourteen cases during the past eighteen months. Three of the patients died: one after being transferred from hospital to infirmary with recto-vesical fistula, the second died of cellulitis of the back due to a bedsore, and the third succumbed to shock two hours after operation. In the latter case, which was very advanced, I not only removed the bladder base but the intramural portion of the left ureter, transplanting it into the left lateral wall (figs. 3 and 4). So extensive is such an operation that the possibilities of recovery are remote, and I am therefore not disposed to repeat it.

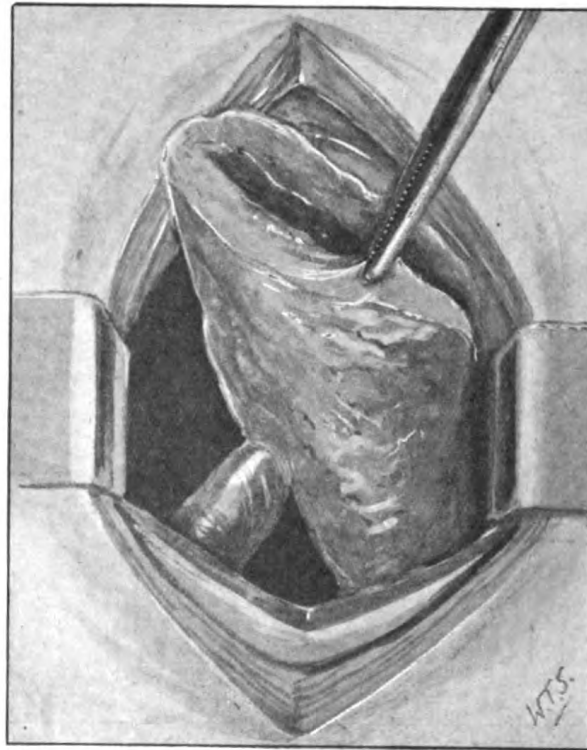


FIG. 4.—Infiltration of the left lateral wall of the bladder with growth. The left ureteric orifice is obstructed causing dilatation of the ureter above the constriction.

In the eleven cases in which the patients have survived, from time to time I have found it necessary to pass a sound to dilate the urethral passage. It will be understood that considerable scar tissue forms at the site of the operation, making a ridge between the bladder base and the urethra.

The post-operative complications have been hæmorrhage, the formation of a recto-vesical fistula and cicatricial contraction at the internal meatus, and delayed closing of the suprapubic opening.

Hæmorrhage from the bladder wound has been dealt with in the usual manner, and so far with success. In two cases recto-vesical fistula developed, one case terminating fatally; in the other case the fistula appears to be closing spontaneously. Cicatricial contraction and its sequela, delayed

closure of the suprapubic opening, have been satisfactorily treated by the aid of the indwelling catheter and subsequent intermittent dilatation.

The presence or absence of deep-seated metastases in these eleven cases has not been determined with X-rays, but I am satisfied that from clinical investigation there has been no recurrence at the site of operation to date.

From the description I have given of this operation it will be noted that it differs from Young's operation not in principle but in the method of approach. Whereas Young has to feel his way to the base of the bladder, by the suprapubic route this part is under direct observation. In addition, the bladder base and seminal vesicles are readily accessible if attacked from above, but Young in his perineal dissection finds himself working at the end of a tunnel, when he comes to the division of the malignant mass at the intra-ureteric bar. Again Young admits permanent incontinence of urine as a possible complication. By the suprapubic operation I have neither seen it nor will I admit that it can occur.

Before leaving the subject of the surgical treatment of the malignant prostate the indications for permanent suprapubic cystotomy should be briefly stated. I only practise this procedure when there is retention of urine, and radical operation is contra-indicated as determined by methods I have already outlined. It should be clearly understood that cystotomy is performed really to relieve retention. The obstruction of the ureteric orifices cannot be affected by such palliative treatment.

## PART II.

THE classic experiment of Becquerel showed that the rays from uranium are able to discharge electrified bodies, and that the three distinguishing features of X-rays, namely, their photographic, ionizing and fluorescent actions, can, therefore, be exhibited spontaneously by such a body as uranium. This discovery was followed by that of radium itself by Mme. Curie; many observers all over the world have concentrated their efforts in seeking out how these rays will act upon the human tissues, normal and pathological.

In this field of research we owe most to Dominici, who first demonstrated the destructive effects on malignant cells of radium buried in the growth. He and other workers have also been able to show that the action of these rays is selective, and that the vulnerability of cells of abnormal growth varies according to age and type. Thus the squamous-cell carcinoma is more sensitive to the effects of irradiation than the spheroidal-cell form, and the spheroidal-cell form more than columnar-cell carcinoma. Again, it has been shown that the more rapidly growing a tumour, the smaller is the lethal dose of radium needed. Lastly, we have to note the attempts of Russ and his co-workers, by means of certain ingenious experiments, to produce in man an immunity to cancer through irradiation of tissues. As malignant disease in the prostate is of the spheroidal-cell type and slow growing, it will be gathered from the preceding remarks that its sensitiveness to the rays of radium is not pronounced; also its anatomical position adds to the difficulties attaching to this form of treatment, for in its vicinity are four tubes, namely, the urethra, the two ureters, and the rectum.

Now the methods of applying radium to this organ are: (1) Internal application (*a*) per rectum; (*b*) per urethra. (2) Burying of radium in the growth. (3) Application of radium following a radical operation. (4) Surface application.

(1) As regards internal application, the radium is inserted into the rectum, and, as far as possible, kept in contact with its anterior wall in the region of

the base of the bladder, or, it may be introduced into the prostatic urethra by a special instrument devised by Young and Fronz of Baltimore. Both these methods are to be deprecated on account of the reaction produced in the mucous membrane. Pain and tenesmus follow the introduction of the radium into the bowel and cause great distress to the patient. A similar disadvantage is encountered when it is placed in the prostatic urethra. The urethra becomes inflamed, frequency of micturition is marked, and retention is often the terminal condition.

(2) I first adopted the burying of radium in the growth on my return from Paris in 1913, where I had the opportunity of seeing Dominici at work. The details of this technique are given in full in the two articles I wrote for the *British Journal of Surgery*, in 1915 and 1920.

Since 1919, the Americans have been trying the effects of emanation needles and tiny glass tubes containing the emanation. Both needles and glass tubes are buried in the substance of the gland. I have had ten years' experience of this method of application, and during this time I have been enabled to keep in close touch with fifteen cases. Not a single case is alive to-day.

The immediate results following the treatment were, without exception, satisfactory. The tumour shrank in size and in some cases disappeared altogether. There was a reduction in the frequency of micturition, and in two cases of retention normal micturition recommenced. These results were very encouraging. However, as months went by, patients returned complaining of increasing difficulty in passing water. Later, some of them developed retention requiring suprapubic cystotomy.

From accounts sent to me by relatives, and from my own observations, the patients in all these fifteen cases, with the exception of one, eventually died of pyelonephritis. The patient excepted was a doctor who died of cardiac failure. In two cases I was able to obtain permission for a post-mortem. In both cases suprapubic cystotomy had been performed. The prostate and base of the bladder consisted of dense fibrous tissue, the ureteric orifices were contracted and the kidneys were atrophic.

The practice of inserting radium into the broad ligaments for carcinoma of the uterus, which is advocated by certain workers with this agent, may be attended by similar results to the bladder-base and ureters. Occlusion of the ureters by fibrous tissue is as dangerous to the life of the patient as malignant disease, and is equally insidious in its early manifestations.

Recently in a case of carcinoma of the cervix which came under my care for vesico-uterine fistula, the radium had completely destroyed the malignant growth, but left the ureters embedded in fibrous tissue. Both kidneys as a result were badly damaged.

Of my fifteen cases of prostatic cancer treated by radium, only one patient survived four years. In the case of this patient, it was necessary to perform a cystotomy for retention, and eventually he died of pyelonephritis.

That radium destroys the malignant mass I have not the slightest doubt, but, as I have already pointed out, the anatomical position of the prostate is the stumbling block. The stimulation of connective-tissue cells by the rays and the resulting formation of dense fibrous tissue lead to serious complications in the ducts which lie adjacent. The prostatic urethra contracts and gives rise to retention, the ureteric orifices become narrowed, with the consequent complications in the kidneys and, to quote a well known French expression, "La malade mort guérie."

(3) The method of applying radium after a radical operation has a strong



adherent in Bumpus of the Mayo Clinic, but I submit that so applied the rays may cause the very complications one is anxious to avoid. The radium, to be effective in destroying any malignant cells left behind after the perineal or suprapubic operation, must be inserted into the wound immediately in front of the rectum. Now, radium produces thrombosis, and therefore the danger of causing a recto-vesical fistula would be very real. Secondly, there would still be the possibility of narrowing the ureteric orifices by the formation of fibrous tissue.

(4) The effects of surface applications of radium may be considered in conjunction with those of the Erlangen method of applying X-rays to such a deep-seated growth as a carcinoma of the prostate. I would preface my remarks on this subject by quoting from an article recently written by Professor Russ, who states: "The importance of studying the normal tissue reaction of the host in radiological problems has not received sufficient attention from physiologists." To the latter must be added the clinicians.

In the Erlangen and surface radium treatments the rays of greatest intensity must penetrate healthy tissues before they reach the carcinomatous prostate, and radiologists admit that, though it is possible to protect the skin, they cannot safeguard the deeper tissues from damage.

In this connexion the researches of Mottram sound a note of warning, which clinicians should not ignore. He has shown that large doses of radium or X-rays quickly produce desquamation and necrosis of the intestinal mucosa and he points out how the anæmia and thrombopænia, which so often follow exposure to these rays, may possibly be connected with a bacterial invasion of the blood-stream occurring in the intestinal tract as a result of injury there.

I also make the suggestion that the growth-activating substances which the cancer cell has the power of elaborating for itself and which the normal cell also forms—but only when its protoplasm is undergoing degeneration—may be increased by powerful irradiation. In this way may be explained the reason why carcinoma in some cases rapidly disseminates, following exposure to radium or the X-rays.

I am not satisfied that any advance in the successful treatment of prostatic carcinoma will be made by the Erlangen method. Those cases that I have seen have been disappointing. The growth appears to have been stimulated rather than retarded. In one case the constitutional disturbance was profound, and ulceration of the skin over the sacrum occurred within two months of the treatment, which consisted of an exposure of five to six hours to the rays; there was retention of urine and four months later the patient was dead.

I have observed that, like the intestinal mucosa, the mucous membrane of the bladder tends to undergo degenerative changes when exposed to these large doses, and when sepsis is present in the form of cystitis the rays cause a further extension of the infection. Similar results are obtained in urethritis, as I have already recorded in the *British Journal of Surgery*.

It will be admitted that many disappointments have awaited those of us who have put into practice in the wards the results of our labours in the laboratory, and I must confess that, in spite of the knowledge acquired of the effects of irradiation upon normal and malignant cells, so long as the formation of fibrous tissue cannot be restricted with certainty radium or X-ray therapy will not be the treatment of choice in prostatic cancer.

I have therefore attempted in the surgical procedure, which I have outlined in the first part of this paper, to bring the operation of excision of cancer of the prostate into line with the modern conception of a radical removal of a



52      Morson: *Treatment of Carcinoma of the Prostate*

malignant growth as it is practised in the breast, the tongue, the uterus, and the bowel.

The immediate results, like those of radium treatment, are encouraging; I trust that ten years hence a more satisfactory report of the effect of surgical removal will be forthcoming than I have been able to give in the case of radium-therapy.

## Section of Urology.

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### The Cholesterol Content of the Blood in Relation to Genito-urinary Sepsis.

By WILLIAM MACADAM, M.D., M.R.C.P., and CECILIA SHISKIN, M.D. Leeds.

(ABSTRACT.)

[This paper is published *in extenso* in the *British Journal of Surgery*, Oct., 1924.]

IN the surgical treatment of cases of obstruction in the lower urinary tract, especially that due to prostatic enlargement, there are two aspects of the patient's general condition which have to be considered in deciding as to the nature of the operative procedure required, and as to the most favourable time for the successful performance of such, viz :—

(1) The degree, if any, of defective renal function present. (2) The capacity of the patient to resist the spread of sepsis to the upper urinary tract and renal parenchyma, since a local focus of infection in the nature of a cystitis, of greater or lesser degree, is almost invariably present in such cases.

This second question appears to be the more important from the surgical standpoint, while it is one in regard to which clinical examination alone often fails to give a satisfactory answer. The present paper is a contribution to the study of this problem. Both experimental research and clinical observation suggest that cholesterol plays an important rôle in relation to the processes of immunity. An attempt, therefore, has been made to determine whether the cholesterol content of the blood might be utilized as a measure of the capacity for antibody formation possessed by the individual, and as a practical guide to the degree of "surgical risk" involved in operative procedures in cases of prostatic enlargement, where the clinical manifestations alone may give no such information.

Using the method of Myers and Wardell slightly modified, we found the normal cholesterol content of the blood in a series of twelve normal cases to vary between 0.191 per cent. and 0.133 per cent., with an average of 0.161 per cent. These values are in agreement with those of other workers. It has to be noted that, as the patients in all our cases of prostatic enlargement are over 50 years of age, the average normal cholesterol content should be reckoned as somewhat higher since hypercholesterolaemia is commonly associated with arteriosclerosis, granular kidney, and other degenerative conditions of the later period of life.

The blood-urea nitrogen was also estimated in all cases by Folin and Wu's method.

Eighty-eight cases of chronic obstruction due to prostatic enlargement,

confirmed at operation or on post-mortem examination, have been investigated at the Leeds General Infirmary. No selection of cases was made; indeed many more patients suffering from genito-urinary conditions were investigated, but were subsequently proved not to be cases of prostatic enlargement. Prostatectomy was carried out in fifty-six cases either as a single or two-stage operation. As for the others, they were considered unsuitable for major operation, sometimes on clinical grounds alone, in many instances as a result of our laboratory investigations. This applies especially to the later cases, as we gained confidence in expressing an opinion as to the surgical risk of prostatectomy from our cholesterol estimations.

Twenty-seven of the patients died, and an autopsy was obtained in all except two cases. It has thus been possible to correlate fully our findings with the degree and distribution of sepsis present, if any.

Our conclusions, briefly, are:—

(I) Out of eighty-eight cases of urinary obstruction due to enlarged prostate, eighteen gave a low blood cholesterol value (below 0.126 per cent). Of these, sixteen died of pyelonephritis and two recovered. Whereas of the other eleven deaths in the series in which the blood cholesterol was 0.130 or higher, only one death was due to an ascending urinary infection.

(II) In approximately only 50 per cent. of the patients with a low cholesterol value who subsequently died of pyelonephritis did clinical opinion of the general condition of the patient contra-indicate operation; moreover, all those cases showed, in addition, more or less defect in renal function upon which, rather than upon the latent sepsis, the clinical manifestations depended.

(III) Thus no certain information as to the resistance of the patient to such post-operative spread of sepsis is afforded in many instances by considering the patient's clinical condition alone. Nor does the degree and type of local sepsis in the nature of the cystitis which is present in greater or less degree in all such cases provide reliable information as to the powers of resistance of the individual. In cases of marked cystitis both high and low cholesterol values were obtained.

(IV) It is suggested that a low "blood cholesterol" in a case of acute or chronic retention due to prostatic enlargement is significant of a low capacity for antibody formation, and points to the case being a "bad surgical risk" as regards prostatectomy.

(V) Genito-urinary sepsis with the risk of ascending infection is of more importance to the surgeon than is nitrogen retention from defective renal functions. A high blood-urea figure alone is not serious as to ultimate prognosis; since such cases, if the blood cholesterol was normal or high, gave a normal blood urea after a period of suprapubic drainage. When prostatectomy was postponed until such nitrogen retention in the blood had disappeared, uniformly successful results were obtained, and a more rapid convalescence ensued.

On the other hand, the combination of a high blood-urea and a low cholesterol content is of very serious prognosis, and points to an undoubtedly bad surgical risk. In all such cases (seven in number), except one in our series, the patients died.

#### DISCUSSION.

Mr. J. F. DOBSON said that these investigations had been carried out by Dr. MacAdam from an absolutely unselected series of cases under different surgeons: they were nearly all hospital patients. He drew attention to the great difference in the treatment and the results of enlarged prostate in private patients and in hospital

patients. In the hospital class of patient it was very difficult, if not impossible, to carry out any satisfactory palliative treatment and sometimes it was necessary to do a prostatectomy in cases which might usually be considered unsuitable. For that reason it was justifiable, both for the surgeon and the patient, to run considerable risks. It was noticeable that in Dr. MacAdam's series the deaths which occurred in cases with a high cholesterol content were mostly from such conditions as cardio-vascular disease, chronic nephritis, pulmonary complications, &c. These cases were running a legitimate risk. He would never regret having done an operation in such a case in which, the local conditions proving satisfactory, the patient succumbed to cardiac or pulmonary complications. The cases with a low cholesterol content were in a different category: of eighteen of these, sixteen died, some merely after catheterization or cystotomy. In the case of these with low cholesterol figures, cystotomy and catheterization were equally fatal. Here, it appeared, they had to deal with a class of case in which the risk of any treatment was extreme. And it had to be noted that in only 50 per cent. of the cases would it be said that the case was a poor operative risk from clinical considerations alone.

It was not clear what this low cholesterol content indicated; whether it meant that the kidneys were already badly infected, or that the resistance of the patient to infection was so low that after any surgical procedure whatever he succumbed to a rapidly extending renal infection. Much further investigation of these cases was necessary. It appeared that in the majority of them an early fatal issue was inevitable and that both catheterization and cystotomy were equally dangerous. He mentioned the need, in this connexion, for further investigation into the value of urinary anti-septics. The occurrence of these advanced and practically hopeless cases which formed a definite proportion of our hospital cases could only be avoided by earlier diagnosis, earlier surgical treatment and the avoidance, as far as possible, of catheterization.

## Section of Urology.<sup>1</sup>

President—Dr. W. LANGDON BROWN.

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### Specimen of a Kidney removed Five Months after Decapsulation.

Shown by KENNETH WALKER, F.R.C.S.

THE decapsulation was not undertaken for chronic nephritis but for an ascending infection following a gynæcological operation. At the time of the operation a large, œdematous, and congested kidney was found and was decapsulated with a view to relieving the intense pain from which the patient was suffering. The relief was immediate and the patient made a more or less uninterrupted recovery, but subsequently complained of tenderness and occasional pain. Expectant treatment failing, the kidney was removed by another surgeon. During the five months that elapsed since the first operation a new fibrous capsule formed which was indistinguishable from the original capsule.

I have had other cases in which I have been compelled to remove a kidney after decapsulation for chronic pain and tenderness, although it is difficult to say whether these symptoms have not been due to the infection for which the decapsulation was undertaken rather than to the decapsulation itself. I should be interested to hear whether others have discovered any relation between a tender kidney and decapsulation.

### Operation Specimens.

Shown by FRANK KIDD, M.Ch.

#### I.—CAVERNOUS ANGEIOMA OF KIDNEY. EMERGENCY NEPHRECTOMY FOR GRAVE HÆMATURIA.

B, MALE, aged 19.

*History.*—February 11, 1924: Since 1914 has had attacks of profuse hæmaturia. Two days before I saw him he was seized with profuse hæmaturia, clot retention and violent pain in the right kidney. He fainted and vomited and filled a whole chamber with clots before he got retention. He had to be catheterized and the clots washed out.

February 13, 1924: He was pale, his pulse-rate 110, and again he had clot retention. He was obviously so ill that an urgent operation was needed and this was carried out at once.

<sup>1</sup> Clinical and Pathological Meeting, held May 29, 1924.

*Operation.*—Cystoscopy—blood pouring out of the right ureter, clear urine coming from the left ureter. Indigo came through from the left ureter in eight minutes. Right nephrectomy.

*Progress of Case.*—No more bleeding: healed by first intention: no further trouble.

*Histological report.*—"This kidney had a double ureter. On section the lower calyces were dilated and filled by blood clot, especially the one near the centre of the organ. Outside this the medullary substance was dissected up by free hæmorrhages and several other hæmorrhagic areas were present but of less degree. Sections cut through the place which was evidently the chief source of the hæmorrhage show masses of free blood just beneath the epithelium of the calyx. This free hæmorrhage complicates the histological picture considerably. There is, however, definite evidence of a so-called cavernous angioma, more properly described as an angiectasis. Large simple sinus-like spaces are present, separated by bands of fibrous tissue forming a sort of net-work. The kidney tissue proper, except in so far as it is congested or affected by the free hæmorrhages, is healthy. There is no sign of any inflammatory process or new growth proper."

## II.—HYDRONEPHROSIS WITH DOUBLE PELVIS, ONE PORTION OF WHICH SUPPLIED A SINGLE CALYX.

P. C., MALE, aged 33.

*History.*—May 27, 1921. Since August, 1920, patient had had severe attacks of pain in the left loin, accompanied by hæmaturia. Ordinary X-ray examination shows no stone.

July, 1923. Patient refused further examination until 1923. Pyelography, July, 1923, showed undoubted hydronephrosis of the left kidney. Indigo came from the right kidney in twenty minutes.

*Operation.*—July 25, 1923, from the front. Left nephrectomy.

*Progress of Case.*—Healing uneventful, no further trouble.

*Specimen* consists of a congenital hydronephrosis with a too high valvular insertion of the ureter and a double arterial and venous supply. The dilated pelvis splits into two portions, the smaller part of which runs and supplies one calyx only.

## III.—PAPILLOMA OF BLADDER. PARTIAL CYSTECTOMY WITH EXCISION OF LOWER END OF URETER.

S. S., MALE, aged 58.

*History.*—July 9, 1923. Hæmaturia for nine months. Cystoscopy—malignant papilloma on the right side of bladder.

*Operation.*—August 4, 1923. Partial cystectomy, the right ureter and a large portion of bladder wall removed with the tumour.

*Progress of Case.*—Healing uneventful.

Patient last seen March 14, 1924. No sign of recurrence of the tumour.

*Histological Report.*—"This was a large, soft, pedunculated villous growth. The stalk of the growth is very narrow, hardly thicker than a pencil. A complete section (in two parts) of the growth, the pedicle and the underlying bladder has been cut. The villous growth has the usual histological features of a vesical papilloma. The pedicle is entirely free from invasion. Much of the

58 Kidd : *Specimens* ; Nitch : *Spicate Oxalate Renal Calculi*

tumour exhibits commencing necrobiotic changes, especially towards the centre of the growth, and it is ulcerated on the surface. There is no histological evidence of malignancy."

IV.—MALIGNANT TUMOUR OF TESTICLE.

D. H., AGED 28.

*History*.—May 19, 1924. For eighteen months has noticed the left testicle getting larger. Physical condition—a solid elastic heavy swelling of the left testicle, not translucent. Wassermann reaction completely negative.

*Operation* this morning. Hinman's incision. Cord exposed and clamped and burnt across with a cautery. Tumour removed and examined with the naked eye before proceeding further. Cord pushed through into the abdomen, peritoneum not opened but pushed inwards towards the middle line, vas ligated. Spermatic vessels, with fascia, cleared up to the left renal vein, where a single small gland was encountered. Spermatic artery and vein tied and the gland removed in one piece with the vessels and fascia. No other glands could be detected along the aorta.

**Spicate Oxalate Renal Calculi.**

Shown by C. A. R. NITCH, F.R.C.S.

W. J. L., MALE, aged 50. P.C.

Radical cure of double inguinal hernia in 1915. Hernia recurred on the left side and was operated upon again in November 1923. Three months after the second operation, began to suffer with pain of a dull aching character, just above the upper limit of the scar. As time went on the pain changed its character and occurred with acute exacerbations immediately after he commenced to walk, and was at times so acute that he was obliged to drive home and lie down; in this way he obtained relief in about thirty minutes. Coughing, jolting and straining did not bring on the pain. No urinary symptoms; no trouble with the bowels, but suffered from considerable flatulence during an attack. He located the pain to a horizontal line about three inches long on a level with the highest point of the crest of the ileum, the centre of the line being at the outer border of the left rectus.

*On Examination* : A large, powerful, healthy-looking man. No signs of recurrence of the hernia. No physical signs of any sort. No enlargement of the kidney and no loin tenderness.

*Cystoscopy* : Bladder and ureteric orifices healthy. Both ureters catheterized. The urine collected from both sides was normal, sterile and free from pus, crystals and debris.

*X-ray Examination* showed two shadows in the left kidney and an opaque bougie was seen to reach over the highest shadow and to come in contact with the lowest one. Both shadows were spicate.

*Operation* : Kidney exposed by oblique lumbar incision. Pyelo-lithotomy : one large spicate stone being removed from the pelvis without any difficulty. A second similar stone, occupying a dilated calyx in the lower pole, was easily displaced and removed through the pelvis. It was then found that there was a third small stone firmly impacted in one of the lower calyces. This was dislodged with a director, which was pushed through the cortex over it and was then removed through the pelvis.

The stone was examined chemically and found to be composed of calcium oxalate.

## Nephrostomy of Solitary Functional Kidney of Nine Years' Duration.

By C. A. R. NITCH, F.R.C.S.

D. D., FEMALE, aged 28.

In 1911, when aged 15, she underwent a plastic operation for bicornuate uterus with an abscess in one horn. Three years later she was admitted to St. Thomas's Hospital with an offensive vaginal discharge which commenced shortly after the operation. The uterus was found to be full of pus and a total hysterectomy was performed. During the operation, which was rendered very difficult by dense adhesions, the bladder was injured and a urinary fistula formed. Three months later (March, 1915), after a period of pyrexia and oliguria, the left kidney became enlarged and tender, and was drained through the loin by Mr. W. H. Battle. The urine contained pus and *Bacillus coli* in pure culture. After the nephrostomy no urine was passed from the bladder, and it was therefore concluded (1) that the right kidney was either absent, or (2) had ceased to function after the hysterectomy, and (3) that the left ureter had become partially and finally totally obliterated by scar tissue. The left kidney was therefore drained permanently by a long silver tube which passed through its cortex into the pelvis. This tube has been worn for nine years and has kept the patient quite dry.

In April, 1920, Mr. Battle asked me to examine her with a view to re-establishing the connexion between the ureter and the bladder. Although no urine had entered the bladder for five years, a 4-oz. distension was obtained under an anæsthetic and a good view was obtained with a cystoscope. The mucous membrane was healthy. The right ureteric orifice was not seen even after a prolonged search. The left ureteric orifice was patent, but a catheter would not pass farther than 2 cm. *Per vaginam* a dense mass of scar tissue was felt at the base of the bladder. The presence of this scar tissue combined with a solitary functional kidney, which was undoubtedly the seat of a septal pyelonephritis, led me to decide against surgical interference.

She then left the hospital, was married, and went to live in Kenya Colony, where she states that for a time she was in perfect health. About two and a half years ago hæmaturia commenced, and has gradually increased. She lost weight and suffered from lassitude. For the past six months, before she left Mombasa for England, the hæmorrhage has been severe, and on occasions almost pure blood has escaped from the tube. For nine months her temperature has ranged between 99° and 101° F., with normal intervals. She has occasional headaches, and has lost her appetite. She thinks that blood may come from some part of the loin sinus, as pain is felt at one spot during the introduction of the tube; but the probabilities are that hæmorrhage is due either to a traumatic ulcer at the point where the end of the silver tube comes in contact with the interior of the kidney, or to a malignant growth following constant irritation. A pyelogram, besides showing a dilated kidney and a short length of dilated ureter, also shows that the natural curve taken by a rubber tube differs from the curve of the silver tube.

A cystoscopic examination performed on May 27, 1924, does not afford any



## 60 Loughnane: *Specimens Illustrating Removal of Testicle*

more information than that already recorded, with the exception of the fact that the bladder contained a quantity of mucin, and would not hold more than 2½ oz. of fluid under deep anæsthesia.

Blood urea = 31 mg. per 100 c.c.

Urea concentration test = 1·9 per cent. in third hour.

### **Four Specimens Illustrating the Removal of Testicle, Vas and Seminal Vesicle, "En Bloc," for Tuberculous Disease.**

Shown by F. MCG. LOUGHNANE, F.R.C.S.

I.—H. C., AGED 22, operation September 1, 1914.

Right testicle, vas, and seminal vesicle enlarged, tender, and very palpable. Symptoms of cystitis present. Cystoscopy showed inflamed vesical mucous membrane. Inguinal incision. Testicle with vas separated down to seminal vesicle extraperitoneally, and removed in one piece, excepting a part of seminal vesicle which was separately removed. Urine escaped through wound for fourteen days. Seen in August, 1916; patient was in good health, had put on weight, and was free from symptoms.

II.—E. L., aged 14, operation June 21, 1915.

Patient had been in sanatorium for twelve months for phthisis. Seen a week after discharge, and was found to be suffering from tuberculosis of the right hip, tuberculous abscess below the knee, and enlargement of the left epididymis and seminal vesicle. Inguinal incision. Testicle, vas, and seminal vesicle removed in one mass extraperitoneally. Wound drained for forty-eight hours, no urine discharged. Abscess below the knee opened and closed fourteen days later, and the hip was put in plaster. Patient seen three months later; was doing well.

III.—G. L., operation 1915.

Two months' history of swelling of right testicle, with sinus, no pain. Right and left seminal vesicles enlarged and tender, right more so than the left. Inguinal incision. Skin, testicle, vas, and seminal vesicle removed extraperitoneally. Wound drained, no escape of urine.

IV.—Tuberculosis of right and left testicle with thickening of both seminal vesicles, sinus over left testicle. Operation in 1921, left orchidectomy. In 1923 sinus over right testicle. Operation: Testicle, vas, and both seminal vesicles removed through right inguinal incision. Healed by first intention.

## Section of Urology.

President—Dr. W. LANGDON BROWN.

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### Lipoid Speckling of the Renal Cortex—the so-called “Myelin Kidney.”

By F. PARKES WEBER, M.D.

MY own interest in this subject was originally derived from early attempts to ascertain the nature and cause of various kinds of pathological specks and streaks observed by me in kidneys at post-mortem examinations. I shall therefore here refer to minute spots due to calcareous deposits, before discussing the appearance and nature of “myelin kidneys.”

The renal cortex may be marked with minute opaque whitish dots and streaks owing to the deposition of calcium salts in diseases associated with the absorption of bone and bone salts, especially malignant tumours. In such cases Virchow<sup>1</sup> spoke of the occurrence of “calcareous metastases” (“Kalkmetastasen”) in the kidneys, lungs and liver. Calcareous deposits—macroscopical or microscopical—in the kidneys may likewise occur in other conditions, for instance, in mercurial poisoning.<sup>2</sup> A. S. Warthin<sup>3</sup> found calcareous changes and “chalk-infarcts” in the kidneys of patients who had undergone Röntgen-ray treatment for leukæmia and Hodgkin’s disease. M. Askanazy and E. Goldschmid<sup>4</sup> found “chalk-infarcts” in the renal pyramids from cases of hepatic cirrhosis. E. W. Baum<sup>5</sup> discussed the minute calcareous deposits found in the little cysts of the cortex of granular contracted kidneys. B. Glaserfeld<sup>6</sup> regarded the occurrence of calcareous deposits in the renal cortex of children as a post-mortem phenomenon. Edwin Beer,<sup>7</sup> in his work on “Lime Deposits, especially the so-called ‘Kalkmetastasen’ in the Kidneys,” concluded: “Lime deposits in the kidneys are a very common pathological condition. Deposits heretofore classed as metastatic differ in no wise from those where no bone disease existed. . . . Age, carcinoma, sepsis and nephritis influence the production of these deposits by local changes, either

<sup>1</sup> R. Virchow, “Kalkmetastasen,” *Virchow’s Archiv*, 1855, viii, p. 103, and 1856, ix, p. 618.

<sup>2</sup> For example compare A. J. Hall, “Two Cases of Mercurial Poisoning,” *Lancet*, London, 1912, i, p. 1467. I think that it has been especially observed in poisoning with corrosive sublimate.

<sup>3</sup> A. S. Warthin, “The Changes Produced in the Kidneys by Röntgen-Irradiation,” *Amer. Journ. Med. Sci.*, Philadelphia, 1907, cxxxiii, pp. 736-746.

<sup>4</sup> Edgar Goldschmid, *Ziegler’s Beiträge z. path. Anat. u. z. allg. Path.*, Jena, 1913, lvi, p. 77.

<sup>5</sup> E. W. Baum, “Ueber die punktförmigen Kalkkörperchen (sogen. verkalkte Glomeruli) der Nierenrinde,” *Virchow’s Archiv*, 1900, clxii, p. 85.

<sup>6</sup> B. Glaserfeld, “Das Vorkommen von Kalk in den Rindengefässen der kindlichen Niere,” *Virchow’s Archiv*, 1907, clxxxviii, p. 92.

<sup>7</sup> Edwin Beer, *Journal of Pathology*, Cambridge, 1903, ix, pp. 225-233.

nutritional or toxic. Deposits are most frequent in cystic and fibroid Malpighian bodies, less frequent in cystic tubules and their epithelium."

Somewhat macroscopically similar opaque white or yellowish-white specks and streaks constitute, however, a remarkable feature in the naked-eye examination of the renal cortex in another class of cases, namely in the so-called "myelin kidney" or lipoid degeneration of the kidney. The specks and streaks in such kidneys are due to infiltration with a lipoid substance of the kind originally named "myelin" by Virchow in 1857. The lipoid substance in microscopical sections takes on a brownish-pink colour when stained with

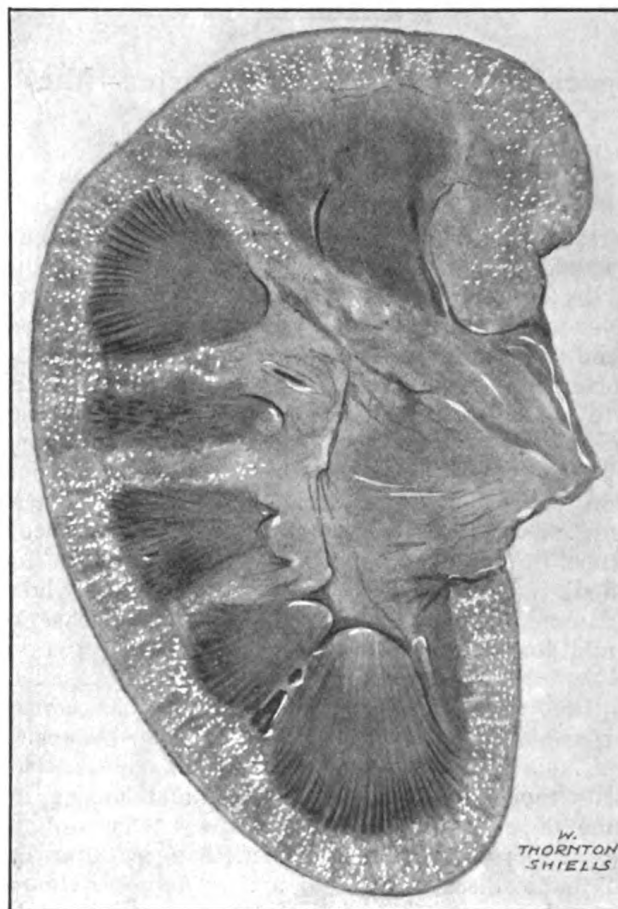


FIG. 1.—The naked-eye appearance of a preserved "myelin-kidney." In the freshly removed organ at the necropsy the appearance was more striking.

Sudan III, and by polariscopic examination with crossed Nicol's prisms can be shown to be doubly refracting (anisotropic) to light. This indicates the presence of anisotropic fatty acid cholesterin esters (*cholesterin* and *cholesterol* are words now used quite synonymously). J. W. McNee,<sup>1</sup> to whom I am

<sup>1</sup> J. W. McNee, "On Lipoid Degeneration of the Kidney and the so-called 'Myelin Kidney,'" *Journ. Path. and Bact.*, Edinburgh, 1922, xxv, pp. 425-432. Cf. also S. S. Chalataw, *Die anisotrope Verfettung im Lichte der Pathologie des Stoffwechsels (Die cholesterindiathese)*, Jena, 1922, pp. 173-178 and pp. 191-192 (on "myelinosis" of the kidneys and on "xanthomatosis" of the renal stroma).

specially indebted and to whose recent paper I would refer for previous literature on the subject, points out that the doubly refracting substance is deposited commonly enough in amounts which do not alter in any characteristic fashion the naked-eye appearances of the kidneys, but that the term "myelin kidney" is applicable when it occurs in sufficiently large amount to give the organ a characteristic and easily recognizable appearance.

The doubly refracting lipoid substance (always associated with neutral fat) is deposited chiefly in the interstitial tissue<sup>1</sup> of the cortex, within cells which



FIG. 2.—To illustrate the "foamy cells." Paraffin section from the cortex of a "myelin-kidney" ( $\times 250$ ). It shows a collection of the characteristic cells with "foamy" cytoplasm ("Schaumzellen") situated amongst the blood and lymph capillaries in the interstitial tissue between the renal tubules. The cells in question mostly have small rather pyknotic nuclei and abundant "vacuolated" cytoplasm, from which the doubly refracting lipoid substance (cholesterin esters) has been dissolved out by the alcohol during the process of preparation. In some of the cells the nucleus cannot be seen, apparently having been separated with a portion of the cell by the cutting of the section.

in ordinary paraffin sections have a "foamy" cytoplasm (whence the term "Schaumzellen" or foam-cells) and resemble the "xanthoma" type of cells, as seen in cutaneous xanthoma, arterial atheroma, &c. The interstitial tissue

<sup>1</sup> See, however, S. Wail, "Ueber Veränderungen der Lokalisation und des Chemismus der Lipide in den Tubuli contorti der Niere," *Virchow's Archiv*, 1924, ccxlix, p. 488. In all probability the anisotropic fatty infiltration occurs *primarily* in the secreting cells of the renal tubules (the convoluted tubules), that is to say, as a parenchymatous change termed "myelinosis" by Chalataw (loc. cit.), the interstitial change, termed "xanthomatosis" by Chalataw, being a secondary process. The "myelinosis" was admirably demonstrated at the meeting in specimens kindly shown by Dr. S. C. Dyke.—F. P. W.

cells which take up the doubly refracting substance (cholesterin fatty acid esters) probably belong to the class of phagocytic "resting *Wanderzellen*" (amœbocytes," the "macrophages" of Metchnikoff, the "clasmatoocytes" of Ranvier, the "adventitial cells" of Marchand, the "polyblasts" of Maximow, the "histiocytes" of Aschoff and Kiyono), which form part of the "reticulo-endothelial cell-system" of Aschoff and Landau, practically the same as the system of "pyrrhol cells" of Ribbert and Goldmann.

In my recent case of "myelin kidney" the patient (F. S.) was a young man, aged 20, who was in hospital for a few weeks in July and August, 1923, with signs of chronic nephritis, suggesting the presence of "large white kidneys" with chronic interstitial nephritis superadded. The history was that his illness commenced in January, 1923, and that he had had œdema, notably at first in the face. Whilst he was in the hospital (July to August, 1923) the urine was excessive in quantity, of rather low specific gravity (1005-1010), and contained albumin (at first, 4 to 6 per mille by Esbach's tube) and a fair number of hyaline and granular tube-casts. The brachial systolic blood-pressure was raised (177-199 mm. Hg), the heart was enlarged to the left, and ophthalmoscopic examination showed the presence of definite "albuminuric" neuro-retinitis, with white spots in the macular region. The Wassermann reaction was not taken. He was readmitted on January 3, 1924, with uræmia and recurring uræmic convulsions. He passed no urine, and in spite of treatment (venesection, lumbar puncture, &c.), he died on the following day. It should be noted that he likewise had signs of acute pulmonary œdema, with frothy (but not blood-stained) expectoration.

The *necropsy* (Dr. G. Welsch) showed pulmonary œdema and an enlarged heart (weighing 17 oz.) with great hypertrophy of the left ventricle. There was no excess of cerebro-spinal fluid in the cranium. The *kidneys* (weighing together 12 oz.) were slightly enlarged, rather pale, with reddish patches. The capsules separated readily. The whole cortex of both kidneys was speckled and streaked with innumerable small opaque white or yellowish-white spots and lines—a typical feature of so-called "myelin kidneys." Especially noticeable were the closely packed parallel fine opaque white or yellowish-white lines running through the breadth of the cortex from the medulla to the outer surface—which was characteristically "speckled" or "flecked." Fig. 1 represents the naked-eye appearance of one of the preserved kidneys, but the appearance of the freshly removed organ at the time of the post-mortem examination was more striking. By *microscopical examination* (for which I am indebted to the kindness of Dr. J. W. McNee) the kidneys showed the glomerular and other changes of chronic interstitial nephritis, together with so-called "chronic parenchymatous" changes, but the especially interesting feature was the presence of groups of "foamy cells" (*Schaumzellen*) with small pyknotic nuclei (*see fig. 2*), these cells being placed interstitially in the cortex, that is to say, in connexion with the lymphatic radicles. In sections cut by the freezing method the cells in question were found to be infiltrated, like "xanthoma cells," with a substance which took on the characteristic lipoid (brownish-pink) staining with Sudan III and (examined by crossed Nicol's prisms) was doubly refracting to light (anisotropic cholesterin esters). In ordinary paraffin sections the abundant cytoplasm of these cells had a "foamy" or "vacuolated" appearance, owing to the lipoid substance having been dissolved out by the alcohol during the process of preparation (*see fig. 2*).

A few teaspoonfuls of urine found in the bladder at the post-mortem examination contained granular and hyaline tube-casts, and  $4\frac{1}{2}$  per mille

albumin (by Esbach's tube). Unfortunately no special examination of the centrifuge-sediment with crossed Nicol's prisms for "lipoid casts" was made.

In 1904 in the kidneys of a woman (M. H.), aged 51, I saw similar "large clear cells," that is to say, cells with "foamy" cytoplasm (in ordinary paraffin sections), arranged in groups here and there between the renal tubules. These cells were doubtless infiltrated with doubly refracting lipoid (cholesterin esters), but I did not examine them with Nicol's prisms. The patient in question had old quiescent pulmonary tuberculosis (with secondary pulmonary fibrosis) and chronic nephritis. She died in hospital in October, 1904, with uræmic convulsions and apparently commencing lobar pneumonia. The kidneys were rather large and pale and of very firm consistence, the cortical surfaces were granular, and the capsules could not be stripped off without tearing away portions of the cortex; microscopically there were parenchymatous and slight amyloid changes present, as well as chronic interstitial nephritis. By naked-eye examination minute opaque yellowish-white specks and streaks were observed in the cortex (notably on the surface of the cortex) of both kidneys,<sup>1</sup> and this appearance of "speckling" was probably due to the clusters of the large cells in question seen under the microscope.

It seems that "myelin kidneys" are generally kidneys of old nephritic cases in which mixed "chronic parenchymatous" and secondary chronic interstitial lesions are present. McNee writes of his three cases:—

"In all three examples of myelin kidney which have been described, there was found a late stage of glomerulo-nephritis, with commencing interstitial changes and degeneration of the tubular epithelium. In such circumstances, the more usual sequel would be the production of a 'large white kidney,' with abundant neutral fat in the tubular epithelium and only a small amount of doubly refracting fat."

The possible prognostic value of the discovery of lipoid tube-casts in a nephritic patient's urine—by examining the centrifuge-sediment with crossed Nicol's prisms—is emphasized by McNee, after F. Munk's suggestion (1913). McNee thinks that the finding of only a few lipoid tube-casts does not point to "myelin-kidney," for *all* cases of "parenchymatous nephritis" show some lipoid in the kidney, but when great numbers of lipoid tube-casts are present in the urine the diagnosis of regular "myelin kidney" becomes fairly certain.

In connexion with the deposition of cholesterin esters in "myelin kidneys" one should remember the frequency of hypercholesterinæmia in uræmic and chronic nephritic cases,<sup>2</sup> just as in patients with cholelithiasis, nodular cutaneous xanthoma, arterial atheroma and arteriosclerosis. The white spots in the retina (especially in the macular region) in so-called albuminuric retinitis have been found to contain anisotropic cholesterin esters.<sup>3</sup> A kind of experimental myelin kidney can be produced in rabbits by cholesterin feeding.<sup>4</sup>

<sup>1</sup> Cf. F. Parkes Weber, *Clinical Journal*, London, 1917, xlv, p. 118. Macroscopically the appearance of the kidneys in this case reminded me of what I described as "Speckled Kidneys" in the *Transactions of the Pathological Society of London* for 1898 (xlix, p. 152).

<sup>2</sup> A. Chauffard, G. Laroche, and A. Grigaut, "Le taux de la cholestérinémie au cours des cardiopathies chroniques et des néphrites chroniques," *Comptes rendus de la Soc. de Biologie*, Paris, 1911, lxx, p. 108. Cf. D. Klinkert, *Berliner klin. Wochenschrift*, 1913, I, p. 820.

<sup>3</sup> H. Lauber and V. Adamiik, "Ueber das Vorkommen von doppelbrechendem Lipoid in der Netzhaut bei Retinitis albuminurica," *Arch. f. Ophthalmol.*, Leipzig, 1909, lxxi, pp. 429-465; S. Ginsberg, "Ueber das Vorkommen lipoider Substanzen im Bulbus—ein Beitrag zur Kenntnis der Retinitis albuminurica," *ibid.*, 1912, lxxxii, pp. 1-57.

<sup>4</sup> Cf. C. H. Bailey, "Atheroma and other Lesions produced in Rabbits by Cholesterol Feeding," *Journ. Exper. Med.*, New York, 1916, xxiii, pp. 69-85, figures 7 and 8. Cf. also S. S. Chalataw, *op. cit.*, and references given by him.

## DISCUSSION ON POLYCYSTIC DISEASE OF THE KIDNEYS.

Sir THOMAS HORDER, Bt.

CLINICAL observation has not, I fear, afforded me any new light that I can throw upon the pathogenesis of this disease. But I may perhaps refer briefly to three cases that I have had under my observation in St. Bartholomew's Hospital during the past three years.

*Case I.*—A man, aged 48, admitted on account of vomiting and diarrhœa. Similar attacks had occurred at intervals during the past twelve months, associated with headache and giddiness and aching pains in the back. Between the attacks the patient had noticed thirst and polyuria. There were also present cramps in the calves and feet at night, with tingling and numbness in the fingers. Just before admission there had been slight swelling of the face and feet in the early morning. The patient had suffered from scarlet fever as a child.

Examination revealed bilateral renal tumours, firm, irregular in outline, and insensitive. The output of urine was diminished during, and for a few days after, admission, a heavy cloud of albumin was present and there was slight pyrexia. The diminished urine was not due to vomiting or to diarrhœa, because these had ceased before admission. After three days, the amount of urine increased and for the remainder of the month's stay in hospital was always above the normal (1,725-2,225 c.c.) The albumin disappeared after the fourth day. The specific gravities were low (1006 to 1010) even when the amount was reduced. More urine was passed during the night than during the day. The blood-pressures were 140 and 100 respectively. The blood-urea figure was 0.172 per cent. The urea-concentration figures were 1.0, 1.1, 1.1 and 1.15 per cent. for the four successive hours of the investigation, with quantities varying between 110 and 225 c.c. The diagnosis in this case was polycystic disease of the kidneys with intercurrent subacute nephritis. I have ascertained that this man is at present at work after two years, but he still gets attacks of vomiting and diarrhœa.

*Case II.*—A woman, aged 34, was referred by me to my gynæcological colleague, Dr. Herbert Williamson, on account of loss of weight and the presence of a tumour which she had noticed in the left side of the abdomen just after the second of two normal pregnancies; this tumour I thought to be an ovarian cyst. The tumour is described in the gynæcological notes as arising from the pelvis to a height of 5 in. in the abdomen centrally situated, but extending into the left iliac region. It was mobile, irregular in outline, and insensitive. A small mass, the size of a marble, could be felt at the upper pole of the main mass. Percussion over the tumour yielded a dull note. The right kidney was palpable, the left not so. By vaginal examination, the uterus was felt to be retroflexed. Upon bi-manual examination the tumour felt *per hypogastrium* lay completely above the brim of the pelvis. It was the size of a coconut, elastic, nodular, and freely movable. It was thought to be a cystic swelling of the left ovary. A mesial incision was made with a view to its removal. On incising the parietal peritoneum the tumour was seen to be presenting in the wound; it was a polycystic kidney of considerable size (12 in. by 6 in.). An incision was made through the peritoneum covering it and the tumour was enucleated from the retroperitoneal tissues; its pedicle was clamped and it was removed. Both ovaries were then identified and found to be normal. The right kidney was palpated and was found to be slightly enlarged, giving the impression of being a hypertrophied organ, but not cystic.

The patient then came into my ward. She was suffering from an acute intercurrent coliform urinary infection, which subsided under treatment. The occurrence of this complication made it impossible to say whether the definite enlargement of the right kidney that I felt was due to polycystic disease of that organ or to pyelo-nephritis; or, as Dr. Williamson had suggested, to renal hypertrophy. The patient left the hospital six weeks after the nephrectomy. Her doctor informed me yesterday that she is now, two and a half years later, in fairly good health.

[June 26, 1924.]

*Case III.*—A man, aged 53, admitted on account of persistent vomiting, a symptom that had been present, on and off, for two years. More recently he had noticed anorexia and weakness, with morning headaches and blurred vision. On examination, in addition to these symptoms of uræmia, it was found that the breath had a urinous odour, the urine was of low specific gravity (1008-1010), pale and clear, not diminished in amount despite the frequent vomiting, and contained 0·15 per cent. of albumin with granular casts. The blood urea was 0·46 per cent. and the vomit contained 0·1 per cent. of urea. The systolic blood-pressure was 180, the diastolic 115. There were no retinal changes observed—neither effusion nor hæmorrhages. The heart was not enlarged and the arteries were not thickened. The abdominal examination revealed bilateral, large irregular masses having all the characters of renal tumours. Both masses were just large enough to bulge the flanks and hypogastria on inspection; the liver was moderately enlarged, but the contour was quite uniform. I say “but,” because knowledge of the fact that cysts are quite commonly found in this organ when examined post mortem in polycystic disease led me to examine the surface and lower edge of the liver carefully from this point of view. Cysts *were* found in this liver post mortem, but it is unlikely from their size that they could have been diagnosed during life. The vomiting persisted whilst the patient was under observation, and towards the end of the third week of admission a couple of general convulsions occurred and the patient died. The post-mortem appearances in this case will be described by Sir Bernard Spilsbury, and will, I think, add something to our knowledge of the pathogenesis of the disease.

#### GENERAL REMARKS.

These three cases confirm the few facts that we know concerning this mysterious affection, that it is not very uncommon, that it occurs in both sexes about equally, that the patients usually come under observation when they are between the ages of 30 and 50, either because they are suffering from the symptoms of chronic renal disease, both uræmic and cardio-vascular, or because they notice the presence of an abdominal tumour which causes pain or sets up obstructive symptoms, during labour, or in the bowel. The course of the disease is clearly very slow. The patients are subject to intercurrent tubal nephritis, and eventually die of uræmia when the disease affects both kidneys, as seems most often the case. If the cysts affect one kidney only, and this organ is removed, they may live for several years and enjoy a fair degree of health. I have already referred to the difficulty there sometimes is in determining whether the disease is unilateral or bilateral, and when we remember that one kidney may precede the other by a very considerable time interval, as regards definite and demonstrable enlargement, and yet the disease be existent in the smaller organ in an early stage, it becomes a matter of grave doubt whether it is justifiable to remove one of these kidneys unless (i) the tumour is causing serious symptoms, and (ii) examination of the other kidney reveals so little enlargement that it is probable that, even if affected, it will not advance to the uræmic stage for some years. In the absence of these two features it is clear that the abdomen should be closed after the exploration. In another case under treatment in the medical wards at St. Bartholomew's Hospital during the past three years,—a woman aged 46, under the care of Dr. Morley Fletcher,—the enlarged right kidney, which led to exploration by Mr. Waring, was very large, and the left, palpated during the operation, was relatively small, though obviously affected.

I have referred to the associated cystic condition of the liver found post mortem. It will be of interest to hear whether anyone has convinced himself of this part of the disease-process during life, or indeed of cysts elsewhere, such as in the pancreas, for these are described as occasionally



present. I have not come across the familial factor said to be present in some cases. Nor have I seen associated congenital defects, such as hypospadias, harelip, &c., in any case that I can remember. Although hæmaturia did not occur in either of these three cases, it is known to be a not uncommon symptom.

As I remarked at the outset, I have no new observations from which to deduce ætiological factors. From my knowledge of the disease I think it most probable that the current view of its congenital and developmental origin is correct. Has anyone had experience of drainage of the cysts as a means of treatment?

Sir BERNARD SPILSBURY.

### THREE CASES OF POLYCYSTIC DISEASE OF THE KIDNEYS ASSOCIATED WITH CYSTIC DISEASE OF THE LIVER.

The cases are the records of three patients who died recently at St. Bartholomew's Hospital. All the patients were males between the ages of 47 and 53 years.

*Case I.*—The patient became unconscious whilst at work and was admitted to the hospital with left hemiplegia: consciousness gradually returned but he died of uræmia a fortnight later. This patient had been invalided out of the Army for nephritis in 1916 and had since suffered from frequency of micturition. At the autopsy a cerebral hæmorrhage of moderate size corresponding in age with the attack of unconsciousness was found in the right internal capsule. The cerebral arteries were atheromatous.

*Case II.*—The patient had been operated upon ten years previously when a left pyonephrosis was drained: since then he had suffered from attacks of acute abdominal pain on the right side which were diagnosed as renal colic. During the last three months of his life he became progressively weaker and was very ill on admission. A large tumour was felt in the situation of each kidney; there was tenderness and rigidity in the right lumbar and iliac regions and the urine contained pus and blood. He became rapidly worse and died from uræmia ten days later. The post-mortem examination showed acute left pyelitis and acute cystitis.

*Case III.*—The patient was admitted to hospital with symptoms of uræmia. The abdomen was large and renal tumours were easily palpable. He complained of morning vomiting and occipital headache; frequency of micturition had been present for the last five years. Death occurred from uræmia three weeks after admission, and on post-mortem examination small ulcers, which were probably uræmic, were found in the cæcum and ascending colon.

In other respects the post-mortem examination of these three patients showed similar conditions. The left ventricle of the heart was hypertrophied, the heart cavities were dilated and the aorta and coronary arteries were atheromatous. The lungs were cedematous. The liver was moderately enlarged in one case, of normal size in the others; numerous small cysts were present on the surface and in the substance of the organ. Most of the cysts were  $\frac{1}{4}$  in. or less in diameter and were filled with colourless watery fluid. The function of the liver was not impaired. On microscopic examination the cysts were found to be lined by a single layer of flattened cubical epithelium supported by a fibrous capsule. Some of the cysts were connected with the portal areas and many of these areas contained multiple bile-ducts.

The kidneys were much enlarged in each case, their weights being from 23 to 53 oz.; they showed the characters of polycystic disease, being riddled throughout with cysts which measured upwards of 2 in. in diameter. The

cysts had thin walls lined by flattened cubical epithelium, and they were filled with a watery fluid or with semi-solid material of a yellow or brown colour ; some contained blood. In the second patient the cysts in the left kidney contained pus. The functional renal tissue was represented only by narrow areas, pale in colour and firm, which on microscopical examination showed a structure resembling that of chronic interstitial nephritis with marked fibrosis and atrophy, dilatation and some compensatory hypertrophy of the convoluted tubules, with hyaline degeneration of many of the Malpighian bodies and advanced arteriosclerosis ; in the third case there was also slight amyloid disease. In all the cases the suprarenal glands, and the cortex especially, were hypertrophied. There was no obstruction in the urinary tract in any of the cases.

In these cases the structure and distribution of the renal cysts lend support to the view that the cystic condition is congenital and is due to a partial failure in the fusion of the secreting tubules with the excretory ducts, a fusion which should occur at the level of the collecting tubules. This view implies that the cysts are derived from secreting tubules which have undergone a gradual distension by the secretion. If this is the correct explanation of the pathology of polycystic disease the failure in the renal function may be explained either by progressive pressure atrophy and replacement fibrosis in the renal tissue between the cysts, or by the assumption of the characters of a true tumour on the part of the cyst epithelium with formation of a cystic adenoma.

But the age at which the disease generally proves fatal and the similarity of the changes in the functional renal tissue to those of chronic interstitial nephritis suggest another explanation, namely, the development of nephritis as a secondary phenomenon and one to which kidneys already seriously crippled by cystic disease may well be especially liable. The association in these cases of high arterial blood-pressure indicated by hypertrophy of the left ventricle, of extensive arterial degeneration and of a tendency to cerebral hæmorrhage, lends further support to this view.

Bristowe first drew attention to the association of cystic disease of the liver with that of the kidneys in a certain proportion of cases. The congenital origin of these cysts was also suggested by their occurrence in infancy in association with congenital cystic kidneys and by the presence in some cases of other malformations. The cysts in their structure resemble dilated bile-ducts, but they do not contain bile. In the cases under review the presence of multiple bile-ducts in the portal areas supports Still's view of the development of cystic disease in the liver as an excessive overgrowth of hypoblast in the duodenal diverticulum from which the biliary passages are derived, the excess formation of bile-ducts failing to establish a connexion with the liver parenchyma and forming retention cysts.

There is no histological evidence in these cases of any inflammatory changes in or around the bile-ducts.

#### Dr. GRAHAM FORBES.

Few who have been present at the post-mortem on an unsuspected case are likely to forget the remarkable appearance which the kidneys present in polycystic disease. The huge increase in size and transformation into a conglomeration of irregular cysts are most striking and impressive, leaving one amazed that such profound renal change can admit of the possibilities of life over a period of years. I recall two instances equally surprising and unforeseen, though clinically far from alike.

## 70 Forbes: *Discussion on Polycystic Disease of the Kidneys*

*Case I.*—A male, aged 49, who had undergone urethrotomy four years previously for relief of stricture; admitted to St. Bartholomew's Hospital under the care of Dr. Gee, with the history of recent hæmaturia and renal colic; tumour palpable in left renal region; urine contained pus, much reduced amount of urea; anuria and death a week later from uræmic coma. Diagnosis, pyonephrosis. Post mortem, both kidneys polycystic; left, weight 26 oz.; right, weight, 24 oz.

In the other case the result was even more striking.

*Case II.*—A Serb private, admitted in January, 1918, to the 36th General Hospital at Vertekop, in Macedonia, suffering from a severe attack of blackwater fever, which was prevalent at the time among both Serbs and British in the Salonika area. Death occurred two days later. To the surprise of all present at the post-mortem, both kidneys showed advanced polycystic disease and were enlarged to twice the size of the closed fist. Previous history of the case was not obtainable, but the man was well nourished, of muscular build, and a private on active service as well.

### SYNOPSIS OF COLLECTED CASES OF POLYCYSTIC DISEASE OF KIDNEYS.<sup>1</sup>

(1) *Congenital.*—Eight cases, five stillborn, three died in early infancy.

In three cases liver also cystic. May be combined with other developmental errors, e.g., anencephaly, polydactylism and meningocele.

(2) *Adult.*—Thirty-nine cases, eight associated with cystic liver. Ages 19 to 68. Records of thirty-six autopsies, 75 per cent. fatal over age of 40.

In twenty-eight cases *both* kidneys cystic, in eight cases *one* only affected, the left in five, the right in three. Wide range in weight from 17 to 22 oz. for each kidney up to 132 and 71 oz. in one case. Left kidney may outweigh right by 10, 38 or even 60 oz.

In six unilateral cases the unaffected kidney stated to be normal in four, granular in one, and in one case nephrectomy resulted in patient's recovery.

Adult cases fall into three groups:—

(1) Absence of renal symptoms, death due to intercurrent bronchitis, pneumonia, or phthisis. Polycystic kidney a post-mortem discovery in eight, or 22 per cent. of cases.

(2) Death sudden or after very short illness in seven, or 20 per cent.; in five due to cerebral hæmorrhage.

(3) Evidence of renal disease in twenty-one cases, or 58 per cent., for varying periods (a few days or months up to as long as twelve years), often symptoms only of chronic interstitial nephritis in the raised blood-pressure, cardiac hypertrophy and atheromatous arteries—rarely albuminuric retinitis. Lumbar pain may be continuous, or in evidence as attacks of renal colic. Renal tumour palpable on one or both sides in twelve out of thirty-six cases. Urinary changes variable, polyuria, later anuria; albumin usually traces only, pus and casts in few cases, occasionally hæmaturia. Urea may be reduced to a quarter or one-third of normal amount. Gradual failure in health with cachexia and wasting for some months, or sudden dyspnœa and œdema after interval of some years' quiescence with abrupt terminal anuria and uræmia. Differential diagnosis from hydronephrosis, pyonephrosis, renal tuberculosis, malignant growth, hydatid, simple renal cyst and pelvic tumour.

Prognosis very uncertain, more unfavourable the nearer the patient is to the age of 50. Life possible for years, or sudden termination from uræmia, as in twelve cases, or from cerebral hæmorrhage, in five cases. Outlook more hopeful in absence of cachexia, pulmonary and cardiac disease, and if urine shows no marked reduction in urea, with freedom from blood and pus.

### ÆTIOLOGY.

(1) *Retention Theory.*—Virchow favoured developmental origin—association with other abnormalities, e.g., polydactylism and anencephaly. Congenital form due to atresia of papillary canaliculi from interstitial fibrosis during intra-uterine life resulting in distension of cortical tubules. Adult form due to persistence of fetal mal-development

<sup>1</sup> *St. Bartholomew's Hospital Reports*, xxxiii, 1898.

and further influence of chronic inflammatory changes, i.e., an interstitial nephritis causing multiple retention cysts. Hence possible relationship between granular cystic kidney and true polycystic disease with inflammatory origin common to both.

*Comparison of Features common to Polycystic Kidney and Chronic Interstitial Nephritis with Simple Retention Cysts. (Derived from Post-mortem Records at St. Bartholomew's Hospital.)*

|                               |     |             |      | Polycystic kidney<br>(36 cases) |           |     | Granular kidney with<br>simple retention cysts<br>(61 cases) |
|-------------------------------|-----|-------------|------|---------------------------------|-----------|-----|--------------------------------------------------------------|
| Age, over 40, in              | ... | ...         | ...  | 75                              | per cent. | ... | 86 per cent.                                                 |
| Sex—male                      | ... | ...         | ...  | 65                              | " "       | ... | 76 " "                                                       |
| " female                      | ... | ...         | ...  | 35                              | " "       | ... | 24 " "                                                       |
| Hypertrophy of left ventricle | ... | Recorded in | 34   | " "                             | ...       | 56  | " "                                                          |
| Atheroma of aorta or arteries | ... | ...         | 28   | " "                             | ...       | 73  | " "                                                          |
| Cerebral hæmorrhage           | ... | ...         | 12.5 | " "                             | ...       | 13  | " "                                                          |

(II) *Neoplastic Theory* (Sabourin, Lejar and Malassez).—Polycystic disease—cystic adenoma.

(III) *Histological Mal-development* (Shattock).—Persistence of Wolffian body (mesonephros) and its cystic transformation, causing pressure on and distension of renal tubules.

(IV) *Primary developmental error in union of glandular tubules* (Still) with secondary interstitial inflammatory changes, affords most probable explanation of cystic disease applicable alike to kidneys and liver, and, as pointed out by Sir Bernard Spilsbury, in preference to theory of purely inflammatory origin formerly in favour, and other suggested theories of the causation of this puzzling disease.

#### Dr. F. PARKES WEBER

remarked that he had been very interested to hear that *genu valgum* might be connected with typical cystic kidneys in children, as it undoubtedly was with the renal disease in so-called "renal dwarfism." In regard to typical cases of congenital bilateral cystic kidneys he did not think that operative interference could be indicated. A man might be well-developed outwardly and able to do good work up to perhaps about 50 years of age; then he might develop uræmic symptoms and die, the post-mortem examination showing huge bilateral bunch-of-grapes-like kidneys. It was a marvel that such a man could live so long in apparent health. In such typical cases, without suppurative or other complications, he could not think that an operation (unless mere exploratory laparotomy in doubtful cases) could do anything but harm.



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WAR SECTION



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## War Section.

President—Air-Commodore DAVID MUNRO, C.I.E., R.A.F.

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### The Present-day Trend of Treatment of Gonorrhœa, with Special Reference to Cataphoretic application of Colloid Silver.

By A. T. FROST, Major R.A.M.C.

SINCE the discovery of the causative organism of gonorrhœa, drugs have been applied to the urethral mucous membrane with the intention of killing the organisms with which they come in contact. Later, when it was realized that no antiseptic could reach the organisms which are buried in crypts and follicles and beneath the surface epithelium, lavage with weak antiseptics became the routine procedure. But as this treatment meant that this was merely waiting until the patient cured himself, various methods of attack have been devised within the last fifteen years. A number of metal instruments have been added to our armamentarium; some of them are still of value in diagnosis, and others are of use in destroying the local foci of disease which delay cure.

At the beginning of the present year the staff of the Military Hospital, Rochester Row, came to the conclusion that two lines of investigation might be tried. One, to try to bring high antiseptics into contact with the gonococcus through the blood stream, and, to drive antiseptics into the deeper foci beneath the surface layers of urethral epithelium.

Lieut.-Col. T. E. Harty, D.S.O., R.A.M.C. (R.) had carried out a series of intravenous injections of acriflavine at the Military Hospital, Portobello, Dublin, in October, 1921. In the fifty-five cases treated he reduced the stay in hospital to 37·1 days. He noted that those cases which reacted to treatment did so early, while others continued to show gonococci for one month after beginning the treatment. The number of days under treatment before the disappearance of gonococci was thirteen. The method employed was intravenous injection of 0·2 gm. of acriflavine dissolved in 200 c.c. of physiological NaCl solution. This dose was increased to 0·3 gm. in 300 c.c. saline solution on the fifth day and repeated on the tenth day. During this course the urine was kept alkaline. In October, 1922, a similar series of cases were given this form of treatment at the Royal Herbert Hospital, Woolwich. The average time in hospital for eighty-eight men was 59·71 days. As the colour of the skin of the men became a lemon yellow as soon as the injection was within the veins, it was considered that this coloration of the skin might be avoided and the rapidity of elimination might be prevented if the drug could be injected into the muscles or under the skin. The first attempts were not successful owing to the bulk of the injection used, 0·3 gm. in 100 c.c., and the inflammatory reaction which

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followed. Moreover it was found during tests for toxicity on rabbits that the animals died from acid intoxication when given a lethal dose of acriflavine. Numerous experiments were then undertaken to solve the problem of the removal of the acid in the preparation, which equals 3·5 c.c. of normal hydrochloric acid for every gramme of the acriflavine, and to make the injection under the skin small in bulk and painless. This was done by adding 3·5 c.c. of normal sodium bicarbonate to one gramme of acriflavine and diluting with distilled water up to 20 c.c., to bring down the content of sodium chloride to 1 per cent. Two to seven c.c. of this solution may be injected under the skin, just over the muscle fascia, with no great pain resulting. Two sites have been used for the injection, one between the shoulder blades, the other behind the great trochanter of the femur. Three doses have been given as a course with a five-day interval. The investigations are still being continued. The figures accumulated up to date are of little value owing to their small numbers—39·1 days in hospital. In chronic gonorrhœa some most successful cases have been met with. To quote one case as an example: A soldier was transferred from another station with a history of one year with a urethral discharge containing gonococci. Three deep subcutaneous injections cured the disease, and he has been free from recurrence for the past six months.

One difficulty remains unsolved. This neutral solution does not keep well, it must be prepared freshly at least every three days, otherwise it becomes irritating to the tissues.

Up to the present acriflavine has been found to be the best of the dye products, as a bactericide. This field of research is only just open, and further results may be expected from the discovery of the new dyes.

Since last May (1923) a new method directed against the gonococcus has been instituted at the Military Hospital, Rochester Row, and has been continued at the Royal Herbert Hospital, Woolwich, since the closure of the former hospital in July. The origin of the work was the production, by electrical methods, of a pure silver colloid, which is stable in distilled water, by Major S. H. Long, D.Sc. This colloid was first used at the Indian Station Hospital, Karachi, for cases of gonorrhœa. A silver catheter with multiple perforations was introduced into the urethra and attached to the positive pole of the electric supply. The catheter was filled with the colloid and a small current of electricity was sent through the urethra—that is, the method was assumed to be ionization. The findings were so promising that the matter was referred to the Military Hospital, Rochester Row, for further investigation. When examined chemically the colloid was found to have no added protective; the particles were of small dimensions; it was not precipitated by 0·5 per cent. sodium chloride in three days, and 1 per cent. sodium chloride took three hours to destroy the colloid completely. Electrically, as the medium is a non-conductor, it was found that the colloid particles moved in the medium from the negative pole towards the positive pole. Bacteriological experiments, with the use of a culture of staphylococci isolated from the urethra, showed that 1,000 million organisms were destroyed in twenty minutes, and that 300 millions were killed in two and a half minutes by the undiluted colloid.

A week's trial was given of the ionic method, using this colloid, during the above clinical investigations, but this method had to be abandoned owing to the pain and discomfort of the catheter within the acutely inflamed urethra. It was then decided to try the effect of using the colloid cataphoretically, that is, applying electric pressure and not electric current through a column of the silver colloid within the urethra.

It is to be noted that this colloid consists of electro-negative particles which move distinctly towards the positive pole when subjected to an electric pressure, as shown by microscopical and electrical tests. Therefore, this method tends to drive the colloidal particles into the urethral mucous membrane when a column of the silver suspension is acted upon by a pressure of 60 volts direct current, or over, through the negative pole, with the positive pole attached to a perineal pad. Sixty volts direct current is the minimum pressure which will set the colloidal particles moving towards the opposite pole. After some experimental work on details, the following method of carrying out the treatment was adopted :—

A room is wired for the treatment of ten patients from a 106-volt direct current supply. Ten electrical points are distributed round the walls of the room, each with a plug and switch connected in parallel, through a switch board, with the source of electrical supply. Two wires from the wall plug bring the electrical energy to the patient. One wire is positive, the other is negative, and each wire is marked, after test, with its polarity. The positive connexion is attached to a wire gauze pad which is wrapped in several folds of washed lint, and on this the patient sits. From the pad an extension is brought forward to enclose the shaft of the penis. The pad is wetted with saline solution. The negative wire is attached to a 1 in. length of silver tubing, which is inserted into the lower part of the rubber tubing of an irrigation apparatus consisting of a 300 c.c. funnel, and 5 ft. of rubber tubing with a glass urethral nozzle being attached. A piece of silver wire is soldered to the inside of the silver tube and extends down to the glass nozzle.

The patient sits on a chair and on the positive pad, care being taken that he does not come in contact with the metal connexion. The funnel, suspended on a stand, or by a wire beside him, is filled with colloid silver, air is expelled, and the bladder and urethra are filled with the colloid by pressing the nozzle firmly into the urethral meatus.

With the sliding resistance on the switchboard at zero—usually marked “weak” on the board, the current is switched on. Next, the switch on the wall for the patient is put on. Then the sliding resistance is moved towards “strong,” until all resistance is cut out. Ten or more cases may be put on together, according to the number of wall points set up. There is practically no current used, even with ten patients the milliammeter does not register as much as 1 milli-ampere, but the pressure shown is 106 volts. In order to prevent the burning of the patient due to excess of current, which might be caused by an accidental short circuit, one wireless receiving valve has been introduced into the circuit, in series with the supply voltage, on the patient's side of the switch board. This valve acts as an automatic cut-out. Even on a dead short circuit the valve will prevent a current of more than 8 milliamperes from passing through the patient, a dose which is harmless, even at the high voltage used. So that the use of this high voltage for medical purposes is feasible and can be manipulated by anyone without electrical knowledge once the circuit is installed, by following a few simple directions.

The routine adopted is the following: The patient has a dose of twenty million gonococci injected subcutaneously, and his urethra is washed out with lime water to remove pus and mucus, and then with distilled water to remove electrolyte. The patient sits on the chair, adjusts the pad, fills his bladder and urethra, as mentioned, and the voltage is applied for three-quarters of an hour to one hour. If, during the treatment, the amount of fluid in his bladder is causing discomfort, or if the amount of urine is sufficient to act as

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a conductor, the bladder is emptied and again filled with colloid. This treatment is repeated daily for four days until the patient's urethra is free from discharge. He then goes through a test for cure, and is discharged from hospital when no signs of recurrence are brought out by the test.

If it happens that the urine is not clear at the end of the first set of four applications of silver colloid, the patient receives a second injection of anti-gonococcal vaccine, and a Kollmann's dilator is passed, and immediately after it he is again treated cataphoretically. It has been found that in those cases which require further treatment there are a number of closed follicles in the urethra, as can be seen by the urethroscope, and that when these are opened the cure can be completed by one or two further applications of the silver colloid.

The test of cure consists of a complete examination of the urethra after irrigation with magnesium chloride (1 in 500 solution) twice a day for two days, preceded by a 100 to 200 million injection of gonococcal vaccine. The examination consists of the introduction of a sound, followed by inspection of the urethral mucous membrane by means of the urethroscope, massage of the prostate and vesiculæ, with microscopic examination of any secretion expressed from them. The urine must be free from pus, and no pus must be present in the urethra.

As this treatment can only deal with urethral infection, it has been decided that if a preliminary examination of the prostate gland or the vesicles shows that the disease has reached them, or signs of any metastatic disease due to gonorrhœa are found, a deep subcutaneous injection of 2 or 3 c.c. of a 1 in 20 solution of freshly prepared neutral acriflavine is given at intervals of from three to five days. The site chosen for injection is over the deep fascia behind the great trochanter.

The number of cases which have been subjected to the combined treatment are too few for the quotation of figures sufficient for estimation of its value. The difficulty experienced is that of getting into the blood and urine a sufficient concentration of the acriflavine to be within the bacterial lethal dose. Tests of some serum and urine show that with a subcutaneous injection of 0.1 gm. the urine concentration is about 1 in 50,000, and in the serum the dilution is ten times greater, which suggests that definite results will be obtained with higher doses; and it was also found that the acriflavine concentration was negligible on the third day, as it had become so rapidly excreted.

During the test of the electrical colloid treatment daily examinations of microscopic slides have been carried out. Two conditions are looked for, one, the presence or absence of gonococci, and their number per field, and the other, the proportion of epithelial cells to pus cells present in the discharge. The first gives a rough estimate of the acuteness of the infection, while the second gives an indication of the stage of the disease. If microscopic slides are taken daily of a case of gonorrhœa from the beginning of treatment it is found that the findings by both can be plotted and the curves run practically parallel. Examination of microscopic slides can thus be used, even when examined only twice a week, as a quite reliable indication of the urethral condition and the progress of the case towards recovery. In chronic cases, where the finding of gonococci is difficult, the cell ratio gives the clue to the patient's progress, and directs the treatment. To obtain comparable results, it is, of course, necessary to take the smears in the same way, and for quickness and reliable counting of the cells the pus should be spread like a blood film for malaria examination or a differential cell count.

The results of the electrical treatment of 107 cases give the following figures :—

Total number of cases treated and discharged from hospital as cured, 107.

Average number of days under electrical treatment, 9.

Average number of days in hospital, 29.9.

Average number of days in hospital at two large venereal hospitals in the year 1922, 69.

It appears to the officers working this method of attack on gonorrhœa that it is more than an experiment, and that it opens up a field of work which promises success when the method is perfected. At present there is further work to be done as to the most efficient voltage required to get the quickest cure. Also, only one non-conducting colloid has been tried; other colloid metals may give better results, or this colloid may be improved. The object of this paper is to indicate the work done in the hope that others may try the methods and that we may arrive, by co-operation, at the scientific facts upon which to base medical control of the disease gonorrhœa, a point at which we have not yet arrived.

#### DISCUSSION.

Lieutenant-Colonel P. H. HENDERSON said that as he had been associated with Major Frost in dealing with the prophylactic aspect of the venereal disease problem, he must take the opportunity of laying stress on the excellent work Major Frost had done in investigating and improving the methods of treatment of gonorrhœa. Early and efficient treatment resulting in cure was undoubtedly one of the most important factors in prophylaxis. If this new treatment proved to be successful, not only in curing gonorrhœa but in reducing the time required for cure, it would be one of the greatest discoveries in the science of medicine that had taken place for many years. This he said because he judged a disease not by any particularly interesting features which it might present, but by the amount of suffering it inflicted on humanity and the loss of efficiency and work which it caused. Few diseases were more widespread than gonorrhœa, and while this method of treatment might achieve the primary object in the Services of reducing inefficiency and wastage through sickness it would have much wider effects, both economic and moral, through the general population, on the whole country. He presumed that Major Frost had had no opportunity of testing this method in the treatment of women? If it proved a success in the treatment of women as well as of men and brought about a rapid cure, then confidential notification and compulsory treatment might become a practical proposition. One of the effects of a rapid and perfect cure might be an increase in promiscuous sexual intercourse; it was, therefore, the more necessary that an efficient means of prevention should be devised. He (Lieutenant-Colonel Henderson) had been somewhat surprised to read recently in a journal of some repute that early disinfection by means of calomel ointment and potassium permanganate solution efficiently applied would prevent venereal diseases. Statistics showed that whereas the incidence of syphilis had been reduced that of gonorrhœa remained unchanged, notwithstanding the application of these remedies. His view was that it was impossible to apply these remedies in the form in which they were supplied to the Services, in this and in other countries as well, in an efficient manner, and this might account for the failure to reduce the number of gonorrhœal infections. He would very much like to see a fair trial given to oxycyanide of mercury (1 in 1,000) applied not as an ointment which varied in consistency and was difficult to apply and wash off, but as lubefacient, which was non-greasy, non-irritating, and of a consistency which lent itself to efficient application, and easy removal by wiping or washing. With an efficient preventative and a means of rapid and complete cure he saw no reason why this disease should not disappear from our midst.

Colonel J. CRAWFORD KENNEDY congratulated Major Frost on the promising results obtained by the treatment. He had been privileged in seeing the treat-

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ment carried out and was extremely interested in the method not only in its application to gonorrhœa but also as possibly applicable to other conditions. Whilst recognizing that the method was still in its experimental stage he suggested that it would have enhanced the interest of the paper still further if Major Frost had referred at greater length to the physical phenomena concerned in the process, and he (Colonel Kennedy) asked what size of colloid had been found the most effective. Could Major Frost state what was the incidence of sequelæ or complications—such as epididymitis—after his treatment?

Lieutenant-General Sir W. LEISHMAN said he confessed to some slight disappointment that the results of this treatment appeared, on analysis, to be somewhat less dramatic than he had anticipated from accounts he had received; but Major Frost had, quite rightly, guarded himself against claiming for it more than he felt at present to be justifiable. The theoretical basis of the treatment was of very wide interest and he (Sir William Leishman) ventured to suggest to Major Frost and his colleagues that it appeared to be one in which laboratory experiments, *in vitro*, might prove fruitful. Suitable combinations of leucocytes, gonococci, plasma and colloid solution might be put up, under varying conditions, in tubes or cells into which electrodes were introduced, and the results studied either microscopically or by special methods devised for the purpose. He looked forward with great interest to the results of an extended application of the method.

Squadron-Leader F. N. SMARTT said that he had given an extended trial to ionization in the treatment of gonorrhœa, and in order to judge the effects of this treatment no other form of treatment was given. The method was of use in certain selected early and chronic cases, but his considered opinion was that, in the acute established disease, no materially better results were obtained from ionization than from other methods of treatment. The treatment was not painful and was not accompanied by an increase of complications such as epididymitis. Soluble metallic salts of Cu, Ag, and Zn, were used; solutions of these salts were good conductors, and ionic penetration of these metals was an established fact. The milliamperage employed was the determining factor in ionization; in acute gonorrhœa doses varying from 5 to 15 ma. could be tolerated. He had not himself used colloidal preparations: these preparations were non-conducting and he asked Major Frost if he would explain how electrical action was obtained by the use of a non-conducting colloid. He understood Major Frost used a pressure of 120 volts, and that this gave a milliamperage of only  $\frac{1}{2}$  ma., and that he used acriflavine in conjunction with his electrical treatment. Was not this a possible determining factor in the treatment, and one which influenced the final results obtained?

Major FROST (in reply) said that there was no reason why this form of treatment should not be used in the treatment of gonorrhœa in women. The size of the colloidal silver particle was about 20 micro-microns and passed through a Chamberland F filter. The smaller the particle the more effective was the colloid. The percentage of epididymitis was not increased by the treatment. The number so far was 2 per cent., except when the patients in some cases took violent exercise. The only microscopical tests tried up to the present were on movement of the colloid particle under electrical pressure, but experiments on the lines suggested by Sir W. Leishman would be undertaken. Squadron-Leader Smartt had used ionization in the treatment of gonorrhœa. There was no relation between the two methods, as cataphoresis was the movement of an unchanged particle towards the tissues under the pressure of a high voltage. The colloid was non-conducting in the sense that only minute milliamperage was allowed to be conducted through it. The results obtained were not due to acriflavine, as this drug was only used in the case of patients suffering from complications.

## War Section.

President—Air-Commodore DAVID MUNRO, C.I.E., R.A.F.

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### The Use of the Aeroplane in the Medical Services in War:

#### PRESIDENT'S ADDRESS.

By Air-Commodore DAVID MUNRO, C.I.E., R.A.F.

I HAVE chosen as the subject of my Presidential address one in which experience so far has been confined entirely to the Air Services, and which is, therefore, an appropriate one for me to discuss. The use of the aeroplane for medical transport has, undoubtedly, a future, and as it has already been used to an extent probably little known, I thought that it would be of interest to you to hear about it.

#### (I) A SHORT RESUMÉ OF THE USES OF THE AEROPLANE FOR AMBULANCE TRANSPORT UP TO THE PRESENT.

The history of aerial ambulance transport, like that of the Air Services generally, is all of recent date, and the first efforts, as one would expect, were improvisations. Indeed, even now and in the future, circumstances arise, and will continue to arise, necessitating the use of improvised methods of transport for sick and wounded by air.

I shall, therefore, first tell you something about improvised methods before proceeding to discuss organized methods of transport by specially designed ambulance machines. By improvised methods, I mean the temporary use of a Service aeroplane to carry sick or wounded, either (i) with no special arrangement at all, or with primitive arrangements, as in all the earlier cases, (ii) by the use of a special kind of stretcher, or (iii) by the adaptation of the machine to carry an ordinary stretcher.

##### (1) *Carrying Sick and Wounded in Aeroplanes.*

There are, of course, many instances of this (some of you will remember the scheme which Colonel Donovan, R.A.M.C., put up to the War Office several years before the war), of which I may just mention a few:—

(a) During the retreat of the Serbian army in 1915 Captain Dangelzer, of the French Squadron, carried a seriously wounded Serbian airman from Mitrovitzza to Prizend, and a few days later the same wounded patient was carried on to Vallona by the famous airman, Lieutenant Paulhan.

(b) In December, 1920, Major Denaim, commanding the French Air Service in the Levant, organized the transport of wounded by air by removing the machine guns from some of his military machines and passing the wounded



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through the opening in the fuselage. By means of this make-shift arrangement more than eighty men were carried between December, 1920, and April, 1921.

[Photographs of French machine with portion of fuselage cut away and man strapped in were here shown.]

I do not know why I have specially mentioned the French, possibly I did so because they were very early experimenters in this field, and have since paid great attention to the development of their aerial ambulance transport system. I will now describe some of our own work in this connexion :—

There is the instance of the recent evacuation by air of 198 British cases in Kurdistan. During May of this year there was an epidemic of acute dysentery amongst two battalions of British troops operating in the Kurdist mountains—over 200 men having gone sick at the same time. The troops were at the time 101 miles by air—considerably more by mountain and desert tracks, which were the only roads—from Baghdad, and they had no transport other than by pack. The operation was one of considerable difficulty, partly owing to the weather and partly owing to the difficulty of selecting suitable or even possible landing grounds in the mountains. The pilots had to exercise great skill and care and the medical officers had to work hard to get their patients to the improvised landing ground. The operation, which was completely successful, took 128 hours 45 minutes. The machines used were the Vickers-Vernon troop carriers. Ninety-five flights were made and 9,615 miles were flown. The patients were all flown down to hospital in Baghdad. None of the sick minded the method of transport, and most of them enjoyed it. Serious cases were sent from Kirkuk to Baghdad in the early morning or late afternoon to avoid “air sickness” during the “bumpy” portion of the day. The operation was an emergency one, and had it been an organized scheme would have undoubtedly taken less time than it did. As it was, had air transport not been used such an evacuation would have taken several days ; it would have caused a great deal of suffering over the available land routes, and there would probably have been fatal cases, of which, happily, there were none. Unfortunately, the only actual ambulance machine that was used had a forced landing early in the operation, and was out of action. I was thinking of this when I said earlier in the address that circumstances would always arise in which improvised transport would be necessary.

It is my view that specially designed ambulance machines are not, and will not in the future, be necessary for “sitting cases,” nor even for some “lying” cases, and that the provision as a routine measure of fittings to carry stretchers on the ordinary troop-carrying machines will enable them to be used for ambulance work when required, without any alteration, and at small cost.

I will mention another case of the use of the ordinary Service aeroplane, and this leads me to my next heading, viz. : “The Use of a Special Kind of Stretcher.” This is the case of General Ironsides. Towards the end of 1921, General Ironsides, who was then G.O.C. in Mesopotamia, was returning by air from an inspection on the Euphrates line, when he was “crashed” some 150 miles from Baghdad (I think near Samawa—I have been unable to find my account of the accident). He broke his femur. Two R.A.F. officers with the necessary material were sent out by air—one from Basra and one from Baghdad—and within three hours of the accident General Ironsides, with his leg in a Thomas splint, was drinking a whisky and soda and waiting for the special train coming up from Basra to take him on to Baghdad (the crash was fortunately near a station on the Basra-Baghdad railway line). I

understand that the original fixing of this splint did not necessitate its being removed, and I know that he made a perfect recovery. Had we had, as we have now in overseas command, a supply of the stretcher to which I am about to refer, he might have been taken to Baghdad by air without having to wait for the train.

(2) *The use of a Special Kind of Stretcher.*

This may have a special interest for Naval men, as the stretcher used is of Naval pattern—what is known as the Neil-Robertson stretcher; adapted, as shown, mainly by the addition of extra linings, and as the result of experience of General Ironsides' case together with the needs of the Cairo-Baghdad mail route.

The structure and method of use of this stretcher is best shown by the accompanying photographs, illustrating the details and use of the stretcher. (Photographs shown here.) The weight of the stretcher is 39½ lb. As you will see it is a folding bamboo stretcher, which makes for the patient a sort of mummy case, which is strapped on to the top of the fuselage of the machine—Bristol Fighter, or 9A, or Vickers-Vimy—on the latter *two* patients can be carried comfortably.

[The President here read an account of two cases in which this stretcher had been used, (i) to convey a surgical emergency case from Sollum to Cairo, and (ii) to convey a diphtheria case from Amman to Jerusalem.]

Squadrons in Egypt and Iraq are now supplied with these modified Neil-Robertson stretchers, called by us "Neilson" stretchers.

(3) *The adaptation of Service Aeroplanes to carry an ordinary Stretcher.*

One of the first and most interesting experiments in this line was carried out in the R.A.F. Somaliland Expedition (known as the "Z" Force) in 1919-20.

[Photographs of the machine used (a D.H. 9A) were shown here, and an extract of Health Report for the Air Force, 1920, describing it, was read by the President.]

The French have used this method fairly extensively. In March, 1921, the authorities of the French Air Service and Army Medical Corps inaugurated a regular service in the Levant and in Morocco, sixty Breguet XIV T machines being used. These were simply adapted bombing machines, the gun turret having been removed and a side door made in the fuselage to admit two stretchers, for which the necessary support fittings were provided to keep them steady and horizontal. The results were eminently satisfactory, over 400 wounded and sick being carried without accident of any kind. Reports on this service were read before the French Colonial Congress for Public Health in September, 1922, by Drs. Bassères, Épaulard and Gravelat, from which the following further figures are taken: 273 casualties conveyed to hospitals (168 in Morocco and 105 in Syria) without serious accident. In the sub-division of Meknes alone (principal centre of operations of Issoual) 125 wounded were removed between May 29 and August 21, 1923. This represented eighty-three flights and a total distance of 8,600 kilometres covered by aeroplanes, carrying wounded without any incident except that machines were twice forced to land at Rabat instead of at Casablanca. The transport of another eighty cases to different centres, representing flights on fourteen different routes, was also mentioned in the reports; also the fact that in one operation the removal of the wounded was carried out by flights of six machines operating together, making it possible to remove all the cases in one hour from Issoual to Meknes—80

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kilometres. In Syria, forty seriously wounded men were removed from Deir-es-Zor to Aleppo in three hours, the distance being 400 kilometres—a twenty days' journey ordinarily on Syrian roads.

In the report the increasing importance of aeroplanes for ambulance service was emphasized and it was pointed out that the condition of the wounded was not aggravated (fur-lined sleeping bags were provided for the passage across the Atlas mountains). The wounded frequently *asked* to be carried by air in order to avoid the suffering of being carried across the desert in mule carts, and in order to be quicker in touch with skilled surgical aid.

At the Congress Dr. Gravelat defined the general principles which should govern the use of aircraft for ambulance purposes, as follows:—

(1) Ambulance aeroplanes should not be used indiscriminately. Their use should be limited to cases where, in the interests of the patients, the advantages exceed the risks.

(2) They should convey patients to the hospitals, this being their normal duty if they are to attain the highest degree of efficiency.

(3) They are of value for the transmission of medical personnel or urgently needed material to stations for those wounded not in a condition to be removed.

(4) Aircraft should be of a standard type used in any particular zone, as this does away with the necessity for special pilots and the provision of special spare parts.

This policy corresponds closely to that which we at present follow, in the use of our British Service Aerial Ambulances.

The mention of machines specially designed for ambulance work brings me to the next division of my subject.

### (II) A BRIEF DESCRIPTION OF THE TYPES OF AIRCRAFT USED FOR AMBULANCE WORK.

As far as I can discover the French again were the first to consider this question (theoretically they had planned an ambulance aeroplane as early as 1912), but I do not think that they were much ahead of us in the practical use of specially designed machines.

All the figures which I have quoted earlier in this paper of French evacuations were those of cases carried on improvised machines, but the French, like ourselves, had grasped the fact that for certain cases something more comfortable and roomy and with better facilities for loading and unloading stretchers, was required. The advent of civil aviation, of course, facilitated the development of such machines, as machines for civil transport have to be designed for the comfort of travellers.

In 1921 the French Ministry of War, in collaboration with the Medical Corps and Air Service officials, put into use a type known as the Breguet-Limousine XIV (T bis) which carries two stretcher cases, a doctor and a nurse. The cabin is roomy and comfortable, and is artificially heated. It is possible to perform urgent operations *en route*, for which instruments are provided such as oxygen generators, outfit for injections, electric hot bottles, &c. We have not proposed the use of operation tables, X-ray outfits, &c., in our designs, but our 1921 design (a Vickers-Vimy machine) gives more seating accommodation than the French Breguet-Limousine and our later Vickers-Vernons are even better in this respect.

[Photographs illustrating the Vickers-Vernon ambulance machine were here shown.]

I will give you a brief description from a report which I wrote myself:—

I had a short flight yesterday in one of the two new Vickers-Vernon ambulance machines (twin engine Napier Lion, 450 h.p.).

Both of these machines are now complete and tested. They appear to me to be very satisfactory.

*Seating.*—Seats, eight, plus an orderly, and carries two stretchers. The seats are most comfortable, deck chair hammock pattern echeloned in behind each other so as to economize space. The seats fold in against the inner wall of the fuselage so as to allow room for two orderlies to walk about and handle stretchers. The stretchers are run in on a rail through the nose of the machine and are carried on special folding brackets on the starboard side above the seats. They have, of course, to be loaded in before the seats are folded out into position.

*Other Fittings.*—After the stretchers are loaded, an ice-box (with compartment for perishable food) is run in on the rails to which it is locked by pins and occupies the space in the nose through which the stretchers were loaded.

In the rear space towards the tail is a w.c. with a high pressure small flush, emptying into a special receptacle which can be detached and cleaned at the end of a journey. This receptacle is fitted with anti-splash and also ventilating arrangements.

A cupboard for tinned foods, &c.; a dispensing cupboard with drugs and dressings, and an electric kettle and sterilizer are also fitted. A wireless mast is carried.

*Ventilation.*—The windows are of fine wire mesh, with a sliding panel which allows of opening. The forward window on the port side is a clear window and can be opened outwards at an angle so as to act like a wind scoop—on the ground it gets the slip-stream of the port propeller. This window can be closed easily in the strongest draught in the air.

The forward starboard window is similarly fitted but has also a frame to take a "Khus-Khus" tattie, or similar contrivance, e.g., a jute square which can be moistened and cool the air by evaporation. Yesterday was a cold day, but I was quite warm in ordinary clothes—it was neither draughty nor stuffy.

*Lighting.*—By windows as above—also electric ceiling lights. Navigation lights are carried.

*Sun-proofing.*—All the windows have blinds which can be drawn.

*Other points.*—Smoothness in running—I could have written a letter at any time. She seemed to me to be very well balanced on controls and in climb and speed was faster than I had expected from a machine of the size; also landed at slow speed beautifully. I was particularly struck with the engine-cooling. The radiator was hardly more than warm to the touch after she had been running the best part of an hour, and about five minutes after we landed I put my hand on the exhaust and found it cold.

The engine note is not disagreeable and altogether I am convinced that sick can be carried *in real comfort* in this type of machine. Its seating capacity and radius of action should make it of real value; and provided that such machines can be kept in running order I believe that the problem of evacuation of sick by air is going to be much simplified.

We have an establishment of these machines which is being increased, and we have also building a Bristol machine with Jupiter engine to carry two stretchers, suitable for going out to a "crash" case and bringing in an injured pilot and passenger. Experiments with machines are, at present, being conducted in the most suitable areas, i.e., Iraq and Palestine.

The question of starting an aerial ambulance convoy with an establishment of machines and personnel on the lines of the motor ambulance convoys of the Army is one in which the results of further experience have to be awaited.

(III) POSSIBLE DEVELOPMENT AND CHANGES IN THE ORGANIZATION AND DISTRIBUTION IN THE FIELD OF MEDICAL UNITS ARISING OUT OF THE APPLICATION OF AERIAL EVACUATION.

Hitherto I have been dealing with facts and with recorded progress, but I now enter the realms of romance of which I have not very much experience, and I shall, therefore, be brief. It is certain that there will be an extended use of aeroplanes in the Medical Services in future warfare. One cannot envisage them as being useful in the front line in trench warfare for obvious reasons, but in a war of movement they may be used to keep in touch with pursuing or retreating forces and to remove serious casualties far from the field. For work on the lines of communication between field ambulances and lines of communication hospitals there is a possible scope. The formation of aerial ambulance convoys for this purpose might result in the staff being able to place lines of communication medical units farther back, or even to have direct communication by air between the field ambulances and the base hospitals. Nor would it necessarily be entirely ambulance machines that would be employed. Aeroplanes used for carrying up troops or stores might be used on the return journey to evacuate sick—the sitting-up cases or even lying-down cases—by fitting the machines with rapidly adjustable folding stretcher supports on the seats, i.e., light supports which, when folded away, take up little or no space. We are now experimenting with these.

In any case, I imagine that the aeroplane ambulance will be used to bring back rapidly from the field ambulances the men suffering from severe head and abdominal injuries.

I have said nothing about the Naval side, but I envisage the use of seaplanes or deck-landing planes to evacuate sick from men-of-war to land.

**An Inquiry into the Causes underlying the Mosquito Nuisance in the Alverstoke (Hants) District and the Initiation of Measures of Control. (Extract from a Report by Surgeon-Commander D. H. C. Given, M.D., D.P.H., R.N.)**

By Surgeon-Commander G. O. M. DICKENSON, R.N.

ON reading Surgeon-Commander Given's report it struck me the subject might be of special interest to all the Services and especially to the Army and Air Force as they are more frequently stationed in places where mosquitoes are a nuisance. As the paper is not my own, and as I cannot claim any expert knowledge of the mosquito in question, I regret that, when the discussion is over, I may not be able to throw any further light on the subject. The author of the report, who is now abroad, was Naval Health Officer at Portsmouth and, as he lived on the Gosport side of the harbour—in Alverstoke—he had daily reminders that mosquitoes were, to say the least, a nuisance in the district.

The mosquito nuisance has been known in the Alverstoke district for a great many years and references are made to it in old diaries written fifty years ago. Surgeon-Rear-Admiral Sir Percy Bassett-Smith, when Fleet Surgeon at the Royal Naval Hospital, Haslar, established the fact that the mosquito mostly found in the district was not the *Anopheles*, the malaria carrier; consequently the matter aroused little interest on the part of the health authorities and therefore nothing was done to deal with it. There is also much evidence to

[December 10, 1923.]

show that the mosquito has become a really serious pest only in recent years, and, judging from investigations that have been carried out, one has been led to the conclusion that conditions have been brought about which, if allowed to remain uncontrolled, would soon have resulted in a mosquito nuisance of such proportions as to affect seriously the economic development of the district.

The first country to recognize the economic importance of mosquito eradication—apart from the question of carrying disease—was America, and in that country they have already had ten or more years' experience, particularly in New Jersey, in dealing with the same kind of nuisance as occurs in Alverstoke. Measures of mosquito control undertaken by the health authorities of Alsace, in France, have been recently recorded, but before 1920 no systematic efforts at controlling the mosquito from the point of view of its being a nuisance appear to have been made in this country.

In 1920 the Hayling Island Mosquito Control Committee was formed under the able directorship of Mr. J. F. Marshall, Fellow of the Entomological Society, and the operations in the Alverstoke district were based largely on the investigations carried out at Hayling Island.

In order that you may follow the conditions existing in the district I must briefly describe its geography. The whole district is low-lying, many parts being actually below high-water level so that during spring tides sea-water percolates upwards through the shingle causing the formation of pools and ponds which remain for about a week after the spring tides have subsided. The moats are fed from the sea at high tide by means of sluices, and the River Alver also enters the moat on Browndown Marsh. The district is also indented by creeks which run inland from Portsmouth Harbour and which are therefore tidal.

[Further description of the district was explained by lantern slides.]

*Reasons why the Mosquito Nuisance has Increased in Recent Years.*

(1) *The Moats.*—I have shown that the River Alver becomes continuous with the moat on Browndown Marsh. The river, when in flood, brings down large quantities of mud which, if not regularly removed, tend to dam the moat at this point. Up to 1915 parties of the Royal Engineers were regularly employed in keeping this part of the moat clear; since then—until only a few months ago—clearing operations have been in abeyance. This dam had the effect of raising the water level in the River Alver for miles inland, and of increasing the area constantly under water.

Instead of the moats being emptied and cleaned—a very costly operation—they have been flooded periodically from the sea at high tide. In this process not only are the moats flushed but the salt water extends some way up the Alver valley, thus producing conditions favourable to mosquito development. Through this process of silting up, the River Alver has gradually become ineffective as a means of draining the land through which it passes. Interesting evidence, corroborating the statements of certain lessees of property along the Alver valley that their land has become water-logged during recent years, was forthcoming in the discovery of the old sluice in the moat bank. This sluice was about 2 ft. below the normal level of the water in the moat.

(2) *The Railway Embankment.*—This embankment was constructed some twenty-five years ago, and has undoubtedly contributed seriously to the present mosquito nuisance. (a) The original course of the stream was deflected to the eastward in order to avoid the expense of bridging it in the middle of the

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marsh. (b) A cross channel was cut to the westward to connect it with a tributary on that side. (c) The original channel was not filled in and has now become a sort of lagoon on either side of the embankment, and is a prolific breeding ground for mosquitoes. (d) The deflection of the stream has involved the introduction of a number of right-angled bends, which increase the tendency to silt up and retard the flow. (e) The stream is brought back to its original course from the opposite direction to its western tributary, with the result that the main stream tends to dam the weaker one from the west. (f) The concrete beds under the two bridges in the embankment are laid several inches too high, so that they act as dams to the water above them.

In the annual report of the Medical Officer of Health for Gosport for 1920, the following references to the local mosquito nuisance are made:—

Complaints have been made for many years regarding the pest of mosquitoes during the summer months at Alverstoke and Stokes Bay, and in the spring of 1919 an attempt was made to limit their activities by spraying stagnant pools with paraffin. I wish to lay special emphasis on the fact that no mosquito larvæ were found in the moats. These moats are almost invariably blamed by the inhabitants as being the breeding places of the mosquito, but it has been conclusively proved on many occasions that the brackish water contained therein is antagonistic to the growth of larvæ, and that none are ever found there.

I wish you to take particular note of this statement, as it was the generally accepted theory that mosquitoes could not breed in brackish water.

Surgeon-Commander Given commenced his investigations in March, 1923, and found numbers of *Culex pipiens* and *Theobaldia annulata*, but was not satisfied that these could be the cause of a nuisance which generally commenced in May or even April. He then got in touch with Mr. Marshall, of Hayling Island, and from him learnt that the culprit was the salt marsh mosquito, *Ochlerotatus detritus*. This mosquito is widely distributed, not only around the coasts of the British Isles, but around the coasts of Europe from Denmark to Macedonia, along the North African littoral to Suez, and from there northwards to Palestine. It is evidently a closely allied species that infests the coastal regions of North America.

### LIFE HISTORY AND SPECIAL CHARACTERISTICS OF *OCHLEROTATUS DETRITUS*.

(a) It passes the winter in the egg or larval stage.

(b) It lays—perhaps sows would be a better word—its eggs extensively over the ground in batches from April to October. The eggs can withstand drying for a considerable time, perhaps twelve months or more.

(c) The adult mosquito has a flight of from two to four miles.

(d) It breeds, by choice, in brackish or salt water, and, as these pools are generally tidal in origin and caused by percolation through the shingle, they seldom dry up during the summer. This explains why, while other mosquitoes disappear owing to lack of water in the dry months, the salt marsh mosquito continues to flourish during the driest and hottest summer.

*Larva*.—The larva is distinguished from most other species by its syphon, which is of medium length, stout, carries a pair of hair tufts about its middle, and on the proximal side of the latter is a pecten or comb, consisting of about twenty-four small pointed teeth of equal size and equally spaced in line with the hair tuft.

*The Adult*.—Both sexes are readily distinguished from other mosquitoes by the mottled, pepper and salt appearance of the dark cross bands on the abdomen, caused by light coloured scales on a dark background. The light coloured bands on the abdomen are of a dull yellow colour and of equal width. The legs and tarsi are uniformly dark, without light coloured bands, and both legs and wings have

light and dark coloured scales. The male genitalia are characteristic of the species, while the females have a sharp-pointed abdomen as contrasted with the rounded end of the Culicidæ and others.

Now we see the real cause of the mosquito nuisance, and it was proved that the generally accepted ideas were wrong and that some mosquitoes *could* breed in brackish water. The fact still remained, however, that larvæ were never found in the moats, and this phenomenon had to be explained. Surgeon-Commander Given was able to demonstrate a very definite practical explanation. He collected large masses of larvæ in nets and transferred them to the moats. They disappeared within a very few hours, being eagerly devoured by the shrimps and small fish (chiefly sticklebacks) which lived in the moats. Another very good example of the efficiency of shrimps in destroying the larvæ was found in a small pond on the golf course. In spite of numerous searches no larvæ were ever found here, and later it was discovered that the pond contained shrimps.

#### OPERATIONS AGAINST THE MOSQUITO.

These were commenced in March, 1923, by small parties of volunteers, chiefly Naval ratings from the the Vernon Experimental Station and a few Naval officers resident in Alverstoke. The interest of the general public was gradually aroused and demonstrations as to the nature of the nuisance and the way to attack it were given to school children, boy scouts and others. By May 1, a party of forty men, twenty from the Army and twenty from the Navy, became available for work at Browndown.

The following is a brief description of the operations :—

(1) Arrangements were made with the military authorities to reduce the water level in the moats by about two feet, thus automatically draining a considerable area.

(2) All the outlying breeding places were then thoroughly controlled by the use of oil or larvicide (White Cross fluid or izal). These were applied by means of four-gallon watering cans. On the golf links burning was found to be the most efficient method of destroying larvæ. A combination of light shale oil and heavy fuel oil was applied and large quantities of oil-soaked waste were employed to get the fire well alight, and from these centres it was spread by sprays of the light oil, large garden syringes being used. When the heavy oil began to burn, the heat produced was sufficient not only to sterilize the ground, but also in many instances to dry up completely the shallower pools and marshes.

(3) Rough drainage was carried out wherever possible, and old watercourses were cleared. Large rakes, specially made in the Dockyard, were found to be the most useful implements to effect this clearance. They had three or four flattened iron teeth bent at a right angle and were fitted to a short wooden handle. These rakes entirely replaced the pick and shovel when dealing with soft peaty soil.

On Browndown Range it was impossible to carry out drainage, as the infested areas were entirely surrounded by higher ground. This difficulty was got over by digging trenches and draining the water into sumps, whence it was pumped into the moat by a hand fire-engine. A similar proceeding was carried out at Stokes Bay, and here Surgeon-Commander Given managed to obtain the assistance of the Gosport fire-engine to pump the water out of the sumps.

Wherever possible, potholes and the beds of pools were filled in with shingle, but the working parties could not be retained long enough to do this as



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thoroughly as could be desired. Owing to the sea-water percolating upwards through the shingle, and to the low-lying ground making efficient drainage impossible, resource had to be made in many places to the frequent application of larvicide.

(4) The final effort made was to stock No. 1 area with fish (sticklebacks) and No. 2 area with shrimps, both collected from the moats. Most of the fish died within twenty-four hours.

The Naval working party returned to barracks on July 9.

### RESULTS.

The following are some of the reports received from various Naval establishments at the end of the operations :

(a) *From the Commanding Officer, H.M.S. Vernon.*

"At the Experimental Station, Stokes Bay, a considerable reduction in mosquitoes has been observed."

(b) *From the Surgeon-Rear-Admiral, R.N. Hospital, Haslar.*

"There has been a marked diminution in the number of mosquitoes in Haslar Hospital and environs during the past two months. The improvement as compared with previous years has been very pronounced."

(c) *From the Commandant, R.M. Barracks, Forton.*

"The work has been productive of very satisfactory results at Browndown. Mosquitoes have practically ceased to trouble the camp and only one case of bad mosquito bite has been reported up to the present this year."

(d) *From the Admiral Superintendent, H.M. Dockyard, Portsmouth.*

"The general impression is that the mosquitoes have not been so numerous this year, but whether this is due to the various remedial efforts made or to the cooler weather experienced to the end of June, cannot be stated."

[With reference to this opinion, it will have been gathered from Surgeon-Commander Given's report that the weather conditions were not unfavourable to mosquito development.]

*Yachtsmen* living on board their boats in Haslar Creek stated that up till this year they have never been able to sit on deck in the evenings, but that lately they have not seen a mosquito.

*Residents in the district* confirm the report that there has been a wonderful change in the district and that many private gardens are now practically free from mosquitoes.

The results have surpassed expectations and a demonstration has been given which shows that the mosquito can be eradicated.

### FUTURE CONTROL OF THE NUISANCE.

Apart from the ordinary methods of drainage and filling in of pools, the knowledge of the life history of the salt marsh mosquito has put us in possession of some useful facts which should be utilized in the campaign of eradication.

(1) *The destruction of all cover* which harbours the adult mosquito and provides conditions favourable for reproduction and egg-laying. This can be carried out in spring and summer by the oil spray method. Perhaps the "flammenwerfer" used by the Germans during the war might prove still more useful.

(2) *The destruction of the eggs* before they hatch out into larvæ. This should be carried out at the end of the summer, before the occurrence of the

autumn and winter floods. The best method of effecting it is to surround the breeding grounds with a barrage of burnt ground sterilized by fire. When this is not practicable the barrage could be completed by the use of strong disinfectants.

(3) *The destruction of larvæ.* The most vulnerable stage in the mosquito's life cycle is that which immediately follows its being hatched out of the egg. Oil and larvicide are most efficacious agents at this stage. The appearance of pupæ should be regarded as evidence of neglect.

(4) *The use of shrimps and fish.* In the course of this report evidence has been produced that shrimps and fish *do* destroy larvæ. Further experiments with these might lead to enormous improvements in dealing with mosquitoes. This method has also the great advantages of costing practically nothing and of working automatically.

(5) In September, 1922, the Journal of the French Academy of Science published an article which indicates that young eels are voracious larvæ eaters. They arrive in shoals in our river estuaries from March onwards and can be obtained by the hundredweight for a very small sum from the Severn Conservators. They possess greater vitality than other fish and can live in almost any stagnant ditch, or even in mud, for long periods. It is hoped that someone will give these elvers a trial in England and fully report the results.

#### DISCUSSION.

Lieutenant-Colonel MACARTHUR, after commenting on the zoological standing of *Ochlerotatus*, said that Surgeon-Commander Given's report emphasized the importance of including in any antimosquito measures a close study of the habits of the species to be attacked—a truism not always honoured in the observance. He pointed out some of the fallacies in assuming that the habits of one observed species were necessarily those of another, for even the same species might alter its habits with a changed environment. Some books stated that *Aedes (Stegomyia) argenteus*, the yellow fever mosquito, took its first feed of blood only by day, and that subsequently it bit only at night. This would mean that infection with yellow fever could be escaped by avoiding an infected district at night. Unfortunately, this statement was not generally true, for he had experienced no difficulty in inducing various individual *Aedes argenteus* to feed from his arm repeatedly by day.

Wing-Commander H. E. WHITTINGHAM remarked that Surgeon-Commander Dickenson's paper was important because it brought to notice the necessity of waging war against mosquitoes and other insects in this country. Broadly speaking, insect life in general depended on organic matter and moisture for its perpetuation. Wherever insects could breed the sanitation was defective. In and around camps such a condition should not be tolerated. The men should not be allowed to look upon these insects as a necessary evil, but be made to adopt prophylactic measures. In fact, war on the insect life in the camps of this country was an ideal way of training men for similar and more arduous duties in warmer climates.

He did not think sufficient attention was paid to the fact that the larvæ of mosquitoes and midges were most vulnerable in their early life. A considerable proportion of these insects passed into the early larval stage in the autumn, and hibernated in the larval stage. This gave man a chance of carrying out extensive insecticidal measures during the cooler weather when, fortunately, the insects were relatively localized.

In Malta many mosquitoes were found breeding in the salt-water pools and pans near the shore. The wrong opinion was prevalent that mosquitoes could not breed in salt water, and, to save labour, fire-buckets were filled with salt water. It was necessary for the sanitary officer to bear this in mind and to impress the fact on all ranks. Other curious breeding places of mosquitoes must be recognized; for instance, Wing-Commander Richardson had found *Anopheles maculipennis* in sulphur wells in Egypt.

\*

## 18 Dickenson: *Mosquito Nuisance in Alverstoke District (Hants)*

The reference to the anti-mosquito habits of eels recalled to mind an interesting condition of some ponds in the grounds of the R.A.F. Central Hospital at Finchley. These ponds had been moderately clean and there were few mosquitoes breeding in them. About two years ago the ponds were emptied and several eels which inhabited them were destroyed. Thereafter, these ponds gradually became filthy and bred innumerable mosquitoes. Once again they were cleaned, but only to relapse into a dirty condition. In an endeavour to keep them clean on the surface ducks were introduced with good results. It was possible that originally the eels kept down the mosquitoes and cleaned the surface water generally.

Colonel J. C. KENNEDY said that he was able to give the name of the salt-water mosquito in Malta referred to by Wing-Commander Whittingham. It was the *Acartomyia Zammitii*, and it was a severe pest all round the coast of Malta, where the larvæ developed in the pools of salt water on the rocks. The fact that the pools consisted of concentrated brine as the result of evaporation did not affect the survival of the larvæ.

## War Section.

President—Air-Commodore DAVID MUNRO, C.I.E., R.A.F.

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### Bismuth in the Treatment of Syphilis.

By Surgeon-Commander R. J. G. PARNELL, R.N.

(Urological Department, Royal Naval Hospital, Haslar.)

IN 1916 Sauton and Robert successfully treated the spirillosis of fowls with bismuth. Sazerac and Levaditi confirmed this work four years later, and if we exclude a few isolated experiments by other workers, we can date the serious study of the treatment of syphilis by bismuth from the year 1920. Since Sazerac and Levaditi published their researches carried out from 1920 to 1922, reports have been forthcoming from almost every medical school in Europe and America. Numerous workers have published a very large number of papers, nearly all of which are the result of investigations upon a small number of cases. These reports make happy reading, many are eulogistic in the highest degree, and some would almost have us believe that the work of Ehrlich is already a "back number." In other words, it seems to me, that bismuth is no exception to the general rule—a new remedy—a new enthusiasm.

On one point the majority of these observers agree: that the spiro-nematicidal effect is slow. The most diverse claims have been made as to the effects of bismuth on the Wassermann reaction. With few exceptions, bismuth has been acclaimed a powerful influence in keeping negative early sero-negative cases (an effect which, as you are well aware, is by no means always obtained by intensive courses of neosalvarsan), and in rendering negative both early and late serum-positive cases, and in keeping such cases negative.

No great importance need be attached to this last claim, viz., the permanence of the negative reaction, since none of these cases can have been followed up for any great length of time.

Excellent results have been reported in the resolution of the inoculation lesions and of the skin and mucous membrane lesions of the early and late manifest stages. There appears to be no definite evidence as to the effect of bismuth on the cytology and Wassermann reactions of the cerebro-spinal fluid, and I regret that I am unable to throw any light upon this problem. I am not convinced that we are justified in withholding intensive arsenical therapy in these cases, especially in early cerebro-spinal involvement, and in treating such a serious complication with a drug which has not yet established itself in this respect. There are meagre reports of "benefit" and "relief" in cases of tabes and general paralysis. A perusal of the literature does not convince me that the results obtained in neuro-syphilis are superior to or equal to those obtained by salvarsan, or even by mercury and the iodides.

This is a very brief and, I hope, a fair *résumé* of the state of affairs when we commenced our investigations at Haslar.

Surgeon Lieut.-Commander P. W. Carruthers was associated with me until October last year, and more recently Surgeon Lieut.-Commander J. B. Crawford. Both these officers helped me ungrudgingly, and the work of which I am to speak to-night is as much theirs as mine.

Treatment was commenced in March, 1923, sodium and potassium tartro-bismuthate being used at first. Owing to the unsatisfactory results obtained with these preparations, metallic bismuth cream has been used since April, 1923. This preparation contains 0.15 gm. metallic bismuth in each cubic centimetre; the composition of the cream being otherwise the same as Lambkin's mercurial cream, viz., a creo-camph. and paraffin base of suitable melting point (37° C.).

*Disadvantages of the Insoluble Tartro-bismuthate Salts.*

- (1) The injections were painful.
- (2) There was a considerable variation in the amount of the preparation absorbed. On one occasion the first dose (1 c.c.) injected fourteen days previously into the right buttock, oozed out of the needle which had been plunged into the same buttock before injecting the third dose. No absorption had taken place in fourteen days.
- (3) The spiro-nematicidal effect was very feeble. *S. pallida* persisted or returned after a temporary disappearance.
- (4) Not once did a commencing sero-negative Wassermann case remain negative. All such cases became positive during treatment.
- (5) The effect on manifest lesions was certainly greater than that of mercury, but inferior to that of metallic cream, and greatly inferior to that of arsenical preparations.

Fifty-seven men have been treated with metallic bismuth; 567 injections were given.

*Technique of Injections.*

These are given into the buttock in precisely the same manner as mercurial injections—once a week (1 c.c.), a total of 1.2 gm. metallic bismuth being administered in eight weekly injections. Strict asepsis must be maintained. The following are the details of the technique:—

- (1) The patient stands in a good light.
- (2) The skin over the site of the injection is cleansed by rubbing vigorously with a swab soaked in ether or lysol.
- (3) The needle must be inspected to make sure that its lumen is patent and that its eye is free from cotton threads, &c. It is then plunged deeply into the upper and outer part of the buttock and must be left *in situ* at least thirty seconds, *to make sure that it is not in the lumen of a blood-vessel*. This is a vital point in the technique, since metallic bismuth is extremely toxic if injected into the circulation. Should blood exude from or well up in the needle, the needle must be withdrawn, and a fresh one plunged into a new site.
- (4) During the short delay of thirty seconds or so, referred to above, the syringe can be filled with the cream. In cold weather it has been found necessary to stand the jar of cream in hot water, as the melting point is approximately that of the body temperature, and it is necessary to stir vigorously before each filling of the syringe. When it is filled, attach the syringe to the needle already in the buttock and inject slowly.
- (5) Withdrawal of the needle should be effected in such a way as to block off its path, to obviate tracking back of the cream to the skin. This is best accomplished by pinching the skin beyond the point of the needle as it is withdrawn.

In actual practice it has been found that this step is not so important as it was with mercurial injections, as the bismuth cream is not irritating to the skin and subcutaneous tissues.

- (6) Immediately after injection the buttock is massaged vigorously and the

patients elect to go for a brisk walk to work off the slight feeling of stiffness, which invariably resolved in less than an hour. The injections were painless, and in no case has a patient complained of discomfort after walking off the stiffness, nor have any refused treatment.

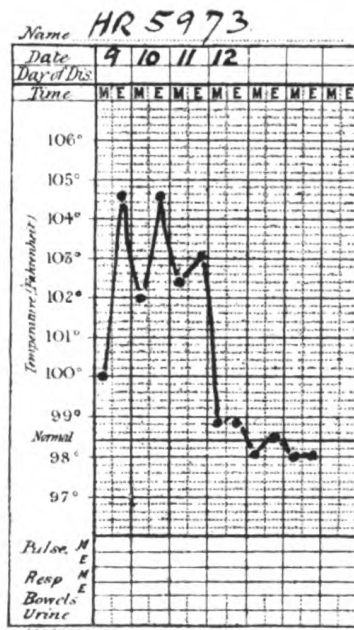
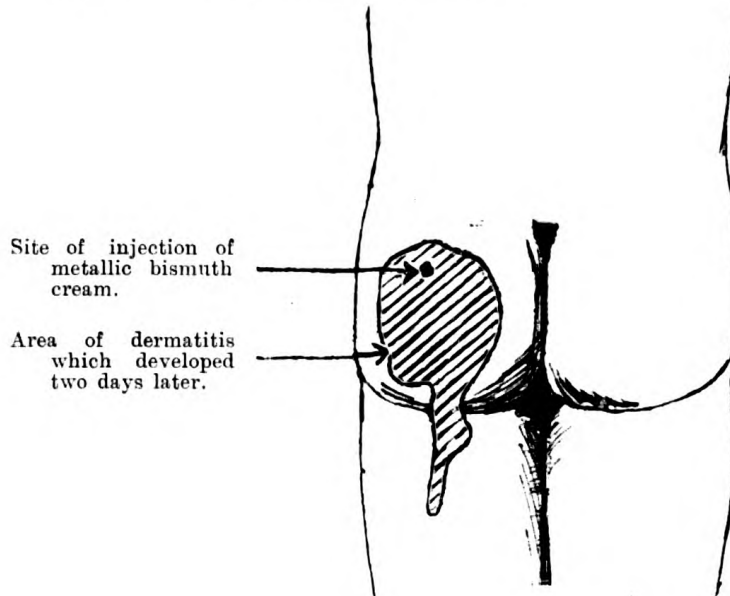


Diagram and Chart illustrating the toxic effects produced in a patient treated with metallic bismuth cream.

No complications other than syncope during injections have been experienced. Syncope is no more common than under any other minor surgical procedure. Two cases (in 567 injections) occurred.

*Toxic Effects.*

Provided the cream be not injected into the general circulation, serious toxic effects are not experienced. Minor toxic effects noted were tenderness and œdema of the gums, usually associated with a black line on the anterior and sometimes on the posterior surfaces of the gums around the upper and lower incisors. Such a black line without symptoms is of no significance.

Serious toxic manifestations occurred in one case only. This patient, admitted on March 15, 1923, was a sero-negative primary case (*S. pallida* was positive). He was given tartro-bismuthate as follows:—

March 16, 1 c.c.; March 18, 1.5 c.c.; March 22, 2 c.c.; March 26, 2 c.c.

*S. pallida* was still present on March 21, but absent on March 26.

On April 7, 1 c.c. of metallic bismuth cream was injected. On the morning of April 9 his temperature was 100° F. The gums were œdematous and septic and an area of œdematous dermatitis, of the shape and size shown in the accompanying diagram, developed round the site of injection. The pyrexia lasted three days (see chart), and during that time the patient was extremely ill.

*The symptoms and signs were:* Pain in the throat and submaxillary glands (both glands were greatly enlarged). There was a well marked dark bluish-black line on the anterior and posterior aspects of the gums around the upper and lower incisors. The fauces were congested. Vomiting occurred twice on April 9.

Next day the edges of the œdematous patch on the left buttock were less marked. The temperature rose to 104.6° F. that evening. The gums and submaxillary glands remained as before.

On the eleventh day the gums and glands were resolving, the buttock was less swollen, the local patch of dermatitis had faded and the temperature began to fall. Except for a crop of labial herpes, no further signs occurred and the patient made a complete recovery, being discharged to duty on April 20.

There was no evidence at the time of injection that the drug had been forced into a vessel.

*Effect on Spirochetes.*

Five cases of early syphilis were treated; all were "A" type cases (*S. pallida* positive—Wassermann negative). Treatment was abandoned after five injections in two cases, and after three injections in three cases, novarsenobillon being substituted.

From these cases it became evident that the spirocheticidal effect is feeble compared with that of arsenical drugs given intramuscularly. In one case the *S. pallida* was found twenty-four hours after the first injection, but were absent forty-eight hours afterwards. Twenty-four hours after a second dose, given seven days later, this organism was however again present. In the remaining cases *S. pallida* was found daily for an average period of one week. The spirochætes were not demonstrated later than one week.

*Effect on the Wassermann Reaction in the Serum.*

All the five negative-serum cases became positive during treatment. (One case developed "secondaries" whilst under treatment.) Further treatment by bismuth alone was not considered justifiable in early syphilis, and was abandoned.

On account, however, of its beneficial effect upon the manifest lesions of syphilis, I decided to confine its use to: (1) Old cases which had nearly all been treated with several courses of novarsenobillon and had persistent positive

Wassermann reactions; (2) cases intolerant of arsenical drugs, as proved by repeated toxic reactions which rendered further arsenical treatment unsafe; (3) cases unsuitable for arsenical treatment; (4) cases with jaundice, due to arsenical preparations.

(1) Twenty-nine cases of latent syphilis with positive Wassermann reactions were treated. All appeared to be in good health. Five cases had cerebro-spinal involvement. (Three showed excess of lymphocytes only and two had in addition a positive Wassermann reaction in the cerebro-spinal fluid.) In one case only has the Wassermann reaction in the serum become negative (patient had been under arsenical treatment quite recently). In all the others it has remained positive or reverted to positive after one or more weak positive findings. It is hardly necessary for me to remind you that the serological findings in old cases are variable, even without treatment, and that syphilis does occasionally tend to resolve spontaneously without treatment.

I think we are justified, as far as *any* conclusions can be drawn from an eight to ten months' observation of a few cases, in assuming that no striking effect on the serum Wassermann in latent cases can be expected from bismuth. In judging its effect upon cerebro-spinal lesions, we must not jump at conclusions unless definite facts are established from an observation of a very large series of cases.

(2) *Cases Intolerant of Arsenical Drugs.*—Eleven cases which had exhibited one or more toxic reactions were treated. It is impossible to decide to what extent bismuth was responsible for their after-history, since it is within common knowledge that cases which have received only three or four injections of an arsenical compound may remain negative and free from symptoms for years, or, again, may relapse serologically and clinically.

One early positive case of manifest syphilis received three doses of 0.45 gm. novarsenobillon. The third dose gave rise to a severe toxic reaction (pyrexia for four days, severe pains in the head and chest, erythema, colic, melæna, diarrhoea, and suffusion of the right conjunctiva). This dose was given on June 1, 1923, the Wassermann reaction having been found positive on May 24, 1923. Bismuth treatment commenced on June 19, 1923. The Wassermann reaction on August 21, 1923, was negative, this being probably due to the novarsenobillon.

No conclusions as to the effect of bismuth on the Wassermann reaction can be formed from these eleven cases. All of them tolerated bismuth well.

(3) One case of aortitis, in which the patient had been very distressed by small doses of novarsenobillon, tolerated and derived some benefit from bismuth. The positive Wassermann reaction was unaffected. A trace of bile pigment was detected in the urine during the short novarsenobillon course.

(4) Three cases of jaundice due to novarsenobillon were treated. The Van den Bergh test showed indirect biphasic reactions. The bilirubin was not estimated in one case; there were eight and four units of bilirubin respectively in the other two cases. All tolerated bismuth well, but in none was the positive Wassermann reaction altered.

Two other cases of jaundice, complicated by manifest lesions, will be referred to later.

#### *Effects upon the Manifest Lesions.*

There is a definite superiority over mercury in the effect of bismuth upon early and late manifest lesions. A striking example of this is demonstrated by the following cases:—

(a) An aircraftsman was admitted on February 25, 1923, suffering from florid manifest syphilis. He was deeply jaundiced. He was originally infected in



December, 1921, and received four courses of novarsenobillon, the last being in November, 1922. His Wassermann reaction had never been found negative; on March 1, 1923, it was positive.

Examination showed moist papules in coronal sulcus and in the fossa navicularis; mucous patches of buccal mucous membrane and of tongue; general adenitis; papulo-squamous syphilide. The Van den Bergh test gave a biphasic reaction and showed four units of bilirubin. Bile salts and pigments were present in the urine. The liver was definitely enlarged and tender. Mercury was given orally up to March 20. The florid lesions of syphilis were still florid and a macular syphilide developed between the papular lesions. Bismuth injections were begun on March 20. By April 5 the patient was discharged from hospital, all lesions and the jaundice having completely resolved. The jaundice was treated by intravenous injections of colossal iodine. He attended weekly for further treatment up to June 24, receiving in all twelve injections (three of tartro-bismuthate and nine of metallic bismuth cream). On September 5 he commenced a second course. He was then the picture of health, and had gained 14 lb. in weight. The liver was not enlarged. Wassermann, June 6 and September 5, 1923, positive. After receiving six more injections he was drafted abroad.

(b) Another case was that of a chief petty officer (now pensioned) who contracted syphilis in 1894. For the last three years he had been treated for dermatitis of both hands. He was sent to me on May 1. He had a serpiginous squamous syphiloderma of the dorsa of both hands. Eight weekly injections of bismuth cream were given. Sixteen days after the first injection the condition had completely resolved. Wassermann positive before and after treatment, but became weakly positive during the course.

(c) A patient who had syphilis in 1916 presented a serpiginous squamous syphilide of the scrotum, which resolved in ten days. Five months from the commencement of treatment the Wassermann was weakly positive, four previous tests since 1916 having been positive.

(d) A case of syphilis of twenty years' duration with positive Wassermann and leukoplakia at the right angle of the mouth, showed complete resolution of this lesion when the patient presented himself for the second course of bismuth. Seven months from the date of the first injection the Wassermann was still positive. One further case is of special interest as it demonstrates the failure of mercury clinically and the success of bismuth.

He was infected in October, 1921, but did not originally come under treatment until January, 1922, when he was in the florid manifest stage of skin lesions. The course of N.A.B. (4.2 grm. in twenty-eight days) was followed by another (2.7 grm.) in June, 1922; the Wassermann was still positive at the end of the second course. On presenting himself three months later for the third course, viz., in September, 1922, he was found to be intensely jaundiced (12 units of bilirubin). Mercury treatment was advised. From October, 1922, to January, 1923, and again from February to May, 1923, he was under continuous mercury treatment in the ship. On June 5, 1923, he was sent to Haslar with florid manifest syphilis of skin and mucous membranes. There was marked general glandular enlargement. Wassermann positive. Metallic bismuth injections were commenced on June 6. The mucous membranes were clear in a fortnight, and on discharge from hospital there was a very faint trace of the skin lesions. A second course of bismuth was completed on December 12, 1923. The Wassermann up to November, 1923, was positive on six occasions. On December 6, 1923, it was weakly positive.

There can be no question, therefore, that bismuth does exercise a very powerful influence upon the manifest lesions, early and late. It exercises, I think, an infinitely greater effect than that produced by mercury.

#### CONCLUSIONS.

Metallic bismuth cream cannot be considered comparable to the neo-salvarsan-type drugs. It would appear to occupy a half-way position between mercury and neo-salvarsan, and although inferior in every respect to Ehrlich's preparations, it is decidedly of greater clinical power than mercury.

Compared with mercury, it is : (1) Far less toxic and pleasanter to take. (2) Of greater clinical therapeutic value. (3) Of slightly greater spiro-nematicidal power. (4) Equally ineffective in influencing a positive Wassermann, and in preventing early sero-negative cases from becoming positive.

Bismuth appears to offer a better outlook than mercury in cases which are intolerant of arsenic. It could be given in cases unsuitable for the arsenical preparations, and liable to tolerate them badly, viz. : cases of gross visceral syphilis, aortitis, arsenical jaundice, &c. In certain cases of this type it should prove of value as prodromal treatment, to precede cautious dosage with arsenical drugs.

Lastly, those who favour adjuvant treatment (viz. : mercury plus neo-salvarsan) should find a, clinically, more powerful and less toxic ally in metallic bismuth.

#### DISCUSSION.

Surgeon-Commander S. F. DUDLEY pointed out how difficult it was to compare the relative efficiency of the different agents used to treat syphilis, when it was not possible to follow large numbers of cases for many years. Especially was this the case as regarded what was probably the most important point of all—the prevention of the late nervous manifestations. He drew attention to the fact that many protozoa could be rendered permanently tolerant of arsenic ; and clinical evidence suggested that after a course of arsenical treatment a similar change sometimes occurred in the *Spirochaeta pallidum*. Levaditi had shown it was much more difficult, if not impossible, to produce bismuth resistant trypanosomes, and in this connexion one of the chief points he made for bismuth was the rarity of clinical relapse. If this was confirmed, and should it be found that arsenic-tolerant organisms were still destroyed by bismuth, a valuable use for bismuth was at once evident—a course of arsenic should be followed by one of bismuth in order to destroy any arsenic-resistant spirochetes that might have survived. Surgeon-Commander Parnell's "old cases" which, in spite of much arsenic treatment, refused to clear up until they subsequently received bismuth constituted strong support of the hypothesis that some relapses were due to arsenic-tolerant spirochetes. He (Surgeon-Commander Dudley) also drew attention to Levaditi's new preparation (bismoxyl), a complex of tissue extracts and bismuth which the French workers claimed to be much superior to metallic bismuth or its salts. This seemed reasonable, if it was true that metals had to form a protein complex in the body before they could function as parasitocides.

For these reasons he (Surgeon-Commander Dudley) considered that if a suitable protein bismuth preparation could be obtained, further researches should be undertaken to compare its action with that of the older bismuth and arsenical drugs before definitely placing bismuth second to arsenic in the treatment of syphilis.

Major FROST said that he was very glad to hear Surgeon-Commander Parnell's paper on the use of bismuth alone in the treatment of syphilis, as in the Army the drug had been used in conjunction with arsenic. Both at the Military Hospital, Rochester Row, and at the Royal Herbert Hospital, Woolwich, this combination had given satisfaction. It was felt that the combination of bismuth and stabilarsin was the most efficient means of treating syphilis at the present day. Cases which were negative to the Wassermann reaction before the beginning of treatment remained negative, and cases which gave a positive reaction became negative in about 80 per cent. of the cases at the end of the first course, and had remained so. A number of bismuth preparations had been tried during the last year in order to find out which was the best for military conditions. As the value of the drug resided, like mercury, in its metal content, it was felt that the choice lay between the suspended, finely divided metal such as the group of which neotropol and bismuthyl were examples, or the oxychloride like bischlorol. The weekly dose of the former was 0.2 grm. and of the oxychloride was 0.16 grm. of the metal, each in 2 c.c. of fluid. The suspended metal was painless if given deeply into the muscles, but it caused pain if it leaked into the subcutaneous tissues. The oxychloride, it was found, could be injected into the deep subcutaneous tissues without pain and X-ray examination showed a steady absorption of the deposit, the oldest of

five injections being a slight shadow, the others shading up to a deep black shadow of the most recent one. In cases of intolerance to arsenic he wished to warn against too early use of bismuth in continuing treatment as this drug also was capable of producing skin lesions. In the treatment of nervous cases of syphilis, especially in the reduction of crisis in tabes, bismuth was, he found, superior to arsenic.

The future position of bismuth would be as an adjuvant to arsenic in the treatment of syphilis, to the exclusion of mercury.

Dr. F. CARMINOW DOBLE said he considered that bismuth came between arsenic and mercury in the treatment of syphilis, and that the patient stood bismuth better than either of the other drugs. He found that if bismuth alone was given the spirochæta was present in the lesions for at least fourteen days, therefore arsenic must be given so as to render the patient non-infectious with the least possible delay. He was of the opinion that these two drugs should be given during the whole course of treatment, unless arsenic was contra-indicated. The makers of several well-known brands had informed him that they would not send the drug out in large containers, but only in ampoules, as the presence of air caused the salt of bismuth to split up, and also it was impossible to be certain that the drug was properly mixed, the heavy metal falling to the bottom of the jar. As regards the brand, he was very favourably impressed with bischlorol, spirillan and bismuthyl.

He gave the drug by deep subcutaneous injections, either in the upper and outer quadrant of the buttock or between the shoulder blades. The injections were painless, and he had given them to boxers and football players just before an important match without any complaints. In the case of jaundice after "914" he had had two patients who gave a strongly positive Wassermann reaction after the jaundice had disappeared; these cases gave a negative Wassermann after two months' treatment with bismuth, although they had twelve and sixteen injections of arsenic respectively without affecting the reaction. A singer who was being treated with arsenic and mercury complained that his voice was being affected by the mercury. When this was stopped he said that the arsenic was responsible. On being treated with bismuth only, he stated that his voice was splendid, but he (Dr. Doble) had not heard a sample of his singing.

Dr. H. SEMON remarked that there was a very serious discrepancy in the results of Surgeon-Commander Parnell and Dr. Sazerac and Dr. Levaditi. It was the duty of all investigators to do their utmost to find an alternative to arsenic medication, for all these compounds were dangerous, even in skilled hands. He mentioned a new antidote—thiosulphate of soda, by intravenous injection, and recommended his hearers to investigate its effects.

Surgeon-Commander PARNELL (in reply) said that he was glad to hear that Major Frost regarded bismuth merely as a substitute for mercury, and as adjuvant treatment to salvarsan. He was not aware that bismuth caused dermatitis, but Major Frost's experience in this respect was a timely warning. He could only assert again that injections of bismuth cream had proved absolutely painless. Perhaps his dosage accounted for the discrepancy between his results and those of Levaditi, quoted by Dr. Semon. At Haslar the course now consisted of two injections of 1 c.c., three of 1.5 c.c., and three of 2 c.c., given at intervals of a week in the case of out-patients, and at intervals of five days in the case of in-patients. He did not consider that a three weeks' interval between injections of salvarsan would obviate toxic reactions. One of the very worst cases of cerebral toxæmia occurred, in his experience, after a second injection administered thirty-four days after the first. He believed that, whatever the intervals, these reactions would occur, and that they could not be foreseen or prevented. Personally, he never exceeded 0.45 grm. as a dose, and he called to mind German statistics which showed that once 0.45 grm. was exceeded, the incidence of toxic effects immediately went up. He would certainly give a trial to the arsenical antidote mentioned by Dr. Semon, viz., thiosulphate of soda.

#### CORRIGENDUM.

In Colonel Henderson's remarks in the discussion on Major Frost's paper, "Present-day Trend of Treatment of Gonorrhœa," *Proceedings*, January, 1921 (War Section), p. 5, line 6 from bottom, for "lubefacient" read "Lubefax."

## War Section.

President—Air Commodore DAVID MUNRO, C.I.E., R.A.F.

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### The Recent Trend of Military Hygiene.<sup>1</sup>

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To the worker in any branch of science there is profit if occasionally he pauses and surveys the backward road over which he and his work have been progressing. The daily path is so full of twists and turns, of corners and blind alleys, that the sense of general direction becomes blunted; local events and current activities distract attention from general principles; and it is only by regarding the present position in comparison with former stages that it becomes possible to appreciate the broad lines and the true direction of the whole advance.

In the subject of military hygiene the present time is particularly suitable for such a retrospect. From 1914 to 1918 not only did our former ideas and methods suffer the very searching test of modern war, but novel problems, unexpected difficulties, unaccustomed tasks were presented almost daily, each and all of them demanding effective solution and performance. That the medical service of the Army came through this test with credit says as much for its adaptability as for its efficiency. Further, sufficient time has now elapsed since the end of the war to permit that sorting out of mixed experiences, and somewhat nebulous ideas, that digestion of raw facts and phenomena, which are essential if they are to be profitable guides for our future efforts. Lastly, there have recently been published as part of the "Official History of the War," two volumes dealing solely, if not very completely, with the "Hygiene of the War." These are amongst the considerations which led me to select "The Recent Trend of Military Hygiene" as a subject for discussion to-night.

There has occurred during the past twenty years a gradual but momentous change, almost a revolution, in our conception of military hygiene, naturally most marked in the past ten years which included the

Great War and its results. These changes are not limited to improvements in organization or in methods such as would result merely from the normal development of scientific knowledge during the period under review; they strike much more deeply and affect even the primary essential aims of the subject. And only to a certain degree, one must confess, have they been the fruits of new discoveries in the domain of hygiene itself; in far greater measure they have been the product of two factors: in the first place that hygiene, more than ever before, has been using and turning to profit the new work of other—and sometimes not apparently allied—sciences, and in the second place that the combatant branches of the army have gained a more adequate appreciation of the close relationship between healthy living and efficiency.

It appears to me that the evolution of present-day military hygiene, the direction in which it has progressed during recent years, and, possibly, the lines on which future developments may be anticipated, will best be realized and most clearly understood if I select certain aspects of the subject and for each of them compare not so much the methods—which may change from day to day or even from hour to hour—but the outlook, the point of view, of yesterday with that of to-day. Before doing so, however, there is one thing I would emphasize, one point I would like to make very clear, and this is that such a comparison is in no sense one between our predecessors and ourselves, there lies no suggestion of criticism or disparagement of those who in their day so well and truly laid the foundations of the existing structure; there lurks, I assure you, no inferential claim that with their knowledge I—or you—would have bettered their results or that given our present opportunities their work would not probably overmatch anything that you—or I—are doing to-day.

The control of infective diseases may be selected for first consideration from amongst the many and varied duties of preventive medicine, as being that aspect of its practice which most strongly appeals to the community at large. In fact to the man in the street the existence of the commoner infective diseases usually represents the only justification which he knows or can understand for the appointment of a health department. And there are several very obvious reasons for this point of view. As a branch of public health work it is essentially spectacular, its success is frequently dramatic, the growth of bacteriological knowledge places it from day to day upon a more secure basis, and, in the words of an American writer, it is “in accordance with a general rule, which governs all mankind, namely, that of doing first the simplest, crudest, and most necessary thing.”

The contents of the annual reports of the Army Medical Department for the ten years before the commencement of the European war are a striking proof of this infectious obsession. Year by year, page after page is filled with records of the incidence of infective disease at home and abroad,

in the vast majority of cases unaccompanied by any attempt at analysis or comment on the reasons for such incidence. There are occasional brilliant exceptions, one of which, the history of investigations into enteric fever in India, furnishes an invaluable picture of a gradually changing point of view in regard to the causation and control of infective diseases in general. There we find early attention devoted largely to places and things as sources of infection, especially to water supplies; gradually it is realized that the danger lies not so much in water which may possibly be contaminated by excreta, but in the excreta themselves, and thus preventive measures are definitely directed towards the known channels by which the disease is being spread rather than to a haphazard process of cleaning up; and the final step is the recognition of the human body as the breeding ground and storehouse of the infective organisms. There naturally follows the development of measures for the better control of the infective individual, increased laboratory facilities for the more rapid diagnosis of early cases, stricter isolation of doubtful cases, provision of depots or centres for the segregation of convalescent cases until proved non-infective by laboratory examinations, and the issue of an order prohibiting men with a history of enteric fever from being employed in cookhouses or on duties involving the handling of food or drinking water. Beneficial though these measures proved, they possessed the cardinal defect that they were directed entirely against the source and route of infection; practically, they ignored the susceptible recipient. It was only when the protection of the not yet infected individual was taken in hand that a satisfactory measure of success was obtained. I refer of course to anti-typhoid inoculation, the effective use of which in India dates from 1906. And it was this combination of measures, mobile laboratories for early and rapid detection of infective persons, segregation and laboratory control of convalescents, and effective vaccine protection of all troops, which produced the novel discovery in the late war that enteric fever can be rendered a negligible factor in the health of an army in the field.

This history of enteric fever clearly shows the evolution of the modern point of view in regard to infective diseases and I suggest to you that the lines of advance have been in two directions: first, in paying attention to the individual, to persons rather than to things or places, as the true source of infection requiring to be controlled or disinfected; secondly, in recognizing the importance of the healthy susceptible as an item in epidemiology, and the necessity for increasing his personal protection by assisting Nature's defences both specifically and generally. Where an infectious disease is being effectively controlled, there you will find these two principles in active operation.

Enteric fever in India, which I have quoted as an example of control in infective diseases, will serve also as an introduction to my next considera-

tion. In 1904 enteric fever represented a fraction over two per cent of all Army hospital admissions in India; even if admissions for what were called "other continued fevers" be added, the total comes to less than five per cent of the total admissions. Far more important from the point of view of military inefficiency were such disabilities as venereal diseases giving 22 per cent of all admissions, diseases of the digestive system furnishing 11½ per cent, minor septic conditions and skin diseases 7 per cent, and injuries 11 per cent. Similarly, amongst troops at home for the ten years from 1904 to 1913, infective diseases were responsible for only nine per cent of all hospital cases, being far exceeded by such other causes as venereal disease 18 per cent, disorders of the digestive system 17 per cent, skin diseases and minor septic conditions 12½ per cent. And it must be remembered that in these figures, striking though they may be, the real comparative unimportance of infective diseases is not truly apparent. The figures represent admissions to hospital and therefore every infectious case is necessarily included, but amongst the other diseases there is no indication of the large numbers—at least as many again—treated in barracks. Yet save for some pious expressions of opinion on the subject of venereal disease, there is little evidence to show that these causes of sick wastage received any preventive attention whatever. In considering military hygiene the textbooks of that period (and official manuals were no exception) usually commenced with one or more chapters upon the more important diseases affecting a military population, from which the unprejudiced reader could only conclude that the soldier was not assailed by any diseases except those of bacterial or protozoal origin. Occasionally there might be found an apologetic lapse into heterodoxy and the brief mention of such conditions as scurvy, alcoholism, or heat stroke. Preventable disease was a synonym for infection. On the whole it must be admitted that some good results were obtained. Amongst troops at home during these ten years, though the incidence of infective diseases shows no diminution whatever when compared with the incidence of other diseases, yet the general sick rates, in accordance with the well-known epidemiological law, decreased year by year in an extremely satisfactory manner.

One gains the impression that it was the stern requirements of the war, the overwhelming necessity to keep every possible man in the fighting line, which bred a new outlook. It became the obvious duty of the medical service to prevent, or at least control, the enormous wastage of man power due to such causes as trench-foot, shell-shock, cardiac affections, dental caries, heat stroke, food deficiencies, nephritis, myalgia, and many other non-infective diseases. And, perhaps most illustrative of all, it was to the medical services that the Army turned in these critical days of April, 1915, counting upon them to evolve defensive measures against the new gas-warfare. I doubt whether it is adequately realized that for two and a half

years (until October, 1917) research on respirator design and the provision of respirators—in fact the whole responsibility for investigations into gas defence in this country as regards the Army—rested upon the hygiene branch of the Army Medical Services. At last the practice of military hygiene had escaped from the strangling restrictions of infection, and, fortunately, this freedom not only survived the war but is still evident to-day. The annual reports on the health of troops from Commands both at home and abroad bear evidence that attention is now being paid to the greater causes of sick wastage, the non-infectious diseases, as well as to the lesser, the infectious group. In some instances there is reported an endeavour to reduce the incidence of accidents and minor injuries, a group which consistently stands very high as a cause of hospital admissions not only in our own but in all armies.

In this sector of the hygienic front the direction of advance is evident. It lies in a far wider conception of what constitutes preventable disease, in a realization that the ordinary infectious diseases furnish only a fraction of the great total of hospital admissions, which represents the sick wastage of the Army, and, therefore, in the inevitable acceptance by the medical service of a greatly increased responsibility for and a much more detailed interest in the everyday activities of the soldier. It has produced the wise medical officer to whom a case of smallpox is less disturbing than half a dozen cases of indigestion due to faulty cooking or messing arrangements in their unit.

It is evident—and it was generally accepted as its true function—that military hygiene in the past concerned itself chiefly with the more obvious, more immediate problems of death and disease and frequently with only a minor group of these, the infections. Concern with the welfare of the healthy man was scarcely recognized, or, if practised at all, was strictly environmental. It was very inadequately realized that the essential aim of hygiene is a double one, not only the prevention and control of disease, but also the maintenance and improvement of health. And these two, though often so regarded, are by no means the same thing; a recent writer has defined the difference as that between a business man who merely escapes bankruptcy and one who makes a fortune. It is a well-established fact that the sick statistics of a unit or a formation are furnished by a very small percentage of the total troops, and until lately little attention was given to the health interests of that much greater number who, owing to their freedom from actual sickness, were counted of hygienic unimportance. So far as I am aware the importance of this point was first specifically emphasized when Colonel Melville, at that time Professor of Hygiene at the Royal Army Medical College, published his book on military hygiene in 1912. In it he writes as follows: “Every medical officer of any experience knows that the sick list is furnished by comparatively few men in any unit;



the great majority never see the inside of a hospital. It is on this great majority, which I should feel inclined to put at a strength equivalent to seventy-five per cent of the whole army, that the eyes of the medical officer should chiefly be fixed ; " and, again : " It is the duty of the military medical officer to see not only that he [the soldier] keeps clear of hospital, but also that he possesses in the highest degree all his physical powers." For the then existing state of affairs in which the medical officer by general consent was expected to concern himself chiefly, if not solely, with disease, several reasons may be advanced : the fixed belief in statistics of actual sickness as the true index of health and the admitted difficulty of giving a statistical representation of changes in physical health short of positive illness ; a natural and justifiable desire to give first attention to the matter of greatest urgency, to the most evident evil, to the condition threatening most harm ; and, above all, a lack of knowledge of how best to care for and operate the human body as a machine. We knew the human body in disease because we had studied it, but the science of maintaining it at a maximum degree of health and efficiency was almost unexplored territory. In fact, though we could drive the human motor-car well enough to avoid collisions or other serious accidents, we were not sufficiently expert to obtain the maximum " miles per gallon."

It is a common observation that the progress of any science is marked by periods of slow accumulation of facts and ideas and periods, when, owing to some new point of view, the accumulated material suddenly fits into place and the whole subject is revolutionized. So it has been in this matter of personal hygiene. For generations the science of physiology has been accumulating theories and facts about the normal human body, which were studied almost entirely in relation to disease ; only recently has there arisen the new point of view that this knowledge can be studied with much more profit in the pursuit of health.

Recent advances and discoveries in physiology have done much to place the practice of health—as distinct from the mere avoidance of disease—upon a definite and scientific basis. There is no lack of instances ; investigations by various observers into the physiology of muscular work, collated into a comprehensive monograph by the late Professor Bainbridge, have given us a sound understanding of physical training and marching. The recent work of Professor Cathcart of Glasgow, assisted by several army medical officers, has furnished us with definite physiological data on which to base recommendations in regard to the load of the soldier, the speed of his march, and the necessary energy value of his ration ; other considerations in the construction of a ration adequate for health have become disciplined factors now that more precise information about vitamins, protein-metabolism, and food values is available ; a fairly complete physiological knowledge of the control of body heat affords a rational basis for advice in regard

to water drinking on the march ; and the whole subject of ventilation has acquired a new exactitude by the application to it of the physiological investigations of Dr. Leonard Hill. Perhaps the occurrence most significant of the change is the publication by the War Office, in the beginning of 1919, of a small pamphlet written for the instruction of the soldier himself and entitled "Elementary Physiology in its Relation to Hygiene." Further evidence of the increasing desire for a constructive health policy is seen in the establishment after the war of the Army Dental Corps, whose duties include not only dental treatment, but also instruction in dental hygiene and the performance of an annual dental inspection. It is also instructive to note that in the United States army the annual medical examination of all officers, formerly limited in purpose—as in our Army to-day—merely to determining the present fitness of the individual for active service, has recently been expanded, even transformed, and the furnishing of data on which to base advice in regard to maintaining the health and increasing the physical efficiency of the person examined, the detection of minor physical defects or early conditions of ill-health, and the recommendation or institution of measures for their correction or prevention are now regarded as the primary objects of these yearly examinations.

It will be seen that in these examples we have evidence not so much of changes in methods as of the development of a totally new responsibility—the study of health with a view to its promotion rather than of disease with a view to its prevention, an awakening consciousness that Hygiene can offer to human life something of immeasurably greater worth than merely not being ill. To a certain extent this policy may be regarded as a logical development of that tendency to which I have already referred in discussing the control of infective diseases, namely to attach increasing importance to the resistance of the not yet infected individual, to devote attention to the soil as much as to the seed of disease, to prefer fireproofing to fire-extinguishing.

The attitude of Hygiene towards external environment displays a similar bias towards health. Sanitary engineering was formerly studied with the sole object of preventing excretal infection, systems of ventilation were designed to avoid the chemical impurities of foul air, analyses of food and water were performed to determine the absence of what was bad to eat or drink, not the presence of what was good, the outside world was eyed askance as being unfriendly and full of lurking dangers. Perhaps in the matter of environment our views have not advanced to as great an extent as in other directions, but sanitary engineering now brings to its work the added considerations of convenience and comfort in life, ventilation has become a problem of providing that physical atmospheric environment most beneficial to the human organism, and foods are examined

as to their content of those principles essential for health. Recently even water supplies have been similarly investigated in regard to the presence of an essential element (iodine). And gradually, bit by bit, are we approaching the conception that external environment is not so much a thing to fear or to fight against, but is more truly something which, properly controlled, holds important possibilities of co-operation and assistance in this matter of healthy living. If the correctness of this conception be admitted, it follows that the sound medical officer can no longer afford to limit his concern only to those factors in environment which may produce disease, but must be prepared to advise upon, and therefore must be in touch with every item in the whole range of the soldier's army life; there results, to repeat a phrase which I have previously used, a much more detailed interest in the everyday activities of the soldier.

In civil public health affairs the evolution of industrial hygiene as an integral branch of preventive medicine has taken place only within recent years, and popular belief, when hailing this newcomer as the newest member of the public health family, appears to have lost sight of the fact that military hygiene (including both the Army and the Navy) is at once the oldest and most extensive example of the application of hygiene to a special industry. The effects upon the soldier's health of his special circumstances and duties have been subjects of study and investigation by military authorities from the early days of history. Ancient military writings contain much advice on such matters as the selection of recruits, marches, exercise and physical training, and even the oldest of such literature enunciates principles of healthy living which have a humiliating resemblance to those we are busy rediscovering to-day. The motive, however, underlying all such work was primarily the avoidance of disease and the maintenance of health, while increased working efficiency was neither sought for nor expected, except in so far as it might result from improved health. Of very recent birth is the conception that the soldier's work and duties can be studied with advantage not only from the point of view of health, but also with the deliberate intention of increasing his efficiency, of eliminating unnecessary expenditure of energy, of discovering the most economical way of using human effort in the performance of a specified duty.

Amongst such investigations may be mentioned certain experimental marches carried out at various times, although the original purpose in every case was to test the value of a ration. There was the experimental march of Parkes in 1875, performed to determine the relative values of coffee, extract of beef, and rum as restoratives, a march made by a company of the West India Regiment at Sierra Leone in 1907 to determine the most suitable scale of rations for the West Indian soldiers on active

service, and two experimental marches, the first in 1909 and the second in 1910, under the direction of Colonel Melville, at that time Professor of Hygiene at the Royal Army Medical College, to test the adequacy of the existing field service ration. Although rations were thus the primary concern, these marches gave opportunity for investigating a number of factors which affect marching efficiency, and the 1910 march, for example, produced several modifications in the existing regulations for infantry training.

The data obtained from these marches were based upon loss or gain of body weight, alterations in body measurements, and the subjective sensations of the victims, and therefore were open to considerable error. Advances in biological laboratory methods furnished the means for observations of much greater precision, and, during the war, when shortage of food made it probable that preferential treatment would have to be given to the army population, especially to young recruits under training, a series of investigations was carried out by Cathcart and Orr. The method employed was that of Douglas and Haldane, the expired air being collected in the Douglas bag during different exercises, and samples analysed by the Haldane apparatus. The immediate object of this inquiry was to ascertain the amount of energy expended by an average recruit during the various parts of his training, from which could be found the necessary energy value of his ration, and therefore it was essentially a health question; but many of the observations were found to have an important bearing on the working efficiency, as distinct from the actual health, of the individual.

It was evident that there existed a wide field for inquiry in regard to the soldier's duties, directed towards determining optimum conditions of work, i.e., the conditions furnishing the greatest economy of physical effort consistent, of course, with health. During the past five years a number of investigations have been undertaken and results of considerable importance have been obtained. Time does not permit a lengthy discussion of these results, but one or two examples will illustrate this recent "energy economy" outlook of military hygiene. There was a valuable historical survey by Lothian dealing with the load carried by the soldier from ancient times to the present day, which furnished an abundance of exact data and enabled us in the first place to judge the question in the light of actual past experience. Cathcart and Lothian, investigating the present pattern of web equipment, showed that minor modifications of certain straps afforded a reduction of as much as seventeen per cent in the actual cost of carrying the load. Cathcart, working with Lothian and Greenwood, determined the effect on energy expenditure of marching at various speeds (the optimum rate proved to be ninety yards per minute), and along with Richardson and Campbell demonstrated that the maximum economic load for a marching man represents forty per cent of the body weight under

favourable laboratory conditions, which should be reduced to thirty-three and one-third per cent for field conditions. Cathcart and Orr, while investigating the energy expenditure of the infantry recruit, showed that considerable latitude might be allowed in the distribution of the load without materially influencing expenditure, but did not question the earlier work of Zuntz and Schomberg, who showed that the cost of carriage of an asymmetrical load, such as a rifle, is considerably greater than that of the same weight symmetrically distributed. Stevenson and Brown (the latter an officer of the Royal Engineers) in 1921 undertook an investigation into economy of effort in trench digging, initiated at the request of the Royal Engineer Board; this piece of work possesses special interest in that it is, perhaps, the example most strikingly indicative of the new association between military hygiene and industrial efficiency, and because it represents, as far as I am aware, the first application of scientific time-and-motion study to military duties. The report has recently been published in the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, and it will be sufficient to note here that there was evolved a new pick and shovel drill giving an increased digging efficiency of about twenty per cent.

I have discussed this aspect of military hygiene at some length, as it represents at once the most recent and the most far-reaching development of the subject. Looking at the last example which I mentioned, namely, economy of physical effort in digging, it would be difficult to imagine an outlook more definitely in conflict with the old narrow-minded conception of Hygiene, as interested solely in diseases—that superficial point of view which but a year or two ago led to the authoritative but somewhat benighted declaration that “public health is now largely applied bacteriology.” Far from it! The effectiveness of sound hygienic measures cannot be measured entirely in terms of disease, because most of its present-day work has to do not with disease but with health. This fact reveals itself, if not in everyday action, at least in everyday language, for there exists a Ministry of Health, not of disease, and we collect *vital*, not *mortal*, statistics. What Hygiene sets out to accomplish in the Army is not merely a reduction in disease, but a positive increase in the sum total both of health and of working efficiency. And our pursuit of the latter implies, as I have emphasized more than once this evening, a much wider and more detailed concern with the whole life and work of the soldier.

The health of a limited community, such as an army, is closely dependent upon the quality of the raw material, and any marked carelessness in the medical examination of men presenting themselves for enlistment will tend to introduce into the Army recruits in whom no subsequent hygienic endeavours can either produce or maintain a satisfactory degree of physical fitness. Hence recruiting in its medical aspect becomes an

important function of military hygiene, and the branch of the medical service responsible for the health of the army community clearly must also control the medical selection of individuals for admission to that community. Further, the recruit is usually a young immature lad, frequently under-fed and ill-developed, and the nature of his early physical training at the regimental depot is a factor of so great importance in determining his future health and fitness that this also must be closely supervised by the medical service. In the Army the necessity for such co-ordination has recently been recognized, and the medical examination of recruits, along with the medical aspects of physical training, is now supervised and controlled by the Director of Hygiene at the War Office. Here also we have a further instance of the manner in which modern military hygiene day by day is being compelled to include wider interests and to undertake far more comprehensive responsibilities.

In regard to the actual medical examination of recruits there has been little change in recent years. There is evident, nevertheless, a distinct leaning towards more precise and definite standards for various conditions, leaving less opportunity for the often erratic personal discretion of a recruiting medical officer at an outstation and tending to a more standardized type of recruit. Thus, for example, there is now a more or less fixed arithmetical definition of dental sufficiency, and the problem of a standard functional cardiac test is at present under consideration. More important, however, because it conforms to the general line of development of the whole subject, is the tendency to examine the recruit not solely as to his freedom from pathological conditions, but also as to his positive fitness for his future work. Thus in the infantry recruit his weight must be considered in relation to the load which he will have to carry on the march, the psychological stress of modern warfare demands consideration of the man's nervous stability, an educational standard is now imposed, and the value of a possible intelligence test is being seriously considered. It is interesting to note a similar tendency in civil industries, in some of which vocational selection tests have obtained a definite footing.

The existence of an organization specially devoted to the control and supervision of health matters in the Army dates back little more than some twenty years to the first appointment of a specialist sanitary officer for each military district at home. It was the experience of the South African War which brought the change into being, when it had become evident that effective sanitary organization and supervision could not be performed by the casual holder of some other appointment; and in the recent great war, few expansions can compare with that of the sanitary service from its 7 officers and 116 other ranks in August, 1914, to a total of 17,000 officers and men in the various theatres of war at the time of the armistice. The

present peace organization was officially authorized in September, 1919, when there were created at the War Office a Directorate of Hygiene, and in Commands at home and abroad the appointments of assistant or deputy-assistant directors of hygiene, the latter replacing the previous specialist sanitary officers. At the same time a corresponding Directorate of Pathology was established and an active service organization for both these branches was promulgated.

In regard to the activities and duties of assistant or deputy-assistant directors of hygiene in Commands, the recent developments, as would naturally be anticipated, merely reflect the various changes in outlook which have already been considered. Originally the activities of the sanitary officer seldom went beyond matters of external environment—barracks, water supply, disposal of refuse—or the investigation of actual outbreaks of disease. He occupied a laboratory in which he did a certain number of chemical analyses and a rapidly increasing amount of bacteriological work, and from which he issued forth on sporadic excursions to combat an insanitary drain or an infectious disease. To-day the actual technical performance of laboratory work is relegated in large part to the specialist in the particular subject, to the sanitary engineer, the bacteriologist, the analytical chemist, the physiologist, and the true laboratory of the present-day sanitary officer lies in the barracks, the camps, the surroundings, the work and duties, in the flesh and blood and human life of the troops whose health and physical efficiency he supervises. More than ever he must concern himself, must keep in the most intimate touch with, every single item of all these diverse activities which constitute the daily life of the man in the ranks.

If this vast extension in the scope of military hygiene is accepted, if it is true that there is nothing in the military heaven and earth which does not to some degree affect the health, the welfare, the working efficiency of the soldier, and therefore nothing which does not become a legitimate concern of the hygienist, it may be argued that hygiene in the Army at once becomes identical with the sum total of the administration of the Army and might logically presume to take the place of the whole existing organization. This would be a justifiable and insuperable criticism if there were claimed not only the duty of advising on all these matters, but in addition the responsibility for their efficient execution. It is, however, an essential principle of our army organization that responsibility for carrying out "all measures necessary for the preservation of the health of those under him" rest definitely upon the unit or formation commander, who is also responsible for the due observance of sanitary orders by all under his command.

The medical services, therefore, can advise and can supervise, but execution means combatant co-operation, and this co-operation must be

not only fervent but based upon knowledge. Education of the whole army community in health matters is essential, and in the practice of hygiene in the Army no greater advance has been made in recent years than has taken place in this respect. Before 1906 general instruction in sanitation was limited to a few desultory lectures organized locally in commands and intended primarily for the instruction of medical rank and file ; but in 1906 the Army School of Sanitation was established at Aldershot for the instruction of regimental officers and men. The importance of sanitary education throughout the Army year by year became more evident and in 1912 the Director-General stated in his annual report, " It is quite probable that the present satisfactory low incidence of disease can only be maintained by increasing attention to the details of hygiene and preventive medicine on the part of regimental officers and non-commissioned officers and indeed the rank and file themselves." During the war sanitary instruction for the regimental officer and man was greatly extended ; in addition to well-organized and fully-equipped schools at home, such as those in London, Leeds and Blackpool, schools and training-centres were established in every expeditionary force and were not limited to the base or the lines of communication, but were found as far forward as army and even corps areas.

This feature presents a definite and unmistakable line of development, making clear this principle that adequate sanitation can never result from the single-handed efforts of any medical service, that it must always depend upon the cordial and the educated co-operation of the rest of the community both as a whole and as individuals. Its aim is the production of what has been called the " personal sanitary conscience." It is only where this exists that even the finest sanitary organization can prove effective, and in the official " History of the War " you will read that " It was in fact the influence of the individual probably more than of the system which was responsible for the maintenance of a high standard of health and the resulting high standard of efficiency amongst British troops. That this influence pervaded the expeditionary forces during the war is in great measure due to the efforts made by the numerous schools of sanitation, as well as to the instruction which had been carried out previous to 1914."

The concerns of military hygiene are so many and so varied that it is not easy to select lines of recent development which shall be characteristic of the subject as a whole. Nevertheless if one examines the evolution of the present-day point of view in each of these provinces which I have mentioned—infections, other diseases, health, environment, working efficiency, recruiting, organization—there become evident some general tendencies common to them all.

There is certainly, in the first place, an increased—and an increasing—appreciation of the importance of the individual, an attention to persons



rather than to things and places, a leaning towards physiology instead of towards sanitary engineering. It is an outlook in which the soldier's body holds equal interest with his barracks, the development of his physique ranks at last on an equality of importance with the disposal of his fæces, and the man himself is considered as an integral part of his environment.

Secondly, there is evidence of a definite orientation towards health rather than towards disease, a persuasion that in the precept "Eschew evil and do good" the latter injunction is the more important, that to improve the health of the many is in every way as vital a hygienic duty as to keep the few out of hospital. Inevitably there follows a logical extension of this principle, so that not only the promotion of health, but, in addition, the improvement of working efficiency becomes a legitimate function of military hygiene. The selection of recruits best adapted for their future duties and the elimination of wasteful methods in the performance of these duties are examples of this policy.

The third development lies in the enormously increased scope of the subject, so that its advisory range, to quote from the Regulations for the Army Medical Services, now includes "any precautionary or remedial measures relating to stations, garrisons, barracks, hospitals, movements, food, transports, encampments, billets, bivouacs, dress, physical training, drills, duties, and" (lest perchance any item whatever in the soldier's life may have been overlooked) "all other matters which may conduce to the preservation of the health of the troops and the mitigation or prevention of disease in the Army." With so great a diversity of important interests it is evident that the specialist in a single particular branch merely as such can no longer claim to dominate the subject as a whole. In fact, the more he knows of his own subject the less useful, except for strictly technical assistance, is he likely to be, as he is not in a position to visualize the situation from any other than his own restricted point of view. No longer does the skilled engineer, the trained bacteriologist, the expert chemist, thereby become at once a well-equipped practitioner of Preventive Medicine.

Lastly, there has been an increasing recognition of the fact that an essential part of hygiene is the education of the whole community; that every individual must co-operate, and that this co-operation must be based upon knowledge. Hence the result that military hygiene includes not only the work of experienced specialists, but also measures for teaching the principles and practice of healthy living to every officer, non-commissioned officer and man throughout the Army.

The various developments which have been considered and the examples which have been mentioned have naturally been limited to the practice of hygiene in the Army, as it is only with that aspect of the subject that I can claim full acquaintance. There is little doubt, however, that the tendencies are universal and may be recognized in every branch of

Preventive Medicine. Even in civil public health, where progress must be slow until a far higher standard of health education permeates the masses, work in industrial hygiene, among school children, and in maternity and child welfare, points to an outlook gradually escaping from the yoke of disease. And I hope that in the subsequent discussion we may hear from representatives of the other fighting services their impressions of the general lines of advance in their respective provinces. There must be developments of interest, for example, in the special problems which face the Navy in dealing with a community occupying a rigid and restricted environment, and the work of the Air Force in the medical examination of flying candidates is a unique example of the "vocational selection" aspect of an industrial hygiene.

I fear that I have been somewhat discursive to-night, and that I must have enlisted from amongst you a good many sympathizers with the essayist who declared that "there is nothing in nature so irksome as general discourses." My object has been to bring to your notice in what manner and to how great an extent military hygiene has been compelled to change its point of view within recent years, and inevitably to take upon itself vastly increased responsibilities. And in these days of rapid change and progress there is profit to be found if occasionally we obey the old counsel "that we make a stand upon the ancient way, and then look about us, and discover, what is the straight, and right way, and so to walk in it."

#### DISCUSSION.

Surgeon-Commander DUDLEY expressed himself in general agreement with Colonel Anderson's views. He thought, however, that bacteriology had still a large amount of valuable aid to give to the practical hygienist, especially in the very direction indicated by Colonel Anderson, namely, by the study of bacteria in relation to the healthy majority, rather than to the minority of the sick. Carrier rates for example might be four- or five-fold greater in non-epidemic than in epidemic times. If it was possible to find out why this was so, doubtless the prevention of epidemic disease would become easier. He (Surgeon-Commander Dudley) endorsed the views expressed on specialism and considered it would be a good thing if no officer was allowed to specialize until he had done at least five years' general duties; otherwise how could the specialist realize what was required of him by his master—the general practitioner? The ignorance of some so-called experts of anything outside their own backyard was almost alarming; and, what was worse still, this type of specialist, fortunately rare, gloried in this ignorance.

Colonel SYLVESTER-BRADLEY said the address Colonel Anderson had given was most illuminating, but he expressed his astonishment at the multitudinous duties which Colonel Anderson claimed as part of the everyday work of the hygiene specialist, and he said he felt that no matter how Colonel Anderson defended the assertion, the present-day military hygienist was usurping the legitimate duties of those who were known in the Service as General Duty Officers.

Colonel Anderson had laid great stress on the part played by the hygienist in taking care of the "fit" as apart from the "unfit": but surely the efficiency of the hygiene branch should be reflected in the statistics of the incidence of disease.

He would call Colonel Anderson's attention to a comparison of pre-war and post-war statistics, showing the average constantly sick ratio and also the invaliding figures for the

same periods. He would draw attention to the figures for one Command only (Aldershot), as this Command was composed of practically all trained soldiers, and the same remarks applied to India.

Colonel Sylvester-Bradley then read the figures in the following Table:—

| Year | ALDERSHOT |                                     |     |                                                    | INDIA |                                     |     |                                                    |
|------|-----------|-------------------------------------|-----|----------------------------------------------------|-------|-------------------------------------|-----|----------------------------------------------------|
|      |           | Constantly sick,<br>Ratio per 1,000 |     | Invalids finally<br>discharged,<br>Ratio per 1,000 |       | Constantly sick,<br>Ratio per 1,000 |     | Invalids finally<br>discharged,<br>Ratio per 1,000 |
| 1912 | ...       | 16.86                               | ... | 8.45                                               | ...   | 28.86                               | ... | 6.68                                               |
| 1913 | ...       | 16.99                               | ... | 8.60                                               | ...   | 29.68                               | ... | 7.49                                               |
| 1921 | ...       | 29.59                               | ... | 43.21                                              | ...   | 52.32                               | ... | 12.76                                              |
| 1922 | ...       | 21.04                               | ... | 31.87                                              | ...   | 31.62                               | ... | 11.87                                              |

He went on to say that in pre-war days the hygienist was a specialist whose work was carried out very largely in the laboratory, and the manifold duties enumerated by Colonel Anderson were the normal duties of the General Duty Officers. Since the war the hygiene branch had usurped these duties from the General Duty Officers, and the average constantly sick ratio had been nearly trebled. He would like to have an explanation of this fact. For his own part, he would be glad to see the specialist return to his "test-tubes" and to his microscope, and the General Duty Officer come into his own again.

Lieutenant-Colonel P. H. HENDERSON said that the address just delivered was one of the most interesting and brilliantly illuminating given before the Section. In thinking over the subject of the paper he was at once struck by the use of the word "hygiene" in the title. Had the address been delivered a few years ago the word used would probably have been sanitation. The importance of nomenclature in the trend of modern hygiene was even to-day not fully realized. Although sanitation was still defined in at least one of their official pamphlets as "the science of preserving health by the prevention of disease," he submitted that that was a wrong definition. Sanitation was the practical application of the science of hygiene. Military hygiene was perhaps best defined as the science of conserving man-power and well-being, and of preserving health by the prevention of disease. The common conception of military hygiene as conveyed to the mind of the average combatant officer, and even to the minds of many medical officers by the term sanitation, was confined to the rather unsavoury subject of the disposal of refuse and sewage, and was summed up in the minds of those officers in the words "drains and latrines." How very different from this was the modern conception of hygiene, an exposition of which they had just heard. No doubt the change in nomenclature had had a considerable influence in widening the outlook and defining the true meaning of military hygiene.

He (Lieutenant-Colonel Henderson) said he had been much struck on first taking over the duties of Assistant Director of Hygiene of a Command, by the very small bearing the ordinary so-called infectious diseases, of which they got a weekly report from the War Office, had on the man-power, and on the loss of actual working days by the troops. On compiling his first annual report he, therefore, endeavoured to show by histograms and statistics the disabilities which were responsible for the greatest loss of efficiency and of working days. He was somewhat surprised to find that minor injuries, chiefly acquired in the gymnasium, riding school and on the football grounds, headed the list. Necessary action was of course taken to reduce this cause of inefficiency, but with his own limited conception of hygiene at that time he added to his report the remark that he did not consider these injuries could be lessened by any hygienic measures. On going to the War Office later, he was interested to read Lieutenant-Colonel Anderson's criticism of this report, and his comment that he entirely disagreed that the measures necessary for reducing the number of injuries could not be regarded as hygienic measures. With his (Lieutenant-Colonel Henderson's) present conception of hygiene he entirely agreed with Lieutenant-Colonel Anderson's criticism of his report, as hygiene must embrace all matters affecting the health and well-being of the troops and their families.

## War Section.

President—Air Commodore DAVID MUNRO, C.I.E., R.A.F.

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### Psychomotor Responses in Relation to Flying.

By Flight-Lieutenant G. H. REID, D.F.C., R.A.F. and  
Flight-Lieutenant H. L. BURTON, M.B., R.A.F., (Medical Service)

IN this paper the term psychomotor response is limited in meaning to the study of the nervous and voluntary muscular processes involved in the adaptation of the human machine to the various forms of stimuli incident to the control of an aeroplane in flight. We do not deal with the accompanying responses of gland and unstriated muscle.

Experience shows that the activities demanded of a pilot call for no supernormal psychical attributes. There are, however, certain aptitudes the possession of which distinguishes those eminently suitable for this work in general and for certain forms of it in particular.

If we consider the varieties of stimuli which are more particularly encountered by a pilot we find the prevailing stimuli are visual in character. Judgments of the position of the machine in space, either in flying or in landing, and appreciation of speed are mainly based on perceptions following visual stimuli arising either from within the machine—i.e., the various instruments and controls—or from without—i.e., the horizon, landscape, or from both.

Particularly is this so in the case of a pupil learning to fly. As experience grows other stimuli are relied upon for the formation of judgments and eventually both visual and other stimuli may be to a large degree reacted to subconsciously.

Auditory stimuli are of less importance to the pilot. The note of the bracing wires and engine may be on occasion the source of information which calls for some form of muscular adaptation. The reception of wireless messages, although important, is subsidiary only to the control of the machine.

Tactile and olfactory stimuli are of still less significance—the feel of wind on one or other side of the face may indicate “side slip” of a machine.

It will be apparent that these stimuli mentioned all arise from the environment. They must be clearly distinguished from the other class of stimuli, which arise within the organism of the pilot himself. This aspect

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of the subject does not appear to have received its due amount of attention from other workers, who have concerned themselves more with the study of the responses to external stimuli.

Internal stimuli arise as the result of change in any organ of the pilot's body. Those, however, which we desire to study more particularly are stimuli arising from the voluntary muscles, joints and tendons, which by their response in the first instance to an external stimulus are *themselves* the source of further stimuli. Upon the processes so initiated depends the harmonious and smooth functioning of the muscular groups by means of which the pilot exercises control of his machine.

Other sources of these internal stimuli are the labyrinths, the function of which is—contrary to a general opinion widely held—rather to furnish information as to the relative position of the head with regard to the trunk than to the position of the head in space. Pressure stimuli arising also from the deeper structures convey information as to the relationship of the pilot's body to the machine with which it is in contact.

The various movements of the voluntary muscles demanded of the pilot need consideration.

In flight an aeroplane may travel in the following directions:—

(1) Forward with rotation about its lateral axis. This change of direction is brought about by a fore and aft movement of the central pillar or "stick" actuating the elevator.

(2) Forward with rotation about its normal, i.e., vertical axis. This movement is brought about by the action of the rudder, controlled by a fore and aft movement of the legs and feet.

(3) In a position of rotation about the longitudinal axis known as "banking": a position maintained during the movement of turning where it is combined with rudder action. The movement is due to an alteration in the wing surface of the machine and is controlled by a lateral movement of the stick activating the ailerons.

By means of these movements separately, or by a combination of two or more simultaneously, the position of the machine may be varied or controlled at will in flight.

At times also, during flight, other movements connected with engine control are called for.

The mechanisms therefore necessary for the actual control of a machine are those which the physiologically healthy individual may be expected to possess. He must be equipped with normal apparatus for the reception of stimuli, a normal skeletal and muscular system, and a nervous system capable of conveying the stimuli to the appropriate muscles. In other words, he is required to be able to perceive certain stimuli and to pay attention to them, to be capable of forming, with reasonable rapidity, sound judgments based on the stimuli received and to be able to perform with precision, speed and consistency, certain muscular movements of the arms and legs.

It is emphasized that no attempt is here being made to gauge the possession of those qualities which are comprised under the term "flying temperament." This depends upon the "mental make up" of the individual and does not appear capable of examination by laboratory tests. The mechanisms involved may be most conveniently regarded from three aspects:—

(1) The receptor apparatus—upon which the stimuli impinge—is the starting point of adaptative changes in the organism, each receptor being attuned to receive certain stimuli, but being refractory to others.

(2) The effector apparatus, the muscles acting upon the bones as levers by means of which the organism communicates movements to the controls of the machine appropriate to the stimuli received.

(3) The transmitting apparatus—the cerebro-spinal system through the paths of which the receptor and effector apparatus are co-ordinated.

An understanding of the mechanism of the psychomotor responses is best based on this concept of the simple reflex arc. All reactions of the organism are built up of a series of such simple reflexes. These reflexes occur both simultaneously in combination, and successively. In addition to their functions of co-ordinating one organ of the body with another, they are themselves co-ordinated one to another. It is to this subject of co-ordination of the reflexes themselves that we have directed our attention.

Literature on the subject of psychomotor responses in their relation to flying shows that in most countries research has been directed very largely to the study of the time taken for the performance of a reflex act of a comparatively simple nature.

In most cases the co-ordination of hand and eye in simple form has been studied by means of an apparatus in which a visual stimulus—commonly taking the form of a coloured light—is presented; to this the subject is instructed to respond as rapidly as possible by pressure or release of an electric key. An electrically controlled clock, which is started by the operator simultaneously with the lighting of the lamp is arrested by the subject's key thereby recording the time taken for the performance of this simple act of co-ordination.

Similar experiments are carried out with auditory and tactile stimuli.

In each case a series of readings is taken and the average time is calculated, as also is the subject's average deviation from his own mean. This latter figure gives a measure of the consistency of his performance. More complicated reactions, so-called "choice reactions" involving a higher degree of psychic, as opposed to muscular, co-ordination are introduced by some workers. In these cases a choice of stimuli in the form of several vari-coloured lights to one of which only the subject is to react may be exhibited, or appropriate keys for each form of stimulus are used.

The form of response is usually manual, but by some experimenters foot movements have also been introduced as part of the effector mechanism implicated.

The actual time taken in the response to stimuli of this kind varies according to the receptor stimulated (auditory or visual) and also upon the direction of the subject's attention. A longer time is taken where this is fixed upon the stimulus he awaits (sensorial type) than in the case where it is fixed upon the muscle movements he is going to execute—muscular type.

The average times found by different workers vary very slightly but are approximately :—

|                                       |     |             | English       |           |
|---------------------------------------|-----|-------------|---------------|-----------|
|                                       |     |             | Muscular      | Sensorial |
| Visual                                | ... | French 19.0 | American 19.7 | ...       |
| Auditory                              | ... | 14.0        | 15.5          | ...       |
| Tactile                               | ... | 14.0        | 14.8          | ...       |
| Times in $\frac{1}{100}$ of a second. |     |             |               |           |
|                                       |     |             | 17.5          | 27.0      |
|                                       |     |             | 12.5          | 22.0      |
|                                       |     |             | 11.0          | 21.0      |

These workers have found a high degree of correlation between the performance of individual pilots under these tests and their aptitude for flying.

From a limited amount of work along similar lines carried out by one of us, such results did not emerge and the figures obtained were inconclusive. It was

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found that on the whole individuals varied but slightly from each other in these times, and large variations were found where least expected, e.g., in cases where the pilot's flying was satisfactory.

In cases of extreme fatigue, psychoneurosis, and head injury, reaction times are often prolonged and performance is erratic. The condition here naturally is fairly apparent clinically and is shown up clearly by the other tests used in the course of routine medical examination as carried out in the Royal Air Force. In this connexion study of reaction times is of little practical value—except that in some cases progress may be estimated by improvement of times and consistency of performance.

The greatest objection to the use of any test of reaction times is that a subject's reactions tend to vary according to his condition at the time—fatigue, excitement and environmental conditions.

It appeared to us that the actual speed of a simple reaction was not the best criterion of flying aptitude. The subject possessing the fastest times may be erratic and may lack general endurance, while the slow type may exhibit consistency and after practice may prove to be in the long run a more reliable pilot. The tests carried out on the lines given do not furnish information on the quickness and precision of the compound effector response, the movement being usually confined merely to the lifting of a finger.

The problem before us was to devise some form of apparatus which, in addition to testing the reaction time of simple movement, would throw light upon the efficiency of the proprioceptive mechanism, for this is largely involved in the co-ordination of the various reflex arcs utilized in the comparatively complicated and delicate movements necessary for the control of aircraft. This corresponds to that quality which is known commonly as "hands."

From the ceptors situated in the muscles themselves there arises a continual stream of centripetal impulses. Any change of muscle tonus, therefore, is communicated through the afferent limb of the reflex arc and the central conducting path to the efferent limb producing appropriate changes of tonus in other muscle groups.

This process takes place wherever various groups of muscles combine to produce simultaneous movements of limbs, which are in turn smoothly succeeded by activity of other groups. The stimulation of the ceptors of this proprioceptive mechanism is secondary to that of the exteroceptive system. For instance, a sound stimulus initiates directly rotation of the head in the direction of the sound. Impulses arise from the muscles so stimulated, which in their turn initiate changes in other often distant muscular groups.

In flying there is no doubt that the external ocular muscles are of the greatest importance in the initiation of stimuli in this system, and it is upon satisfactory co-ordination of the succeeding reflexes that skilful landing and flying depend.

The apparatus described below is evolved from the original Control Indicator invented by Flight-Lieut. Reid for the purpose of night or fog flying. This apparatus furnishes, by the illumination of four groups of electric lights arranged centrally around the speed indicator, coloured red and green, the position of the machine in space—whether flying straight, to port or starboard, and whether banked or on a level keel.

Degree of alteration of the position of the machine in one or more dimensions in space is shown by the number of lamps illuminated in one or other group—movements of the rudder or control pillar acting through the machine extinguish these lights, while two white lights remain illuminated when the

machine is flying straight and level. In the apparatus in use, where the machine itself is stationary, the mechanism has been adapted so that the illumination of the lamps is controlled directly by the position of the control pillar or rudder. There is a movement of the lights concomitant with that of the controls which gives co-ordination of hand and external ocular muscle movement.

The diagrammatic sketch of the arrangement of the reaction time apparatus is shown in fig. 1. The rudder bar carries a metal arm "A," which passes

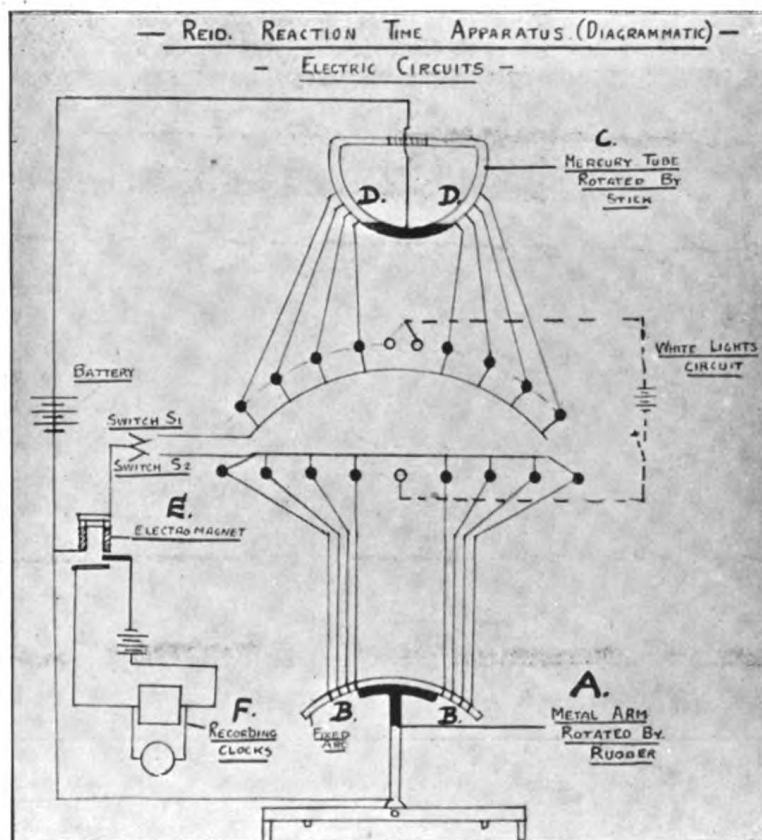


FIG. 1.—Diagrammatic sketch of apparatus.

over a number of contacts "B" according to its angular position, and so illuminates a number of lights, which thus move as the rudder is moved. When the rudder is centred, only the centre white light remains lit.

The control stick rotates an arcuate tube containing mercury "C," so that when the stick is moved this mercury passes over a number of platinum contacts "D" and lights an upper row of lamps according to the amount of the movement, and as the stick is centralized the coloured lights would be extinguished one by one until only the central white light remains.



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A relay "E" operates a d'Arsonval clock "F" and a tape machine, so that when any coloured lamp is lit the timing devices are recording, and are automatically shut off when the controls are centred.

The apparatus is fixed in an aeroplane fuselage, and the switches and timing apparatus mounted on the rear end for convenience of working. Switches S1 and S2 are mounted so that either can be moved for hand or foot movements, or both together for combined movements. Fig. 2 shows the general arrangement of the apparatus.

In the study of psychomotor responses it is important if possible to widen out the range of response so that any clock errors may be relatively unimportant. In the present apparatus this is done by setting the controls very sensitively,

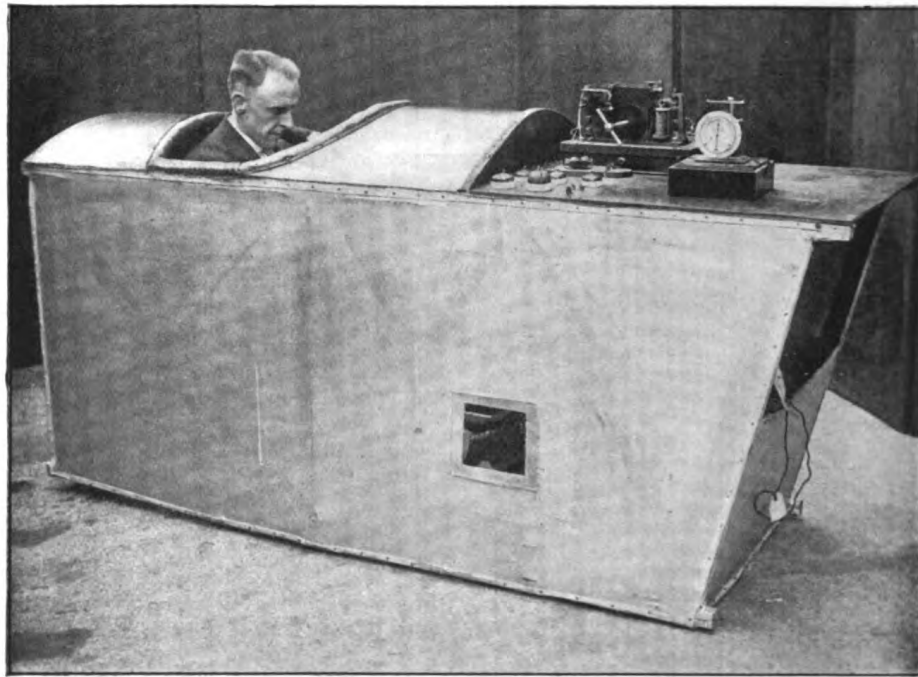


FIG. 2.—General view of apparatus.

and a range is obtained for a combined movement varying from 2'35 sec. to 12'35 sec. for different subjects.

The methods of investigation are as follows :—

The subject is seated in the machine (fig. 2) and the mechanism explained to him. A preliminary series of trial reactions is given, and, when the idea is grasped, a number of tests are given as follows :—

- (1) Hand movement alone to right and left.
- (2) Leg movement of the rudder to right and left.
- (3) Combined movements of the one arm and one leg simultaneously—usually stick to one side with full rudder to the opposite side—a position corresponding to that obtaining in the manoeuvre known as a "spin." In this case both controls must be exactly centralized.

In the experienced pilot the reflex arcs here involved operate simultaneously, in the inexperienced successively. The *effects of distraction* may be observed by sounding a Klaxon horn unexpectedly during a series of observations. The effect is noted, not only upon the actual time of that response, which may be increased, but particularly upon the succeeding responses. A marked increase in reaction time which persists over several tests suggests some instability of nervous control.

An electrically-controlled clock and an automatic time-recording apparatus are automatically started synchronously with the exhibition of the lights to the

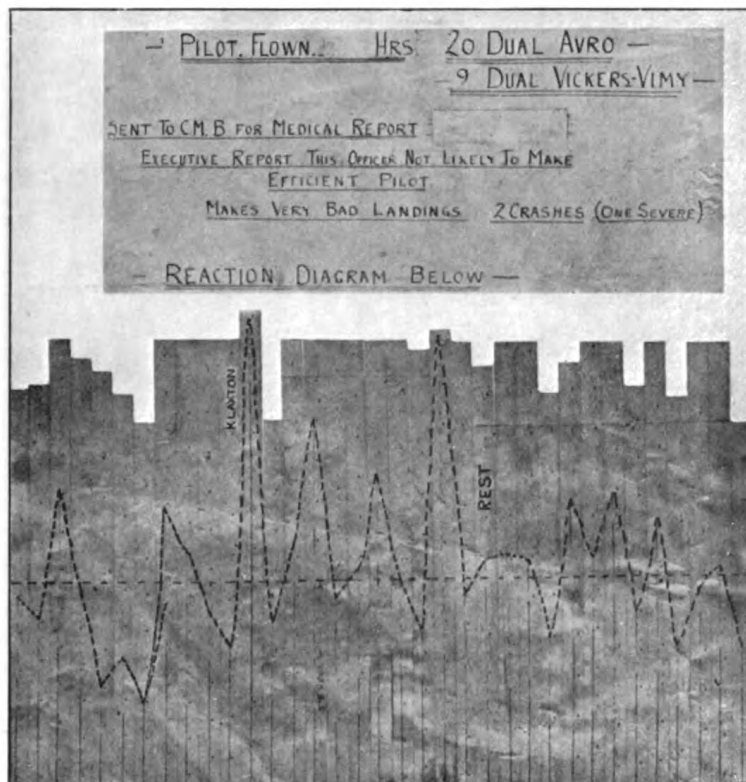


FIG. 3.—Photograph of graph plotted from records obtained in a series of thirty-five reactions of one individual. The results were automatically recorded on paper tape, the strips being arranged in order.

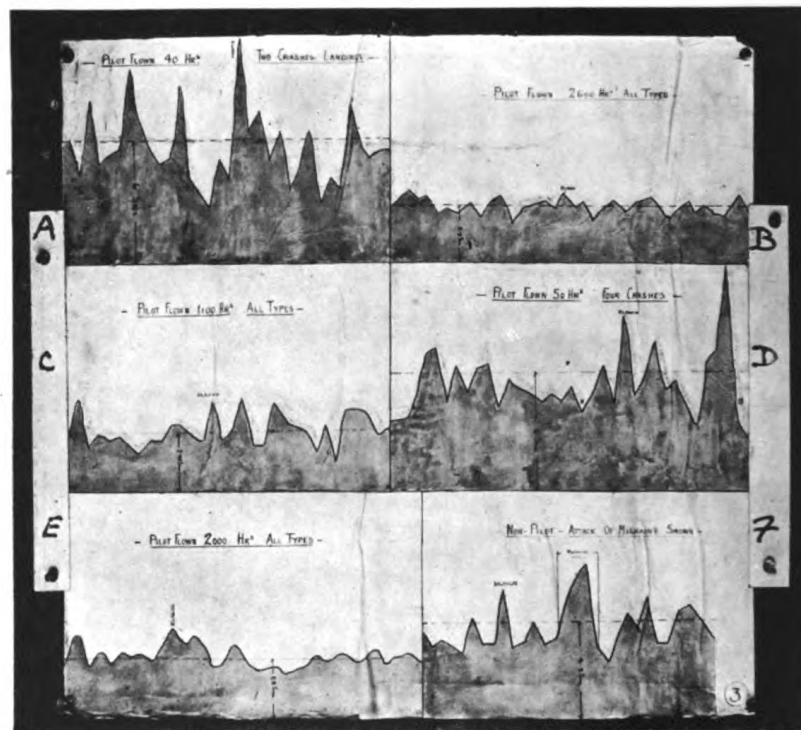
Manœuvre equivalent to recovery from a spin. Graph demonstrates (1) A high average time—shown by horizontal dotted line; (2) Inconsistency of performance—high peaked outline; (3) Increase in reaction time following distraction by Klaxon horn; (4) The effects of short rest.

subject and arrested at the moment he centralizes his controls. The times of a series of observations are recorded and the mean time and variation calculated therefrom.

Graphs have been prepared showing the time-curve of individual performances of hands and feet separately and of the subject's performance of the

combined movement. Also, a graph has been drawn showing the mean time of each subject's performance in each test so that the mean time of any particular case may be compared with that of others.

Results show that the time taken for the centralization of the rudder control is the shortest, that of the stick to be a little longer when each are tested separately. This is due to the greater sensitivity of the stick control in the apparatus, which therefore increases the difficulty of the manoeuvre.



Ordinates equal times in seconds.

FIG. 4.—Examples of six cases.

Six examples of results obtained in different cases in the same manoeuvre—recovery from a spin.

A and D show high average time and inconsistency of pilots who failed to make satisfactory progress, and crashed.

B and E show rapidity of reaction and consistency in experienced pilots.

C shows rapid reaction but erratic results after distraction by Klaxon horn. Subject is an experienced pilot who recently suffered from fracture of skull and cerebral irritation.

F shows type of result of a non-flying subject. The result of an attack of migraine in slowing of reaction time is shown. The subject suffered from partial loss of visual field. It will be noticed that the average time is lower in this case than in the cases A and D, although these subjects have flown 40 and 50 hours respectively.

The right-hand time is usually the shorter except in the case of a left-handed individual.

Results thus obtained bring out the following points:—

(1) The degree of approximation of the time curves of hand and feet taken separately to one another for purposes of comparison.

(2) The degree of approximation of the curve of simultaneously combined hand and foot movement to that of each taken separately.

(3) The actual speed of reactions of each type as shown by the ordinates in the graph.

(4) The consistency of performance which is gauged by the flatness or irregularity of the curve.

(5) Erratic performance is characterized by a multi-peaked curve.

We have endeavoured provisionally to interpret the results we have so far obtained from the examination of fifty-six cases.

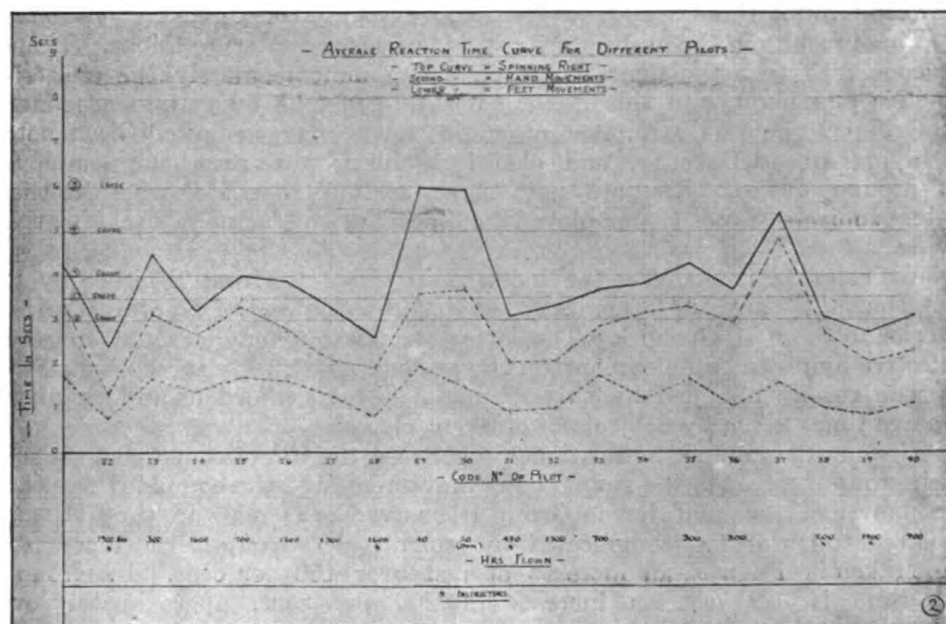


FIG. 5.—Graph showing the average time of a series of subjects in (a) Right spin—upper curve; (b) Hand movement alone—middle curve; (c) Foot movement alone—lower curve.

The ability of the subject may be gauged by the approximation of his result to the lowest of the five horizontal lines, below which fall the results of pilots of recognized ability.

Results falling between lines 1 and 3 are those which are deemed satisfactory.

We have first divided our cases into three groups :—

(1) Those who were at the time of examination engaged on full flying duties.

(2) Those who have received flying training but who are not engaged at present on this form of duty.

(3) Those who have never been instructed in the control of a machine.

Examples of the individual diagrams are shown on figs. 3 and 4.

The curves are also shown in fig. 5 of the average reaction times for the various subjects tested, and this allows of a comparison being made.

The mean times and the mean variations of the reactions in each class for each test are as follows :—

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| Group 1.  |      |     |      | Group 2. |      |     |      | Group 3. |      |     |      |
|-----------|------|-----|------|----------|------|-----|------|----------|------|-----|------|
|           | Time |     | M.V. |          | Time |     | M.V. |          | Time |     | M.V. |
| Hands ... | 2.61 | ... | 0.37 | ...      | 3.51 | ... | 0.45 | ...      | 3.65 | ... | 0.79 |
| Feet ...  | 0.95 | ... | 0.11 | ...      | 1.34 | ... | 0.28 | ...      | 1.7  | ... | 0.32 |
| Spin ..   | 3.28 | ... | 0.37 | ...      | 4.8  | ... | 0.7  | ...      | 5.6  | ... | 0.86 |

Times in seconds.

From these figures it is apparent that Group 1 shows the best results, both as regards time and consistency ; Group 2 shows an increase in time taken in each reaction amounting to 1.52 sec. in the " spin " test ; Group 3 again shows a similar increase in the times of each reaction compared with Group 2 of 0.8 sec. in the spin. There is also a progressive increase in the mean variation between Groups 1 and 3.

These results in themselves are capable of several interpretations. It may be concluded that the figures of each group indicate merely the effects of practice and habit, and this factor certainly must be taken into account.

Group 2 contains a number of subjects who have definitely been noted as flying unsatisfactorily, and also a number who are being employed on ground duties. We are not in a position to state whether these latter subjects were so employed on account of some defect in their flying.

We can explain therefore the increase in times of Group 2 either by the fact that they are out of practice or that their powers of co-ordination are inferior to those of Group 1. The latter assumption may account for their defective flying and employment on the ground.

The results obtained by this grouping at least afford us an idea of the average times taken by subjects of different classes.

If we next examine the relationship between the time taken by subjects of each group to perform the simple hand movement, and the combined hand and foot movement, we find that in Group 1 the average *increase* in time taken is 0.65 sec. to perform a complicated movement. In Group 2 the increase of time taken is 1.4 sec., an increase of just over 100 per cent. In Group 3 the time is 1.9 sec., an increase of 200 per cent. approximately over Group 1.

This figure is the one which we believe to give measure of the efficiency of the powers of co-ordination in which both the exteroceptive and proprioceptive fields are called upon to play their part, particularly that portion of the latter which is connected with the external ocular muscles.

It is when we contrast the results of subjects who have received flying training, who have crashed on one or more occasions, and whose flying has definitely been noted as subnormal, with those which we have obtained from the groups 1 and 2 that the striking figures emerge.

The following is an analysis of five such cases :—

(1) *Case 29.*—Flown forty hours dual and thirty-seven hours solo—crashed twice landing recently. Medically examined on account of inability to land. (*Vide* fig. 3A.)

|                |           |           |
|----------------|-----------|-----------|
| Spin 6 seconds | Hands 3.6 | Feet 1.9  |
| M.V. 2.1       | M.V. 1.4  | M.V. 0.85 |

(2) *Case 30.*—Flown twenty hours dual, no solo—still under instruction, sent home from Egypt.

Examined for defective visual judgment.

|          |           |          |
|----------|-----------|----------|
| Spin 5.9 | Hands 3.7 | Feet 1.4 |
| M.V. 1.9 | M.V. 1.45 | M.V. 0.6 |

(3) *Case 37.*—O.O learning to fly. Done thirty hours, under instruction.

|          |           |          |
|----------|-----------|----------|
| Spin 5.4 | Hands 4.7 | Feet 1.6 |
| M.V. 1.9 | M.V. 1.8  | M.V. 0.7 |

(4) *Case 49.*—Fifty hours flying. Crashed four machines in France. Not flown since. (Vision defective.) (*Vide 4D.*)

|          |           |          |
|----------|-----------|----------|
| Spin 7.0 | Hands 6.1 | Feet 2.1 |
| M.V. 2.2 | M.V. 2.1  | M.V. 1.1 |

(5) *Case 59.*—Done 100 hours flying. One crash during training. Took twenty hours Avro dual and nine hours Vickers Vimy. (*Vide fig. 3.*)

|          |           |           |
|----------|-----------|-----------|
| Spin 5.9 | Hands 5.0 | Feet 1.4  |
| M.V. 1.8 | M.V. 1.3  | M.V. 0.62 |

Note time of hands.

This officer was referred on account of inability to fly. A case of psychoneurosis.

These results should be compared with those obtained from experienced pilots—*vide E* and *B*, fig. 3. Note shortness of time taken and level curve.

We fully recognize that sufficient cases have not yet been examined to warrant arrival at any definite conclusions, and desire to emphasize the fact that the work carried out has been purely experimental. The tests are not used in the selection of candidates, or grading of pilots.

We believe however that:—

(1) Tests on the lines of those we have carried out with this apparatus will, by furnishing a measure of the individual's co-ordinating power, indicate the degree of his ability to fly and land successfully.

(2) Periodic tests during flying training will afford indication as to rate of progress and of acquisition of flying skill. In cases in which an individual's flying is noticed to be unsatisfactory, light may be thrown by these tests on the actual cause of such fault; if not in a positive manner at least by elimination of the fact that he does not possess the power of rapid co-ordination.

In cases where co-ordination is defective, advice on the question of the continuance of training or flying might be given with a resultant economy in life and machines.

(3) The apparatus might actually be used as a means of training pupils to acquire on the ground those particular co-ordinations necessary for aeroplane control.

(4) The suitability of a pilot for any particular type of machine might be gauged from the results of his tests by this apparatus. The fastest times indicating delicacy of adjustment would suggest an aptitude for scout machines.

(5) The apparatus is capable of recording the reaction times of a pilot while actually engaged in flying. The effects of high altitudes and diminished oxygen tension would then be automatically recorded.



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*Note.*—The Occasional Lectures in the Society's *Proceedings* are indicated by the abbreviation *Occ. Lect.* The discussions before the whole Society are indicated by the abbreviation *R.S.M. Disc.* Both these, comprising the General Reports of the Society, are placed first in the bound volumes. The *Proceedings* of the combined Sectional Meetings (Joint Discussions) are bound together and follow the General Reports. The entries referring to the Joint Discussions are indicated by the abbreviated titles of the Sections taking part. The *Proceedings* of the Sections are placed last; these are arranged alphabetically, each Section being separately paged. The *Proceedings* of the Sub-Section of Proctology are included in the Section of Surgery, and are indicated by the abbreviation *Proct.* preceding the numerals.

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